NBER WORKING PAPER SERIES

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

Alan Gerber
Mitchell Hoffman
John Morgan
Collin Raymond

Working Paper 23071
http://www.nber.org/papers/w23071

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
January 2017

We thank Jason Abaluck, Stefano DellaVigna, Laura Derksen, Fred Finan, Sean Gailmard, Don Green, Jennifer Green, Gianmarco Leon, Yusufcan Masatlioglu, Ted Miguel, David Myatt, Matthew Rabin, Gautam Rao, Jesse Shapiro, Richard Thaler, Francesco Trebbi, Rob Van Houweling, and seminar participants at Berkeley (political economy seminar and psychology & economics seminar), CIFAR, Ohlstadt, Oxford, Pitt Behavioral Models of Politics Conference, Princeton, SITE (Experimental Economics), Toronto, Toronto Rotman, and Yale for helpful comments. We are grateful to Dan Biggers for his guidance on the 2014 experiment. David Arnold, Christina Chew, Sandrena Frischer, Hongjia Hu, Faisal Ibrahim, Jeffrey Kong, Will Kuffel, Cara Lew, Elena Litvinova, Melina Mattos, Kevin Rapp, Nick Roth, and Irina Titova provided outstanding research assistance. Financial support from the National Science Foundation, the Haas School of Business, the Center for Equitable Growth, the Burch Center, and the Social Science and Humanities Research Council of Canada is gratefully acknowledged. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

One in a Million: Field Experiments on Perceived Closeness of the Election and Voter Turnout
Alan Gerber, Mitchell Hoffman, John Morgan, and Collin Raymond
NBER Working Paper No. 23071
January 2017
JEL No. D03,D72,H10,P16


## ABSTRACT

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



Alan Gerber
Yale University
Institution for Social and Policy Studies
77 Prospect Street
New Haven, CT 06520
and NBER
alan.gerber@yale.edu

Mitchell Hoffman
Rotman School of Management
University of Toronto
105 St. George Street
Toronto, ON M5S 3E6
CANADA
and NBER
mitchell.hoffman@rotman.utoronto.ca

John Morgan
Haas School, UC, Berkeley
545 Student Services Building, #1900
Berkeley, CA 94720-1900
morgan@haas.berkeley.edu

Collin Raymond
Department of Economics
Amherst College
305 Converse Hall
Amherst, MA 01002
craymond@amherst.edu



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

# 1 Introduction

Why people vote is a central question in political economy. Many workhorse models share the assumption that beliefs about the closeness of an election influence whether an individual turns out to vote.[1] However, owing due to endogeneity issues (described below), this assumption is challenging to test. We test it using 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.

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

[1] This includes not just the private values instrumental model considered by Downs (1957) and Riker and Ordeshook (1968), but also the common values setting of Feddersen and Pesendorfer (1996). The closeness of an election also plays a role also in more recent "ethical voter" models such as Feddersen and Sandroni (2006). Section 2 provides a detailed discussion regarding this comparative static in voting models.

[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.

correlated with the election being close (Cox, 1999, 2015).

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

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

2. Both in terms of margin of victory and the probability of a very close race, voters substantially 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.

3. Most importantly, these changes in beliefs do not translate into behavior as predicted by instrumental voting models (even if individuals misperceived probabilities about closeness). Although many models imply that belief changes translate into changes

in turnout, we find no such connection—voter turnout is statistically independent of beliefs about closeness.

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

Next, we present additional evidence that helps rule out alternative explanations and shows that the results are robust to sub-samples that might seem more conducive for finding impacts of closeness beliefs on turnout. We present (sometimes tight) bounds on the relevance of perceived closeness for the relationship between actual closeness and turnout.

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 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.[3]

While many papers show that there is greater turnout in elections that are close or have

[3]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.

smaller electorates, there are numerous confounds in interpreting these results as supporting that beliefs about closeness affect turnout. One approach 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. It is hard to fully overcome the concern, however, that there could have been greater attempts at mobilization in races with a smaller electorate (or in closer races).[4]

A complementary approach to examine whether closeness affects turnout is to use lab experiments. Though samples are generally small, they offer researchers unparalleled control over the environment and help rule out mobilization responses and other confounds. For example, Levine and Palfrey (2007) find strong evidence of higher turnout in smaller elections and when the election is closer. 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. Agranov et al. (2014) expose lab voters to different polling information regarding the distribution of their induced preferences, showing that voting propensities are higher when the expected margin of victory is lower.[5]

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.[6] In addition, we are aware of very few studies that seek

[4]Bursztyn et al. (2017) examine interactions between polls and newspaper reporting in Switzerland. Observational data are also often used to test predictions of non-instrumental models, e.g., that turnout reflects social incentives (Funk, 2010), habit (Fujiwara et al., 2016), or media influence (DellaVigna and Kaplan, 2007).

[5]Duffy and Tavits (2008) and Agranov et al. (2014) vary whether people are randomly assigned to receive polls, which is ideal for examining if whether polls are present 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 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). See Palfrey (2009) for an overview.

[6]In an earlier study, Ansolabehere and Iyengar (1994) randomly assign polls to around 400 voters. They find that closer polls do not affect turnout (consistent with us), but that they do 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. Kendall et al. (2015) measure and randomly shock voters' subjective beliefs regarding candidate valence and policies (instead of regarding election closeness).

to measure or influence voter beliefs about electoral closeness.[7] 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.[8] Second, our sample size is much larger in both of our experiments (Enos and Fowler (2014) had 936 contacted persons in their data), allowing us substantially more statistical power. Third, we provide evidence from 20 elections instead of 1 election, thereby providing greater external validity. Fourth, we consider how our results relate to a broad range of voting theories.[9]

Section 2 discusses theories of turnout. Section 3 describes the design of our 2010 field experiment, while Section 4 presents results. Section 5 presents our 2014 follow-up experiment. Section 6 discusses alternative explanations and robustness checks. Section 7 concludes.

[7]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.

[8]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.

[9]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 of their messages is that elections in general have the potential to be close (instead of providing information about whether the particular election in question is close). They find no difference between the close appeal compared to other appeals. The results are not directly comparable to ours because they study getting a message vs. nothing, as opposed to getting a close message vs. a not-close message.

# 2 Theoretical Considerations

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

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

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

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

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

minority candidate, then P1 continues to hold.[10]

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 continues to 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.[11]

The leading alternative to instrumental voting models are ethical models. Starting with Riker and Ordeshook (1968), scholars have argued that voters may be motivated to turn out by a sense of duty, thus deriving utility from the act of turnout separate from the consequences of the vote itself. Later work has sharpened 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.[12]

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 as if hypothesis that all others sharing their ideology follow

[10]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.

[11]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.

[12]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.

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., Forthcoming) 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 than if such future interactions are unlikely. Such models are not directly concerned about the relationship between the perceived closeness of the election and turnout.[13]

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 a variety of factors including the distribution of voting costs, the distribution of the benefits of voting, the current beliefs about closeness, and any aggregate uncertainty. Appendix D.6 calibrates a very simple instrumental voting model, and we discuss it later in Section 6.

[13]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.

# 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.[14]

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 over them over PC or WebTV.[15] Members receive various rewards and prizes for participating in surveys. Knowledge Networks collects demographics for all members, and the panel is designed to be roughly nationally representative of US adults along these characteristics (Liebman and Luttmer, 2015).

In choosing our sample of states, we excluded CO, MA, ME, MN, and RI, as these were states where there was a major third party candidate. In addition, we restricted our sample to states (1) where the was a poll within the last 30 days indicating a vote margin between the Democrat and Republican candidates of 6pp or less and (2) where there were two polls that differed between each other by 4pp or more. This left us with 13 states: CA, CT, FL, GA, IL, MD, NH, NY, OH, OR, PA, TX, and WI. In each state selected, we used KnowledgePanel members who were registered voters. From the KnowledgePanel registered voters in these states, we had 5,413 subjects assigned to Close Poll and 5,387 subjects assigned to Not Close 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.** The first survey was sent out to subjects on October 19, 2010, which

[14]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 avoiding conducted 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. Research in political science shows that voters generally view gubernatorial races as the second most important type of race (after president) and as significantly more important than senate races (Atkeson and Partin, 1995).

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

was two weeks before the election. The order for the first survey was as follows (see Appendix Figure C1 for a visual timeline and see Appendix E.1 for screenshots with question wording):

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

2. Subjects answered three political knowledge and interest questions.

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

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

5. We then asked subjects about the chance that they would vote; their chance of voting for the different candidates; and the chance the election would be decided by less than 100 or 1,000 votes.[16] We decided to ask subjects about the event of the election being decided by less than 100 or 1,000 votes instead of the outright event of being decisive, as some political scientists and psychologists we spoke to believed that such questions would be easier for subjects to comprehend.[17] All belief questions were administered without any incentives for accuracy.[18]

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

[16]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.

[17]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.

[18]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 where ask 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, 2016). Especially given the wide range of backgrounds, ages, and education levels in our sample, we suspect that adding financial incentives for accuracy via a quadratic scoring rule would not have reduced elicitation error (and might have even increased it). While most of our variables are binary, for the continuous variable of predicted vote margin, we did not elicit subject's uncertainty (see Kendall et al. (2015) for an example that does), doing this for simplicity and time/financial constraints from the survey company.

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.[19]

We decided to ask the same questions immediately after treatment so as to detect if there was any immediate impact on voting intentions. Given that the time before an election is often filled with significant information in the news media, we wanted to give our treatment the best possible chance of having an impact on voting intentions. The median amount of time on the survey was 4 minutes (25th percentile=3 minutes, 75th percentile=7 minutes).

The survey had a 62% response rate. The rate was 62% both among those assigned to receive the Close Poll treatment (3,348 responses of 5,413 individuals) 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 our analyses restricting to these 6,705 individuals who did the survey, as belief data are only observed for those taking the survey.

**Selection of Polls and Information Treatment.** Poll choices were finalized on October 17, 2010. To select the polls, we identified the poll during the 40 days prior to the start of the experiment (which started October 19) 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*

[19]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 in some manner. We take comfort from the fact that, as we document later, a large share of voters do not update their beliefs at all, suggesting that a large share of voters did not feel any social compulsion to update beliefs.

*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%*[20]

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

**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.

[20]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.

Appendix A.1 discusses how person-level correlations between NBLLN and perceived closeness of an election support that our belief data are sensible.

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

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

The goal of the 2010 experiment is to examine how voter beliefs affect turnout. Thus, the main individuals of interest are people who were assigned to the close poll or not-close poll groups and who responded to the survey. Table 1 shows that across most variables, respondents from the Close Poll group and Not Close Poll group have similar characteristics. There is only one characteristic which differs across the two groups at the 5% level. Specifically, voters in the not-close group had a slightly higher pre-treatment belief that the election would be decided by less than 100 votes (but not for less than 1,000 votes or Predicted Margin). To address this imbalance, we will often control for the pre-treatment belief about less than 100 votes.

Even though we are using an online survey, the sample is broadly diverse both demographically and ideologically. The sample is 61% female, is 53 years old on average, and has a significant share with a master's or PhD degree. Appendix Table C3 gives summary statistics.

# 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 1 occurred in Oregon, where it was 1.03).

One reaction to Figure 2 is that many voters do not have advanced education and may

not fully understand probabilities. To address this, Appendix Figure C2 restricts to the roughly 1,400 voters with Master's or PhD degrees. Even among these well-educated voters, the median perceived probabilities of less than 100 and less than 1,000 votes were 5% and 10%, respectively. Thus, the median belief is small 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.1, 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. However, for the share that do change, 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. In addition, consistent with theory, we see that voters who are less informed update more. We measure how informed voters are using their self-expressed interest in politics (1-5 scale), whether they could correctly identify Nancy Pelosi as the Speaker of the House, and the share of the time they voted in the previous 5 elections. For example, a voter who identifies as having very low interest in politics updates by 4.7pp, whereas a voter with a very high interest in politics updates by only 1.8pp.[21]

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.[22] 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 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%

[21]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.

[22]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.

increase in the believed probability relative to the median (or about 7% relative to the mean). Thus, these seem like sizable impacts on beliefs.

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 variable is highly persistent (such as voting, where some people always vote and others never vote), there are often significant gains in statistical power by controlling for pre-treatments records of the outcome variable.[23]

**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 C8 shows a regression of turnout on demographic characteristics in detail. We focus primarily on column 1 of Table C8, 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.

[23]In Appendix Table C9, we present our main IV results without controlling for past voting history, and obtain the same conclusions (though with less precise standard errors).

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.[24]

Beyond the factors already discussed in the Introduction, the OLS results may be biased for multiple additional reasons.[25] First, measurement error in beliefs could attenuate results toward zero. Second, causation could run in the opposite direction, e.g., people who intend to vote may develop self-serving beliefs, justifying their intention to vote by coming to believe (or reporting) that the election is close. Third, there could be additional unobserved factors affecting both beliefs and turnout.

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

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

[24]The coefficient on margin of victory in column 1 of Appendix Table C8 is $\approx 0.04$, and $\frac{43}{5.04}$ is over 200.

[25]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).

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

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

We can thus rule out that a 5pp decrease in perceived margin of victory or 5pp increase in the perceived probability of a very close election is anywhere near as important as that of other voting predictors like age, education, and income, where we saw relations on the order of 10-40pp. Even though the IV estimates have standard errors that are roughly 10 times larger (or more) than those from OLS, our estimated "zeros" still have a reasonable amount of precision.[26] Section 6.2 returns to the question of precision for our 2010 study.

**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 C7 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

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

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 there are three limitations. First, although we have a large sample size, we are unable to reject that the changes in beliefs could have caused a small change in voting. 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, it is possible that 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.

We did a second large-scale experiment during the 2014 gubernatorial elections that addresses all three of these limitations. 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. Second, we draw on the population of registered US voters, as opposed to online survey-takers. 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, 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

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

[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

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.

The randomization was conducted using voter file records provided by an anonymous vote validation company.[30] We stratified by state, and attempted to stratify by whether someone voted in the general elections of 2008, 2010, and 2012.[31] 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 person to have their name listed on the postcard. As is common in the literature, we consider all people within a household to be treated, and our unit of analysis is a person. Our main results are qualitatively similar if we restrict attention to individuals to whom the postcard is addressed (Appendix Table C21).

Appendix Tables C10 and C11 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 (Appendix Table C2).

**Results.** Table 7 regresses turnout on treatment dummies. We do this (instead of an IV

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.

[30]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).

[31]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.

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

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

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

How tightly estimated is our "zero result" from the close poll treatment compared to the not close poll treatment? Based on the 95% confidence interval in column 4, we can rule out a treatment effect of more than 0.77pp. This is small relative to many (but not all) non-partisan interventions in the turnout literature (Gerber and Green, 2016). For example, Gerber et al. (2008) show that two postcard social pressure treatments increase turnout by 5-8pp. Experiments with one get-out-the-vote mailing 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.[32]

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 C22 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 ($\approx 0.2$ for 2010 vs. $\approx 0.3$ for 2014).

# 6 Robustness: Alternative Explanations and Additional Investigations

## 6.1 Alternative Explanations

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

[32]The 95% CI top-end of 0.77pp is based on comparing a close poll postcard vs. a not close poll postcard. For comparing with the literature, we can also consider the impact of a close poll postcard vs. nothing. Based on column 3, we can rule out that getting a close poll postcard increases turnout by more than 0.69pp relative to nothing, which is less than the 0.75pp literature rough benchmark. The pooling of studies by Gerber and Green (2016) include a significant number of partisan mailer studies, which generally have a negligible effect of turnout (Gerber and Green, 2016)—without these studies, the average effect would be higher than 0.75pp.

to provide the polling information as close to the election as possible, while still providing time logistically for the information to arrive. To deal with subjects forgetting the polls, we sent them a follow-up email reminding them about the polls we showed them. 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.[33] Relative to the not close poll treatment, the close poll treatment had no impact on post-treatment intended probability of voting (see Appendix Table C12). These IV results are even more precise than the IV results on administrative voting, with standard errors roughly one-quarter smaller. For example, in column 6, with the regressor of post-treatment belief about less than 100 votes, the 95% CI is [-1.07, 0.24]. The 0.24 upper limit means we can rule out that a 5pp increase in the perceived probability of a less than 100 vote margin would increase intended turnout probabilities by more than about 1.2pp.

Another way to shed light on this 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 C13).

**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

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

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

That people update closeness beliefs in response to one poll may be surprising to some social scientists who eagerly follow election polls online and may be very familiar with what polls have been taken. However, our results are consistent with evidence in political science that many voters are relatively unsophisticated and uninformed (Carpini and Keeter, 1997). 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 C14 (2010 experiment) and C15 (2014 experiment), our zero result is robust to restricting to individuals who don't always vote.[34]

Given that many people do not update beliefs (and that our experiment involves exogenously "shocking" beliefs), one question is whether our results are robust to restricting to

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

cases where a person updates their beliefs. Appendix Table C16 shows our results are robust to this restriction.

Turning to a different issue, one could 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 C17 shows no impact of beliefs on turnout when restricting to voters who rate themselves as having strong political ideologies.[35]

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 C18 (2010 experiment) and C19 (2014 experiment).

**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 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.1. 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

[35]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 C17) and obtained the same conclusions. Likewise, impacts of turnout might be larger among voters with low interest in government (as such voters might be less likely to know about polls). One could also tell a story that impacts would be larger among those with high interest in government (because these people would care about the polls). In either subpopulation, there is no relation between closeness beliefs and turnout.

over-estimation. In addition, using data on the number of seconds that each subject spent on each question, we document that subjects took significant time to answer the various belief questions, suggesting they took the questions seriously.

**Was the experiment "strong" enough to matter theoretically for turnout?** Though we find strong 1st stage effects of our treatment on beliefs, could it have been that the changes in beliefs would not be large enough to affect turnout theoretically? This question is difficult to answer because it depends on the particular model of voting one is considering (recall that several classes of models predict that perceived closeness should increase turnout), as well as numerous assumptions about key parameters (e.g., distribution of voting costs, distribution of voting benefits, current beliefs about closeness, and any aggregate uncertainty). Nevertheless, Appendix D.6 examines the theoretical consequences of increasing beliefs in the context of a simple instrumental model. We find that the observed 2010 increases in perceived chance of less than 100/1,000 votes would predict sizable changes in turnout (on the order of 5-7pp), which is substantially higher than our reduced form estimates in Appendix Table C22. 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.

## 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 some of our confidence intervals rule out even fairly modest levels of $s$, strengthening our overall claims of precision

for our "null result." See Appendix A.2 for details on the discussion in this subsection.

Let $T$ be voter turnout, $P$ be perceived closeness, and $E$ be other factors that affect turnout such as elite mobilization. Both $P$ and $E$ are functions of actual closeness, $C$. That is, $T = T(P(C), E(C))$. We differentiate turnout with respect to actual closeness:

$$\overbrace{\underbrace{\frac{\partial T}{\partial C}}_{\text{Table } C7}}^{B} = \overbrace{\underbrace{\frac{\partial T}{\partial P}}_{\text{IV coefs}} \cdot \underbrace{\frac{\partial P}{\partial C}}_{\text{Table } C4}}^{sB} + \overbrace{\frac{\partial T}{\partial E}\frac{\partial E}{\partial C}}^{(1-s)B}$$

Consider moving from Election X to Election Y where the margin of victory decreases by 10pp. Appendix Table C7 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 4.7pp, an increase in perceived chance of less than 100 votes by 1.3pp, an increase in perceived chance of less than 1,000 votes by 3.9pp, and an increase in perceived chance of less than 100/1,000 votes by 2.6pp. We multiply these changes by our IV estimates of the turnout impact of such changes, obtained from columns 3, 6, 9, and 12 of Table 6. Then, we divide by $B$ to get $s$.

For the 2014 experiment, we can rule out that the postcard increases turnout by more than 0.77pp. In Tables 3-4, we estimate roughly that our close polls treatment decrease perceived margin by 2.8pp, increased the perceived chance of less than 100 votes by 2.5pp, increased the perceived chance of less than 1,000 votes by 2.3pp, and increased perceived chance of less than 100 or 1,000 votes by 2.4pp. We do not observe beliefs in the 2014 experiment. Instead, in order to derive estimates of $s$ from the 2014 experiment, we assume that the 2014 postcards affect beliefs to the same extent as the 2010 online survey, a quite strong assumption that we discuss in Appendix A.2. Then, by the logic of the two sample IV estimator (Angrist and Krueger, 1992), we can divide our 2014 point estimate by the degree of the 1st stage 2010 treatment effect (just identified case) to obtain the 2014 estimate of how

beliefs affect turnout.[36]

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 rows. A value of $s = 0.13$ implies that 87% of the relationship between actual closeness and turnout is driven by factors other than perceived closeness. Furthermore, if closeness beliefs are measured using perceived chance of a margin of less than 100 votes, the estimates imply that, with 95% confidence, we can rule out $s > 0.26$ for the 2010 study and rule out $s > 0.12$ for the 2014 study.

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 provides some suggestive evidence that our experimental results seem inconsistent with $s$ being large.

## 6.3 Did Information Change Preferences Over Candidates?

One potential concern is that individual's 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.[37]

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 seen in the randomly assigned poll. Our main finding is that we fail to find causal evidence of bandwagon effects with respect to actual voting (Appendix

[36]We calculate two sample IV standard errors using the Delta Method, as in, e.g., Perez-Truglia and Cruces (Forthcoming). Details are in Appendix A.2.

[37]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 Seebauer, 2010; Kartal, 2015). Cason and Mui (2005) find that the participation rates of the majority are higher than the participation rates of the minority.

Table C24). 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 C20). For brevity, these findings are detailed in Appendix A.3.

# 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.

Like all experiments, each of the two experiments has limitations. Even though the 2010 experiment was large, we could not rule out modest-sized effects, 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 a close poll stimuli delivered via a 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 provide strong evidence that believing the election to be close seems unlikely to substantially affect turnout. Of course, as mentioned previously, the size of any predicted relationship varies depending on a variety of unobservable variables (e.g., voting costs, aggregate uncertainty, voting benefits). While we cannot rule out that our treatments have *any effect* (e.g., we cannot reject that the close poll treatment in the second experiment could increase turnout by 0.8pp), our results suggest that considerations about closeness are not a *significant* determinant of turnout in US gubernatorial elections.

Our conclusions are specific to the type of election we study. For example, in much smaller elections, it is possible that closeness considerations may affect turnout. We would

also speculate that beyond the size of the electorate, the context of an election might be important. For example, 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.

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), as well as models based on social norms (e.g., Gerber et al., 2008; DellaVigna et al., Forthcoming). 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 seem to suggest that the observed closeness-turnout relationship may be mostly driven by elite mobilization and other endogenous features of close elections as opposed to believed closeness making individuals directly more likely to vote.

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

# References

**Agranov, Marina, Jacob Goeree, Julian Romero, and Leeat Yariv**, "What Makes Voters Turn Out: The Effects of Polls and Beliefs," 2014. Working paper, CalTech.

**Althaus, Scott L., Peter F. Nardulli, and Daron R. Shaw**, "Candidate Appearances in Presidential Elections, 1972-2000," *Political Communication*, 2002, *19* (1), 49–72.

**Angrist, Joshua D and Alan B Krueger**, "The effect of age at school entry on educational attainment: an application of instrumental variables with moments from two samples," *Journal of the American statistical Association*, 1992, *87* (418), 328–336.

**Ansolabehere, Stephen and Shanto Iyengar**, "Of horseshoes and horse races: Experimental studies of the impact of poll results on electoral behavior," *Political Communication*, 1994, *11* (4), 413–430.

**Ashworth, Scott and Joshua D. Clinton**, "Does advertising exposure affect turnout?," *Quarterly Journal of Political Science*, 2007, *2* (1), 27–41.

**Atkeson, Lonna Rae and Randall W. Partin**, "Economic and referendum voting: A comparison of gubernatorial and senatorial elections," *The American Political Science Review*, 1995, *89* (01), 99–107.

**Banducci, Susan and Chris Hanretty**, "Comparative determinants of horse-race coverage," *European Political Science Review*, 2014, *6* (04), 621–640.

**Battaglini, Marco, Rebecca B Morton, and Thomas R Palfrey**, "The swing voter's curse in the laboratory," *Review of Economic Studies*, 2010, *77* (1), 61–89.

**Benjamin, Daniel J., Don Moore, and Matthew Rabin**, "Misconceptions of Chance: Evidence from an Integrated Experiment," 2013. Working paper, UC Berkeley.

**Blais, Andre and Robert Young**, "Why Do People Vote? An Experiment in Rationality," *Public Choice*, 1999, *99* (1-2), 39–55.

**Bond, Robert M, Christopher J Fariss, Jason J Jones, Adam Kramer, Cameron Marlow, Jaime E Settle, and James H Fowler**, "A 61-million-person experiment in social influence and political mobilization," *Nature*, 2012, *489* (7415), 295–298.

**Bursztyn, Leonardo, Davide Cantoni, Patricia Funk, and Noam Yuchtman**, "Polls, the Press, and Political Participation: The Effects of Anticipated Election Closeness on Voter Turnout," 2017. Work in progress, UC Berkeley.

**Carpini, Michael X Delli and Scott Keeter**, *What Americans know about politics and why it matters*, Yale University Press, 1997.

**Cason, Timothy N. and Vai-Lam Mui**, "Uncertainty and resistance to reform in laboratory participation games," *European Journal of Political Economy*, 2005, *21* (3), 708–737.

**Coate, Stephen and Michael Conlin**, "A Group Rule-Utilitarian Approach to Voter Turnout: Theory and Evidence," *American Economic Review*, 2004, *94* (5), 1476–1504.

**_ , _ , and Andrea Moro**, "The Performance of Pivotal-voter Models in Small-scale Elections: Evidence from Texas Liquor Referenda," *Journal of Public Economics*, 2008, *92* (3-4), 582–596.

**Cox, Gary W**, "Electoral rules and the calculus of mobilization," *Legislative Studies Quarterly*, 1999, pp. 387–419.

_ , "Electoral Rules, Mobilization, and Turnout," *Annual Review of Political Science*, 2015, *18*, 49–68.

_ **and Michael C Munger**, "Closeness, Expenditures, and Turnout in the 1982 US House Elections.," *American Political Science Review*, 1989, *83* (01), 217–231.

_ **, Frances M Rosenbluth, and Michael F Thies**, "Mobilization, social networks, and turnout: Evidence from Japan," *World Politics*, 1998, *50* (03), 447–474.

**Delavande, Adeline and Charles F. Manski**, "Probabilistic Polling And Voting In The 2008 Presidential Election: Evidence From The American Life Panel," *Public Opinion Quarterly*, 2010, pp. 1–27.

**DellaVigna, Stefano and Ethan Kaplan**, "The Fox News Effect: Media Bias and Voting," *Quarterly Journal of Economics*, 2007, *122* (3).

_ **, John List, Ulrike Malmendier, and Gautam Rao**, "Voting to Tell Others," *Review of Economic Studies*, Forthcoming.

**Downs, Anthony**, *An Economic Theory of Democracy*, New York, NY: Harper, 1957.

**Duffy, John and Margit Tavits**, "Beliefs and voting decisions: A test of the pivotal voter model," *American Journal of Political Science*, 2008, *52* (03), 603–618.

**Enos, Ryan D and Anthony Fowler**, "Pivotality and turnout: Evidence from a field experiment in the aftermath of a tied election," *Political Science Research and Methods*, 2014, *2* (2), 309–319.

**Farber, Henry**, "Union Organizing Decisions in a Deteriorating Environment: The Composition of Representation Elections and the Decline in Turnout," *ILR Review*, 2015.

**Feddersen, Timothy and Alvaro Sandroni**, "A Theory of Participation in Elections," *American Economic Review*, 2006, *96* (4), 1271–1282.

__ **and Wolfgang Pesendorfer**, "The Swing Voter's Curse," *AER*, 1996, *86* (3), 408–24.

__ **and** __ , "Voting behavior and information aggregation in elections with private information," *Econometrica*, 1997, pp. 1029–1058.

**Fiorina, Morris P.**, "The Voting Decision: Instrumental and Expressive Aspects," *The Journal of Politics*, 1976, *38* (2), pp. 390–413.

**Fleitas, Daniel W.**, "Bandwagon and underdog effects in minimal-information elections," *American Political Science Review*, 1971, *65* (02), 434–438.

**Foster, Carroll B.**, "The Performance of Rational Voter Models in Recent Presidential Elections," *The American Political Science Review*, 1984, *78* (3), pp. 678–690.

**Fujiwara, Thomas, Kyle Meng, and Tom Vogl**, "Habit Formation in Voting: Evidence from Rainy Elections," *American Economic Journal: Applied Economics*, October 2016, *8* (4), 160–88.

**Funk, Patricia**, "Social Incentives and Voter Turnout: Evidence from the Swiss Mail Ballot System," *Journal of the European Economic Association*, 2010, *8* (5), 1077–1103.

**Gartner, Manfred**, "Endogenous bandwagon and underdog effects in a rational choice model," *Public Choice*, 1976, *25* (01), 83–89.

**Gerber, Alan S. and Donald P. Green**, "The effects of canvassing, telephone calls, and direct mail on voter turnout: A field experiment," *American Political Science Review*, 2000, *94* (03), 653–663.

__ **and** __ , "Field Experiments on Voter Mobilization: An Overview of a Burgeoning Literature," in "Handbook of Field Experiments (forthcoming)," Vol. 1 2016.

__ **,** __ **, and Christopher W. Larimer**, "Social Pressure and Voter Turnout: Evidence from a Large-Scale Field Experiment," *American Political Science Review*, 2008, *102* (01), 33–48.

**Geys, Benny**, "Explaining voter turnout: A review of aggregate-level research," *Electoral Studies*, 2006, *25*, 637–663.

**Gimpel, James G, Karen M Kaufmann, and Shanna Pearson-Merkowitz**, "Battleground states versus blackout states: The behavioral implications of modern presidential campaigns," *Journal of Politics*, 2007, *69* (3), 786–797.

**Großer, Jens and Michael Seebauer**, "Public opinion polls, voter turnout, and welfare: an experimental study," *American Journal of Political Science*, 2010, *54* (03), 700–717.

**Hansen, Stephen, Thomas Palfrey, and Howard Rosenthal**, "The Downsian Model of Electoral Participation: Formal Theory and Empirical Analysis of the Constituency Size Effect," *Public Choice*, 1987, *52*, 15–33.

**Harbaugh, William T**, "If people vote because they like to, then why do so many of them lie?," *Public Choice*, 1996, *89* (1-2), 63–76.

**Hillman, Arye L**, "Expressive behavior in economics and politics," *European Journal of Political Economy*, 2010, *26* (4), 403–418.

**Hoffman, Mitchell and Stephen Burks**, "Worker Overconfidence: Field Evidence, Dynamics, and Implications for Employee Turnover and Returns from Training," 2016. Mimeo, University of Toronto.

**Kahneman, Daniel and Amos Tversky**, "Subjective probability: A judgment of representativeness," *Cognitive Psychology*, 1972, *3* (3), 430 – 454.

**Kamenica, Emir and Louisa Egan Brad**, "Voters, dictators, and peons: Expressive voting and pivotality," *Public Choice*, 2014, *159* (1-2), 159–176.

**Kartal, Melis**, "Laboratory elections with endogenous turnout: Proportional representation versus majoritarian rule," *Experimental Economics*, 2015, *18* (03), 366–384.

**Kendall, Chad, Tommaso Nannicini, and Francesco Trebbi**, "How Do Voters Respond to Information? Evidence from a Randomized Campaign," *American Economic Review*, January 2015, *105* (1), 322–53.

**Ledyard, John O.**, "The Paradox of Voting and Candidate Competition: A General Equilibrium Analysis," Working Paper 224, California Institute of Technology, Division of Humanities and Social Sciences 1981.

**Levine, David K. and Thomas R. Palfrey**, "The Paradox of Voter Participation? A Laboratory Study," *American Political Science Review*, 2007, *101* (01), 143–158.

**Liebman, Jeffrey B. and Erzo F. P. Luttmer**, "Would People Behave Differently If They Better Understood Social Security? Evidence from a Field Experiment," *AEJ Policy*, 2015, *7* (1), 275–99.

**Matsusaka, John G.**, "Election closeness and voter turnout: Evidence from California ballot propositions," *Public Choice*, 1993, *76* (4), 313–334.

__ **and Filip Palda**, "The Downsian Voter Meets the Ecological Fallacy," *Public Choice*, 1993, *77* (4), 855–78.

**McKenzie, David**, "Beyond baseline and follow-up: The case for more T in experiments," *Journal of Development Economics*, 2012, *99* (2), 210–221.

**Morgan, John and Felix Várdy**, "Mixed motives and the optimal size of voting bodies," *Journal of Political Economy*, 2012, *120* (5), 986–1026.

**Mulligan, Casey B and Charles G Hunter**, "The Empirical Frequency of a Pivotal Vote," *Public Choice*, 2003, *116* (1-2), 31–54.

**Palfrey, Thomas R.**, "Laboratory experiments in political economy," *Annual Review of Political Science*, 2009, *12*, 379–388.

__ **and Howard Rosenthal**, "A strategic calculus of voting," *Public Choice*, 1983, *41*, 7–53.

**Perez-Truglia, Ricardo and Guillermo Cruces**, "Partisan Interactions: Evidence from a Field Experiment in the United States," *Journal of Political Economy*, Forthcoming.

**Piketty, Thomas**, "Voting as communicating," *The Review of Economic Studies*, 2000, *67* (1), 169–191.

**Rabin, Matthew and Georg Weizsacker**, "Narrow Bracketing and Dominated Choices," *American Economic Review*, September 2009, *99* (4), 1508–43.

**Razin, Ronny**, "Signaling and election motivations in a voting model with common values and responsive candidates," *Econometrica*, 2003, *71* (4), 1083–1119.

**Riker, William H. and Peter C. Ordeshook**, "A Theory of the Calculus of Voting," *The American Political Science Review*, 1968, *62* (1), 25–42.

**Shachar, Ron and Barry Nalebuff**, "Follow the Leader: Theory and Evidence on Political Participation," *American Economic Review*, 1999, *89* (3), pp. 525–547.

**Shayo, Moses and Alon Harel**, "Non-consequentialist voting," *Journal of Economic Behavior & Organization*, 2012, *81* (1), 299 – 313.

**Simon, Herbert A.**, "Bandwagon and underdog effects and the possibility of elections predictions," *Public Opinion Quarterly*, 1954, *18* (03), 245–253.

**Stock, James H, Jonathan H Wright, and Motohiro Yogo**, "A Survey of Weak Instruments and Weak Identification in Generalized Method of Moments," *Journal of Business & Economic Statistics*, 2002, *20* (4), 518–29.

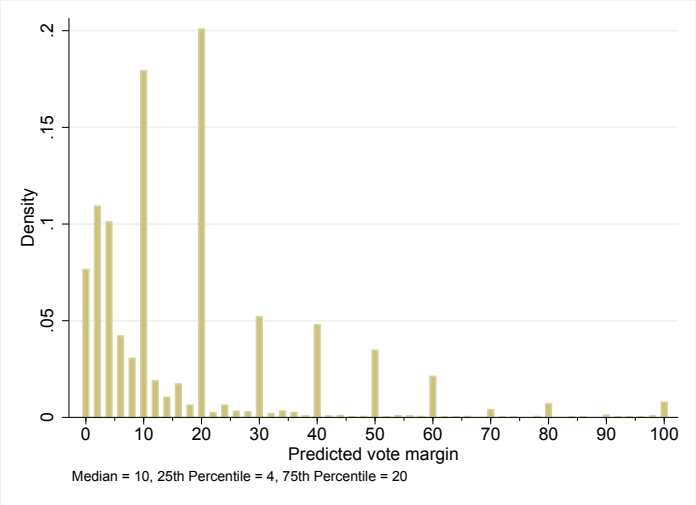
**Trivedi, Neema**, "The effect of identity-based GOTV direct mail appeals on the turnout of Indian Americans," *The Annals of the American Academy of Political and Social Science*, 2005, *601* (1), 115–122.

**Tyran, Jean-Robert**, "Voting when money and morals conflict: An experimental test of expressive voting," *Journal of Public Economics*, 2004, *88* (7-8), 1645–1664.

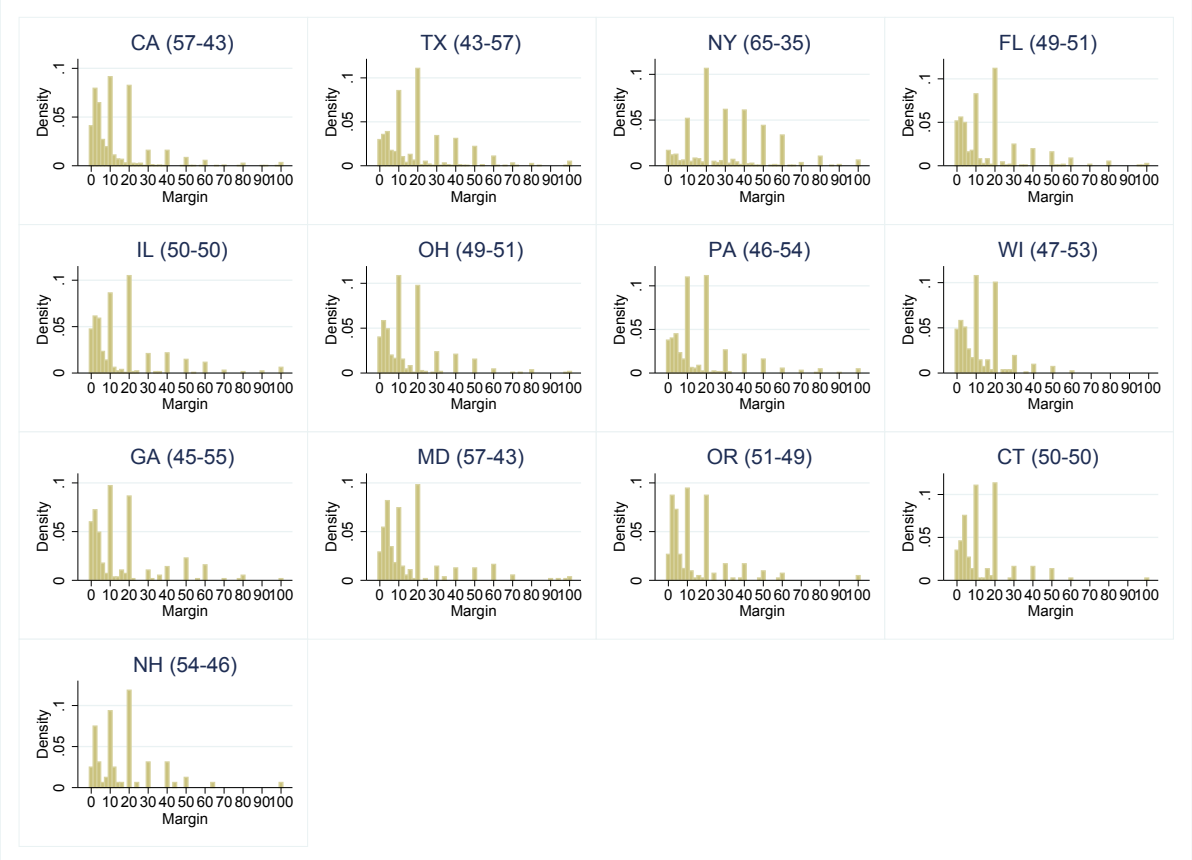
**Wong, Janelle S**, "Mobilizing Asian American voters: A field experiment," *The Annals of the American Academy of Political and Social Science*, 2005, *601* (1), 102–114.

**Zafar, Basit**, "Can subjective expectations data be used in choice models? Evidence on cognitive biases," *Journal of Applied Econometrics*, 04 2011, *26* (3), 520–544.

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



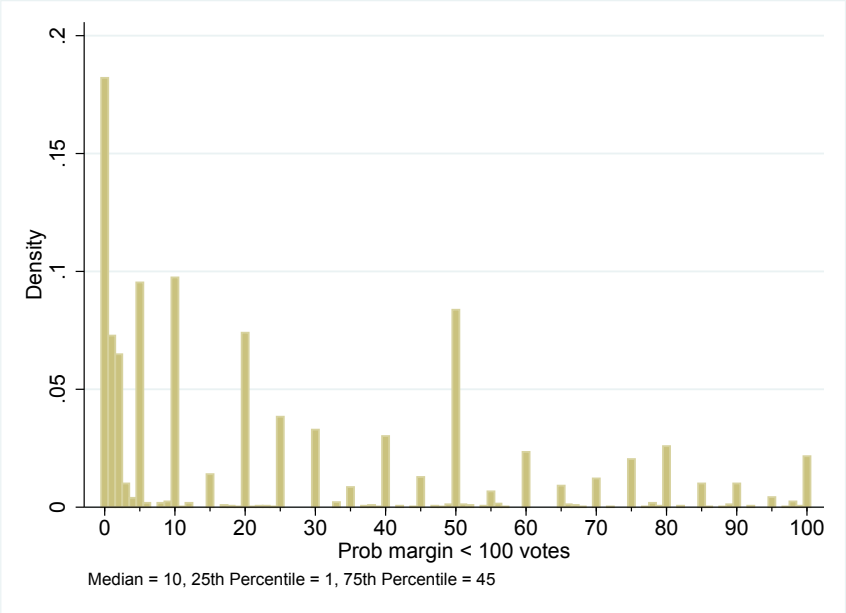

(a) Overall



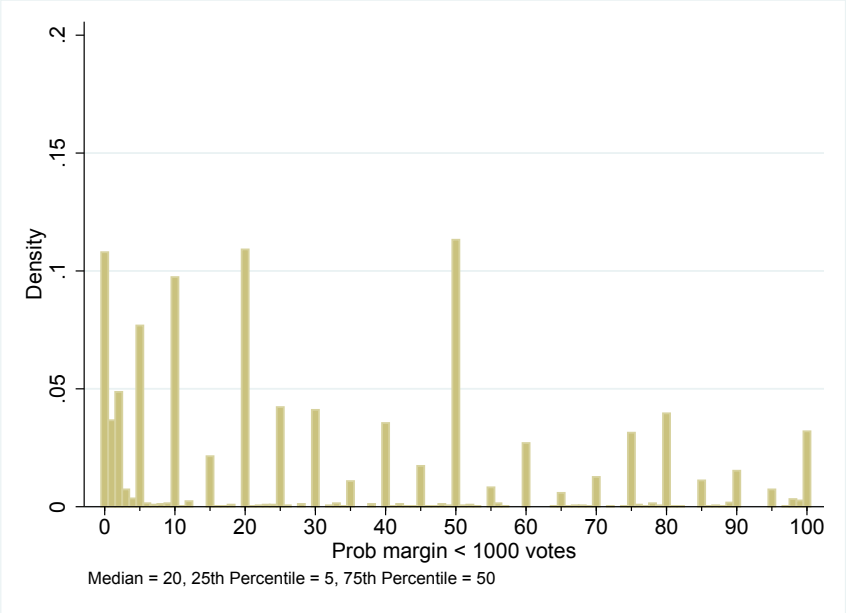

(b) Across States

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)



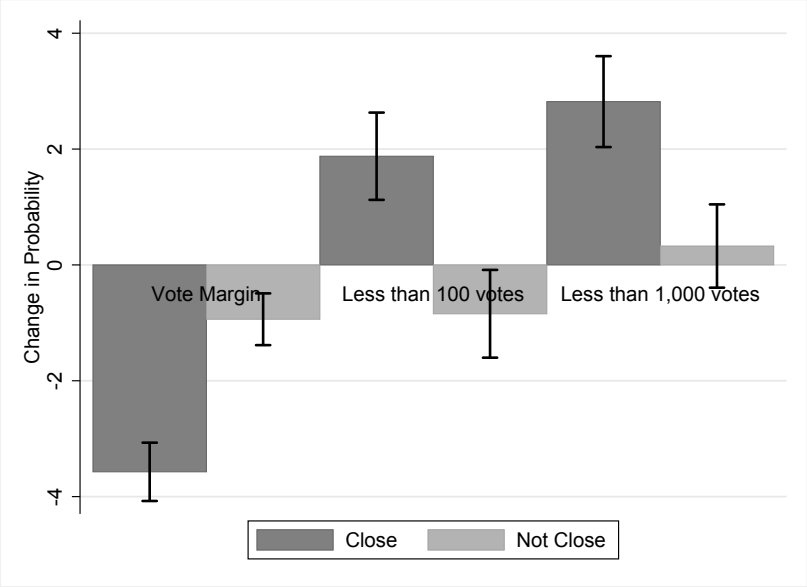

(a) Less than 100 Votes



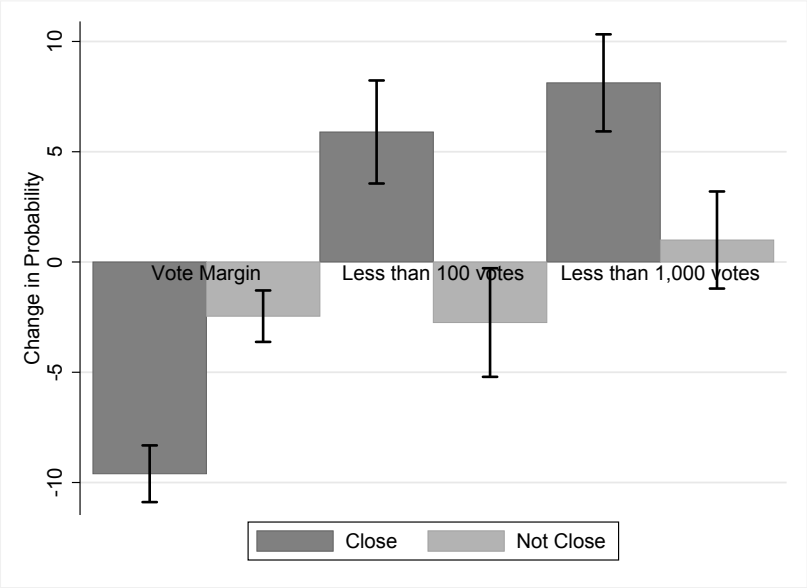

(b) Less than 1,000 Votes

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).

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

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

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

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

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

Notes: This table describes how voters' perception of the vote margin, their perception the election is decided by less than 100 or 1,000 votes, their predicted probability of voting, and their intended probability of voting for the underdog candidate (the candidate behind in the polls) change under the two information treatments (close poll and not close poll).

**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) | $\Delta b$ (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) |
| 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)*** |
| 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) |
| 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). 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) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 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 |
| 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 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. * significant at 10%; ** significant at 5%; *** significant at 1%

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

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Close poll (vs. not close poll) | 0.29 | 0.29 | | 0.29 |
| | (0.25) | (0.25) | | (0.25) |
| Close poll (vs. control) | | | 0.34* | |
| | | | (0.18) | |
| Not close poll (vs. control) | | | 0.05 | |
| | | | (0.18) | |
| Small electorate likely | | | | -0.17 |
| | | | | (0.25) |
| F(Close vs. NotClose) | | | 0.242 | |
| Additional controls | No | Yes | Yes | Yes |
| Observations | 126,126 | 126,126 | 1,385,318 | 126,126 |
| R-squared | 0.38 | 0.39 | 0.39 | 0.39 |

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

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

| Belief variable used: | Δ beliefs from 10pp drop in actual turnout | 95% CI for impact of beliefs on voting | 95% CI for impact of beliefs channel | Point estimate on $s$ | 95% CI for $s$ |
|---|---|---|---|---|---|
| **Panel A: 2010 Experiment** | (1) | (2) | (3) | (4) | (5) |
| Predicted vote margin | -4.7pp | [-0.76, 0.43] | [-2.0, 3.6] | 0.23 | [-0.60, 1.05] |
| Pr(Marg <100 votes) | +1.3pp | [-1.08, 0.69] | [-1.4, 0.9] | -0.07 | [-0.41, 0.26] |
| Pr(Marg <1,000 votes) | +3.9pp | [-0.58, 1.35] | [-2.3, 5.2] | 0.44 | [-0.66, 1.54] |
| <100 or 1,000 votes | +2.6pp | [-0.58, 0.73] | [-1.5, 1.9] | 0.06 | [-0.45, 0.57] |
| **Panel B: 2014 Experiment** | (1) | (2) | (3) | (4) | (5) |
| Predicted vote margin | -4.7pp | [-0.28, 0.07] | [-0.3, 1.3] | 0.14 | [-0.10, 0.39] |
| Pr(Marg <100 votes) | +1.3pp | [-0.08, 0.31] | [-0.1, 0.4] | 0.04 | [-0.03, 0.12] |
| Pr(Marg <1,000 votes) | +3.9pp | [-0.09, 0.34] | [-0.4, 1.3] | 0.14 | [-0.10, 0.39] |
| <100 or 1,000 votes | +2.6pp | [-0.08, 0.32] | [-0.2, 0.9] | 0.09 | [-0.06, 0.25] |

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 both panels, 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 C23. For both panels, column 3 equals the confidence interval in column 2 multiplied by column 1. For both panels, 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 both panels, 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. See Section 6.2 for further information on this exercise, and see Appendix A.2 for further details.