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

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The Online Appendix is organized as follows. Appendix A provides additional discussion related to Sections II-IV. Appendix B gives more details on the data. Appendix C provides additional figures and tables. Appendix D provides documents used in the experiments.

A Additional Discussion

A.1 Discussion on IV Estimates (Section II.C)

One seemingly non-standard feature of Table 4 is that we use the same instrumental variable to instrument different closeness variables one at a time. Our view is that the different closeness variables likely represent related forms or constructs of a person's underlying perception of election closeness. To the extent that the different closeness variables represent different underlying concepts, we show here that any resulting inconsistency in the IV estimates is in the direction away from 0, making the true impact of each closeness variable an even tighter zero than the one we estimate (under the assumption that the different closeness variables do not affect turnout in the unexpected direction, if they have any affect at all).

To see this, consider an IV model of the form in Table 4:

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

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

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

In instrumental voting models, the impact of x_1 is negative (i.e., greater predicted vote margin leads to less turnout) and the impact of x_2 is positive (i.e., greater predicted probability of a very close election leads to more turnout). Above, we've shown that if the instrument affects both x_1 and x_2, and x_2 affects y in the expected direction, then the estimate of x_1

on y is biased downward, i.e., biased upward in magnitude, provided that x_1 affects y in the expected direction. Intuitively, suppose an instrument separately affects two endogenous variables. Then, if one runs an IV regression using one variable at a time, some of the impact of the second variable will be attributed to the first.[1]

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

Last, it is unsurprising that the IV estimates are statistical 0's, given that the reduced form relationship between getting the close poll and turnout is also zero (Appendix Table C29).

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

Our results analyze beliefs in levels instead of logs, as this seemed the simplest way to proceed (particularly for the decomposition in Section IV.B). However, our IV results are robust to analyzing beliefs in logs instead of levels, which we believe is a useful robustness check, given the dispersion in stated beliefs. For example, Appendix Table C21 performs our IV analysis on the subsample of people who update their beliefs after seeing the poll information. In Appendix Table C22, we re-do this analysis but using log(1+beliefs) instead of beliefs in levels. Based on the 95% CIs, decreasing the perceived margin by 10% (0.1 log points) increases turnout by no more than 0.79pp in column 3. In column 12, increasing the perceived chance of a very close election by 10% increases turnout also by no more than 0.79pp. Our main IV results in Table 4 are also robust to beliefs in logs.

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

Further discussion on eliciting beliefs without incentives. As noted by footnote 12 in the main text, we did not use incentives for eliciting beliefs for two reasons: (i) Legal concerns about payments constituting gambling on elections or paying people to vote and (ii) Concerns that a quadratic scoring rule would be confusing for subjects of various ages and educational backgrounds. Still, some readers, particularly those from a lab experimental background, may be concerned about whether we successfully elicited beliefs. For example, the chapter of Laury and Holt (2008) in the *Handbook of Experimental Economics Results* provides examples of some situations where economic behavior by laboratory subjects is different based on whether financial incentives are used. For example, choices in risky gambles over large stakes seem to be affected by whether questions are hypothetical or not. However, Laury and Holt (2008) also acknowledge that there may be lab situations where using incentives may not matter.

We have four responses to this concern. First, our randomized treatment (close or not close polls) provides a natural way of addressing measurement/elicitation error, including potential error from not using incentives. Second, while not standard in lab experiments, eliciting beliefs without incentives is standard practice in most field data (Manski, 2004),

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

including in leading studies published in top journals (Wiswall and Zafar, 2014; Delavande and Kohler, 2015; Kendall et al., 2015). Third, Hoffman and Burks (2019) randomized whether field beliefs were incentivized and found no impact. Fourth, as discussed in the main text, and as discussed further below, the belief data appear highly sensible in many ways.

Consistency of our beliefs data with evidence in behavioral economics. One way to examine whether beliefs are sensible is to examine whether subjects' beliefs are consistent with evidence and theory in behavioral economics. In fact, a long-line of papers in psychology and economics have documented (and modeled) individuals' over-estimation of small probabilities; the work of Kahneman and Tversky (1979) on prospect theory is a notable early effort. Probability over-weighting can help explain anomalies such as the Allais (1953) paradox. Recent work using at field data (e.g., Snowberg and Wolfers, 2010; Andrikogiannopoulou and Papakonstantinou, 2016; Chiappori et al., 2019; Gandhi and Serrano-Padial, 2014; Barseghyan et al., 2013) have, in line with our results, found evidence for over-estimating events with negligible probabilities. In fact, our elicited probabilities regarding an almost zero-probability event—i.e., a "close election"— are roughly similar to estimates that Barseghyan et al. (2013) find in an entirely different environment. Structurally estimating a model of probability weighting using insurance choice data, Barseghyan et al. (2013) find that individuals act as if they place weights of approximately 6-8% on almost zero-probability events.

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

To see whether this model can help explain our belief levels, we examined whether individuals with more NBLLN are more likely to over-estimate the probability of a close election. In particular, our coin experiment tests each individual's views about the aggregate result of a sample consisting of a large number (1,000) of coin flips. We suppose that individuals who exhibit greater NBLLN systematically over-estimate the probability of "extreme" samples with a large number of observations. In our case, with a fair coin, the probability of getting between 481 and 519 heads is 78% (Benjamin et al., 2013).[2] Given the high true probability of 481-519 heads, we conceptualize an extreme sample as one outside this range.

Consistent with Benjamin et al. (2013), we find that subjects substantially underestimate the probability of 481-519. In our data, the average probability assigned to 481-519 heads was 44% instead of 78%. However, there is substantial heterogeneity and it is correlated with perceived chance of a very close election. Measuring NBLLN using the probability that a person puts outside of 481-519 heads, Table C6 shows that voters with greater NBLLN assign higher probability to the election being decided by less than 100 votes (column 3), less than 1,000 votes (column 5), or less than 100/1,000 votes. This holds controlling for

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

education, income, and other controls. Thus, individuals who overestimate the probability of extreme events in the coin-flipping domain, an easily understood stochastic process, tend to produce the highest estimates of a very close election.[3]

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

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

A.4 Two-Sample IV (TSIV) Estimation (Section III)

For the 2014 data (as well as the pooled 2010/2014 data), we cannot run an IV regression of turnout on post-treatment beliefs, instrumenting with receiving the close poll treatment. Instead, we perform a reduced form regression of turnout on whether someone received the close poll treatment, and divide the estimate by a first stage estimate using the 2010 data. In the just identified case, the TSIV estimator (Angrist and Krueger, 1992) is given by:

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

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

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

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

We use this formula for calculating TSIV standard errors.[4] Note that if there is no first stage estimation error (i.e., $var\left(\hat{\theta}_F\right) = 0$), then we have that $se\left(\hat{\theta}_{TSIV}\right) = \frac{se\left(\hat{\theta}_R\right)}{\hat{\theta}_F}$.

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

Two sample IV requires that both samples are drawn from the same overall population. While there are some differences between the 2010 and 2014 populations in observable demographics (compare Tables 1 and C14), the differences are relatively small. As discussed in Section III, one noticeable difference between the 2010 and 2014 experiments is the voting rate, where the rate was 72% in 2010 and 53% in 2014. As argued in footnote 34, this seems likely due to the internet sample having a relatively high voting rate. Still, we believe that the 2010 and 2014 populations are broadly similar.

Another way of evidencing that the 2010 and 2014 samples are broadly from the same overall population is to compare the reduced form estimates. As noted in Section III of the paper, the reduced form estimates are quite similar. With full controls, the estimate is 0.29 for 2014 (Table 6) compared to 0.23 for 2010 (Table C29).[5]

A.5 Assumption on Belief Impacts in 2014 RCT (Sections III and IV.B)

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

Another key issue is what share of postcards are read. Data on readership of standard mail postcards comes from the US Postal Service Household Diary Survey, which is based on household mail diaries. Most US postal service mail is either first-class or standard mail, and standard mail consists mostly of advertising mail (2014 USPS Household Diary Survey, p.5).[6] As seen in Table A3-31 in the 2014 USPS Household Diary Study, for 2013-2014, about 25-30% of standard mail postcards are discarded, set aside, or have a Don't Know/No

[4]Perez-Truglia and Cruces (2017) also compute TSIV standard errors using the Delta Method.

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

[6]The 2014 USPS Household Diary Survey is available at: `https://www.prc.gov/docs/93/93171/2014%20USPS%20HDS%20Annual%20Report_Final_V3.pdf`.

Response, whereas about 70-75% of standard mail postcards are read or looked at. This is a fairly high rate in absolute level and much higher than 20%. It likely reflects in part that postcards have substantially higher rates of readership relative to other media. For example, only about 55% of letter-sized envelopes in the survey were read or looked at.

Instead of standard mail, one could focus specifically on junk mail. Junk mail, also called advertising mail, "is any advertising, promotional, or sales material sent through the postal service" (2014 USPS Household Survey, p. 41).[7] One recent prominent economics study commenting on both junk mail and readership of mailers is Perez-Truglia and Cruces (2017). As noted in Perez-Truglia and Cruces (2017), the American Environmental Protection Agency found that 44% of junk mail is discarded, either unread or unopened.[8] This would include both mailers and letters, so this would suggest that more than half of junk mail is read. Perez-Truglia and Cruces (2017) conducted their own large-scale mail-in survey and achieved a response rate of 21.2% using a mailer that had to be opened. Thus, in Perez-Truglia and Cruces (2017), 21% is clearly a lower bound on readership, as a person cannot respond to a mailer if they have not opened it.

Junk mail readership is also addressed in the USPS Household Diary Studies. As seen in Figure 5.3 of the 2014 USPS Household Diary Survey, for 2014 advertising mail, it reports that 57% of households read the mail, 21% scan the mail, and 21% don't read advertising mail.[9]

While the evidence on likely readership rates is somewhat varied, it appears to us that 50% is a reasonable readership bound, and is broadly consistent with the USPS Household Diary Studies and the EPA Study. It is also more plausible than even smaller lower bounds in terms of interpreting other estimates in the literature.[10]

A.6 Additional Discussion on Section IV.B

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

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

[7] Thus, what is junk mail is more subjective than standard or first-class mail.

[8] The source is the US Environmental Protection Agency, "EPA Junk Mail Reduction," 6/28/2006; accessed via the Internet Wayback Machine on April 23, 2019.

[9] Similar rates of junk mail readership are observed in other years of the USPS Household Diary Survey, including 2015, 2016, and 2017. See Figure 5.4 on page 43 of the 2017 USPS Household Diary Survey at `https://www.prc.gov/docs/105/105134/USPS_HDS_FY17_Final%20Annual%20Report.pdf`.

[10] Suppose instead that we had selected an even lower bound such as 20%. We believe that this would lead us to infer that mailers have a tremendous effect on turnout, to the extent that it is implausible. As mentioned in the main text, a large literature finds that mailers (including postcards) tend to significantly boost turnout, sometimes by as much as 5-8pp (Gerber and Green, 2016). If only 20% of mailers were read, this would imply treatment effects of 25-40pp, which seem implausibly large to us, particularly compared to treatment effects from other forms of communication, like phone calls and in-person canvassers, where we know that treatment occurred for sure. Assuming a lower bound readership rate of 50% would lead the 5-8pp effects to scale to 10-16pp, which are still very large, but more plausible.

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

The second of these issues is discussed above in the context of TSIV. Thus, we primarily focus on the first issue.

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

While there are strong reasons focusing on perceived chance of a very close race, a perhaps more disciplined approach (and one that uses all the data) is to combine the different estimates of s together. To do this, we weight the estimates of s according to the precision of their estimates.[11] Specifically, let $\hat{s}_{marg}$, $\hat{s}_{100}$, and $\hat{s}_{1,000}$ be our estimates of s based on the three belief measures predicted vote margin, Pr(Marg <100 votes), and Pr(Marg <1,000 votes), respectively. Then, our overall estimate of s is given by:

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

where h_{marg}, h_{100}, and $h_{1,000}$ represent the precisions. To calculate a standard error for the overall estimate of s, we use the Delta Method, combined with the assumptions that $cov(\hat{s}_{marg}, \hat{s}_{100}) = cov(\hat{s}_{marg}, \hat{s}_{1,000}) = cov(\hat{s}_{100}, \hat{s}_{1,000}) = 0$, leading to:[12]

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

In forming our overall estimate of s, we choose to use the estimates of s based on the three belief measures of predicted vote margin, Pr(Marg <100 votes), and Pr(Marg <1,000 votes), as they are all based on separate data. An alternative approach is to use estimates of s based on only two belief measures, namely predicted vote margin the predicted of a margin of less than 100 or 1,000 votes. As seen in Appendix Table C33, combining these two measures leads to slightly less precision for the overall estimates than in Table 8, but precision is still very high: we can reject an s value of no more than 0.23 in our preferred pooled specification.

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

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

Assuming 20% of the 2010 belief impacts during the 2014 RCT. If we assume that beliefs impacts during the 2014 RCT were only 20% as large as those during the 2010 RCT, then we obtain an estimate of $\hat{s}_{overall} = 0.27$, with a 95% confidence interval of $[-0.08, 0.63]$. That is, if we assume a very modest impact on beliefs for 2014, then we can rule out that no more than 63% of the relationship between actual closeness and turnout is driven by perceived closeness.

Standard errors for s. In Table 8, the column 5 confidence intervals for s include estimation error from our main IV estimation (as well as from first stage estimation error for Panels B and C), but ignore estimation error in estimating how perceived closeness responds to actual closeness and in how turnout responds to actual closeness. We do this to focus on understanding the precision of our experimental estimates (as opposed to combining the precision of our experimental and non-experimental estimates).

A.7 Additional Discussion on Bandwagon Effects (Section IV.C)

Bandwagon effects could stem from multiple sources. First, individuals may simply prefer to conform to the actions of others (Callander, 2007; Hung and Plott, 2001; Goeree and Yariv, 2015) either due to intrinsic preferences for conformity, or a sense of duty. Thus, individuals receive a payoff not just from having their favored candidate win, but also from voting in a way that conforms to the median voter. A second potential mechanism is the strategic considerations at play when there is a common values component to the candidate qualities. However, if we look at the set of individuals whose beliefs do not shift with the poll results, then we would still expect our main test to be valid on this sub-sample.

Table C31 investigates these effects. In the first stage, column 1 shows that the randomly assigned poll-shown Democrat vote share causes an increase in a person's predicted Democratic vote share, which is unsurprising given the earlier evidence that people update beliefs. For every 1pp of the Democrat being ahead in the poll shown, people update 0.27pp in their belief. In columns 2-5, we examine the relation between a person believing the Democrat is ahead and their likelihood of voting Democrat.[13] The OLS result in column 2 suggests a positive relation, with a 1pp increase in Democrat vote share associated with a 0.16pp higher chance of voting Democrat. In the IV results in columns 3-5, there is no statistically significant relation (though standard errors are larger). The OLS estimates may be biased by a number of factors, including unobserved variables (e.g., whether a person watches Fox News could affect how they vote (DellaVigna and Kaplan, 2007) and their perception of who's ahead), self-justifying beliefs (i.e., deciding to vote Democrat for another reason and then justifying the belief to themselves that the candidate is popular), and measurement error in beliefs.[14]

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

[14] Appendix Table C32 shows that poll-shown Democrat vote share does lead individuals to express a greater intention of voting Democrat in our IV regression. We think that greater attention should be paid to the behavior of voting Democrat as opposed to a mere intention, as it is the behavior which is most consequential. Still, studying intentions may still be useful for us in the event that the poll information

Some theories of voting (such as common value instrumental models) predict that increased closeness beliefs should increase turnout conditional on people not changing their preferences. Thus, besides testing whether people's preferences were affected, we can also restrict to the sample of people whose preferences did not change. As seen in Appendix Table C27, our main IV results are qualitatively robust to restricting to this sample.[15]

Further Comparison of Our Results to the Literature. As noted in footnote 6 in the main text, the earlier field experiment of Ansolabehere and Iyengar (1994) found evidence of bandwagon effects as a result of randomly assigning one of two polls to around 400 voters. Given that we fail to find causal evidence of bandwagon effects with respect to actual voting, why might our results differ? One possibility is that Ansolabehere and Iyengar (1994) analyze intended vote choice, whereas we analyze actual (self-reported) vote choice. Indeed, as noted in footnote 14 in the Appendix, we do find bandwagon effects with respect to intended Democrat vote share. A prominent more recent paper finding evidence of bandwagon effects is Knight and Schiff (2010), who use a structural approach to find strong evidence of bandwagon effects in presidential primaries. One possibility for difference in results concerns primary vs. general elections. In primary elections, one is comparing among options within one's party. Because the ideological differences among candidates is presumably smaller than in a general election, voters may be more susceptible to social influences.

B Data Appendix

B.1 2010 Experiment

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

The randomization for the 2010 experiment was carried out by the statistics team at Knowledge Networks, the firm administering the experiment. Knowledge Networks conducted the randomization (as opposed to the researchers) to protect the confidential information of subjects. The randomization was conducted in SAS by sorting individuals by state, education, whether the person voted in the 2008 general election (self-reported), gender, race (white, black, hispanic, other, or 2+ race), age (breaking age into 4 categories: 18-29, 30-44, 45-59, 60+), and a random number.[16] After sorting, individuals were given a

we showed was overcome by another source of information. Combining the positive insignificant impact of Democrat vote beliefs on actual voting Democrat, combined with a positive significant impact on intention to vote Democrat, we would interpret the results as limited or inconclusive support for bandwagon effects.

[15]Further corroborating evidence is also provided by an earlier considered robustness check, where we re-did our main IV results restricting to voters with a strong ideology (Table C23). Such voters seem more likely to view voting as a private values endeavor than non-ideological voters.

[16]More precisely, the 5 race categories were: "white, non-hispanic," "black, non-hispanic," "other, non-hispanic", "hispanic", and "2+ races, non-hispanic." The education categories were: "1st, 2nd, 3rd, or 4th grade," "5th or 6th grade," "7th or 8th grade," "9th grade," "10th grade," "11th grade," "12th grade no diploma," "high school graduate - high school dipl," "some college, no degree," "associate degree," "bachelors

number "count" corresponding to their row number (i.e., a person in the 7th row was given the number 7). People with mod("count",3)=0 were assigned to Close Poll. People with mod("count",3)=1 were assigned to Not Close Poll. People with mod("count",3)=2 were assigned to Control. The sample was selected in the week of October 11, 2010 and assigned in the week of October 18, 2010.

A common approach in voting experiments (as well as field experiments in general) is to control for randomization strata (e.g., Pons, 2018). In our case, there are many small strata, such that controlling for every single strata strains the regression.[17] However, we gradually add control variables. In our full specifications in columns 3, 6, 9, and 12 of Table 4 in the main text, we control for state, education, gender, race, and age. We also control for actual voting in 2008 instead of self-reported voting. Thus, we are (approximately) controlling for all the stratification variables (even though we do not include fixed effects for every strata).[18]

As mentioned in footnote 21 in the main text, our past voting controls measure whether a person voted in past general elections in 2000, 2002, 2004, 2006, and 2008. However, young voters in 2010 may not have been eligible to vote in some of these past elections. This is not driving our results because the results are qualitatively similar (though less precise) without past voting controls. We have also repeated 4 while additionally including a control for being age 27 or younger, and the results were very similar.

Our analysis of the experiment is focused on comparing individuals receiving either the Close Poll or Not Close Poll treatments. In addition, there are individuals who were assigned to the Close or Not Close treatments (but who didn't respond to our survey), as well as individuals assigned to Control (who received no survey from us). Though we have fewer variables covering all 3 groups (the 3 groups being assigned to Close, assigned to Not Close, and Control), we also made summary statistics comparing across the 3 groups. Those assigned to the Close and Not Close treatments are well balanced. Among the 3 groups, the Control condition had a lower voting rate in the past 5 elections than those assigned to the Close or Not Close groups, as well as a slightly higher chance of being registered Democrats instead of Republicans.[19] On further investigation, we discovered that this was entirely driven by the state of California. Removing California, the 3 groups are well balanced. In Appendix Table C29, the only table that uses the Control individuals, we address the imbalance by controlling for past voting rate. Our main 2010 results are also qualitatively similar to removing California.

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

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degree," "masters degree," and "professional or doctorate degree." Over 97% of individuals who responded to our survey have "high school graduate - high school dipl" or above.

[17]Specifically, among the 6,705 people in the 2010 sample who did the pre-election survey, we have 1,623 strata. Over half of these strata (864 of 1,623) have only one observation from the sample of 6,705 people.

[18]Conclusions are unchanged if one repeats the main IV results while controlling for strata dummies.

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

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

B.2 2014 Experiment

As mentioned in footnote 32 in the main text, the anonymous vote validation company imposed a number of sample restrictions to create the voter lists for the experiment. These were:

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

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

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

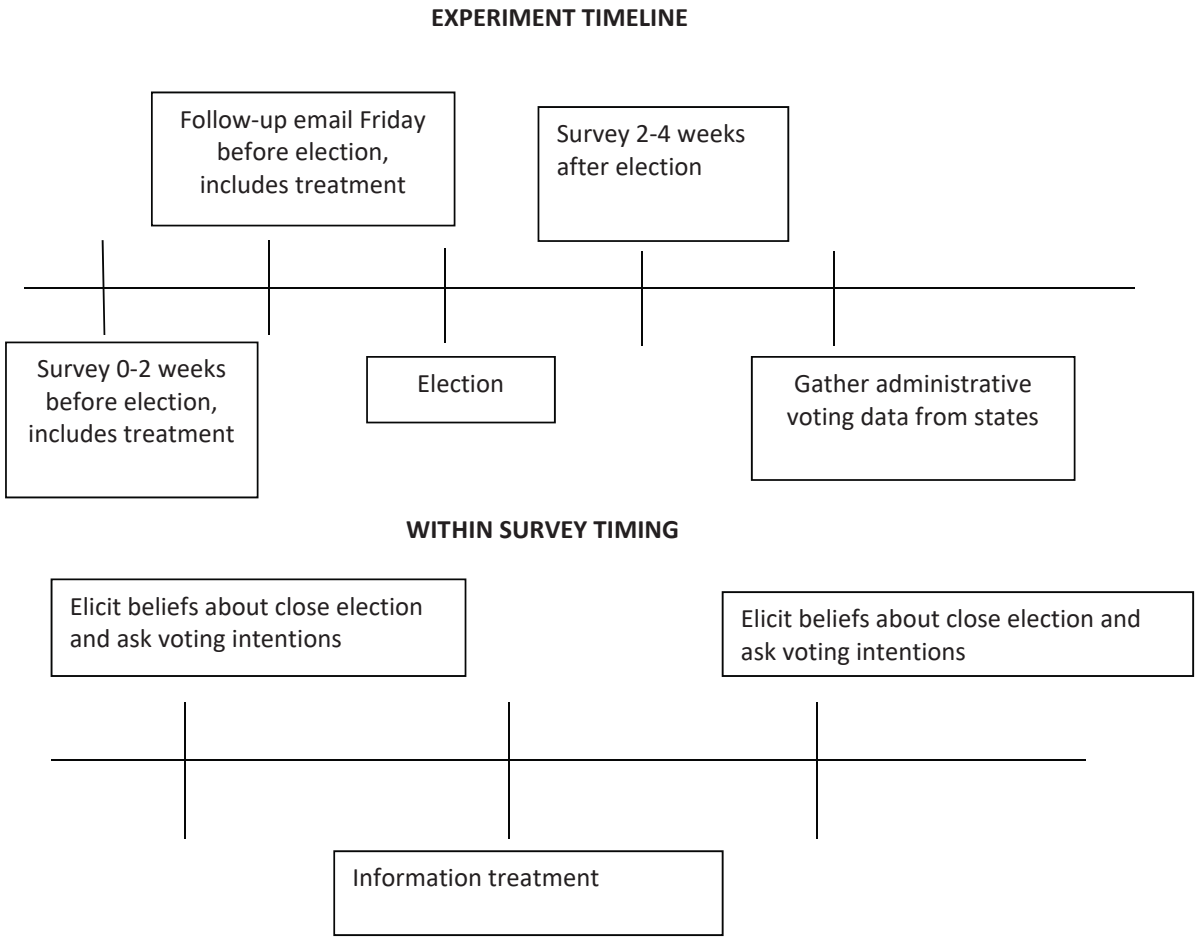
As described in the main text, in choosing polls, we first selected the most close and least close polls within the last 30 days. Because Fox News is often considered a contentious news source, we limited ourselves to non-Fox News polls (this caused us to exclude only two polls). The polls are a collection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

B.3 Additional Data

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

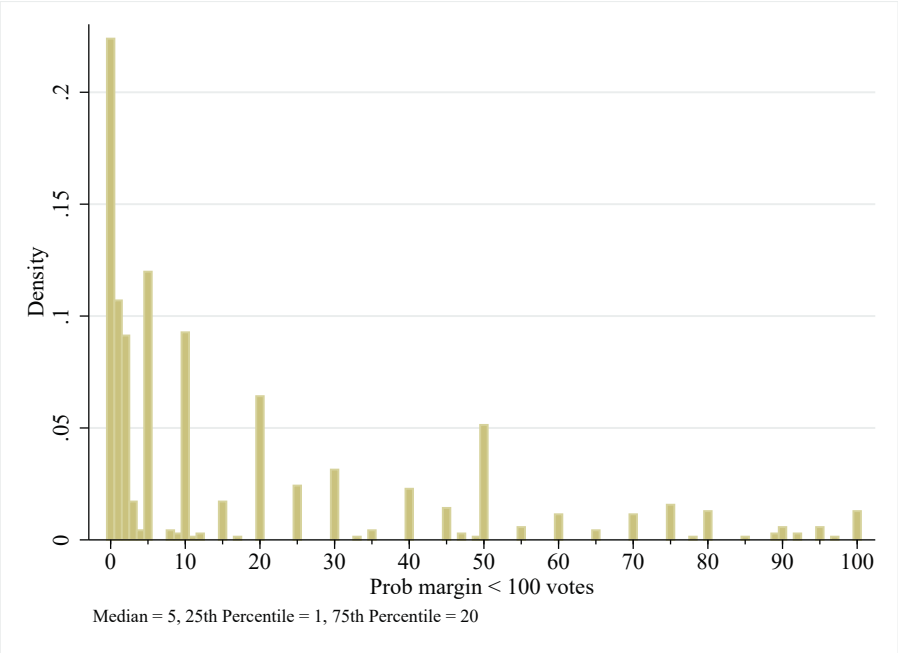
C Additional Figures and Tables

Figure C1: Timeline for the 2010 Experiment

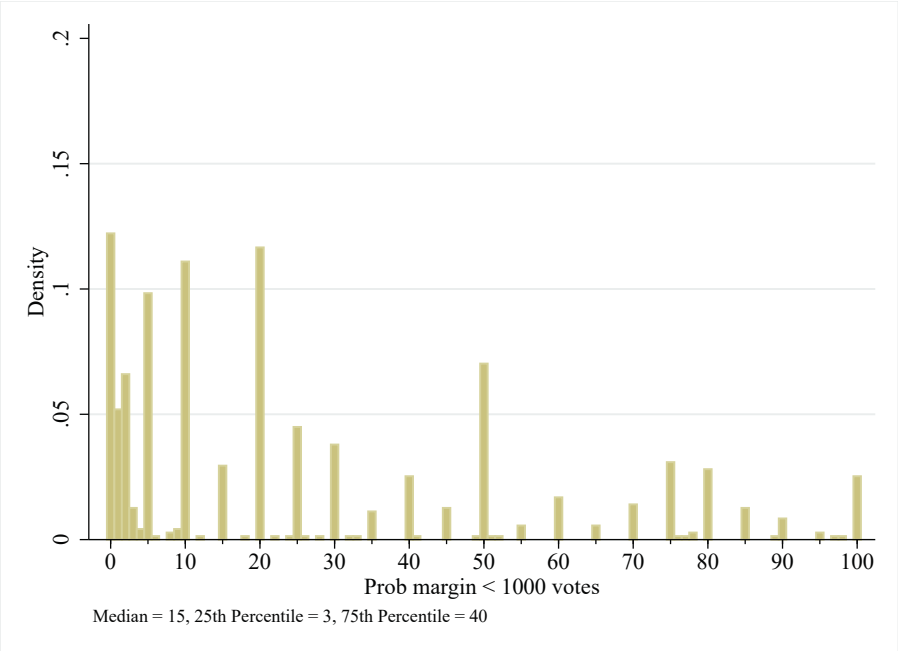


Notes: This is a timeline for the 2010 experiment. The survey time periods of 0-2 weeks before the election and 2-4 weeks after the election are approximate; please see the main text for survey dates.

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



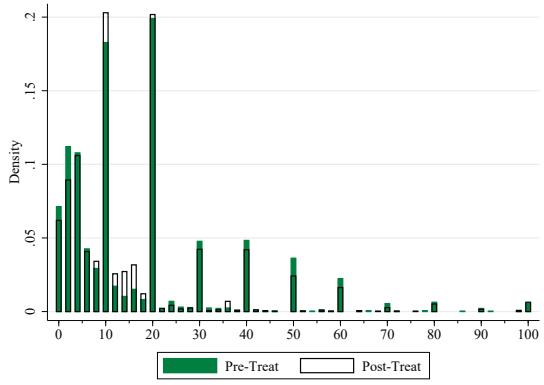
(a) Less than 100 Votes



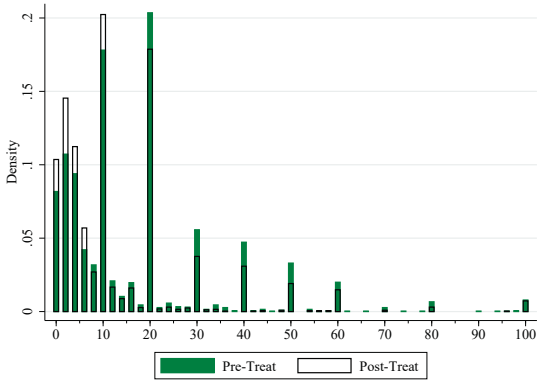
(b) Less than 1,000 Votes

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

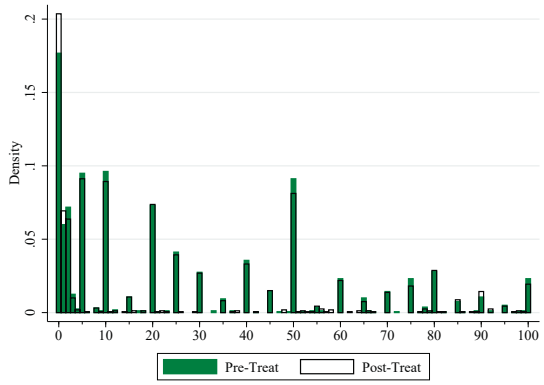
Figure C3: Distribution of Closeness Beliefs Before and After the Close and Not Close Treatments (2010 Experiment)



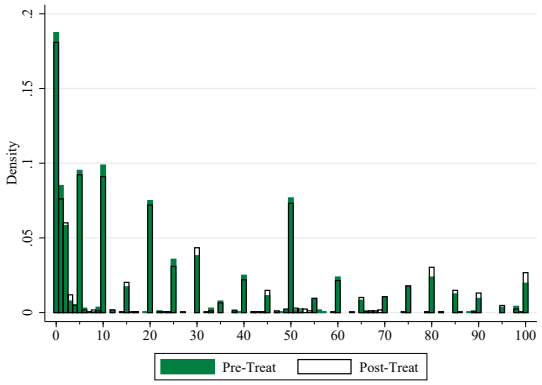
(a) Predicted Margin, Not Close Poll



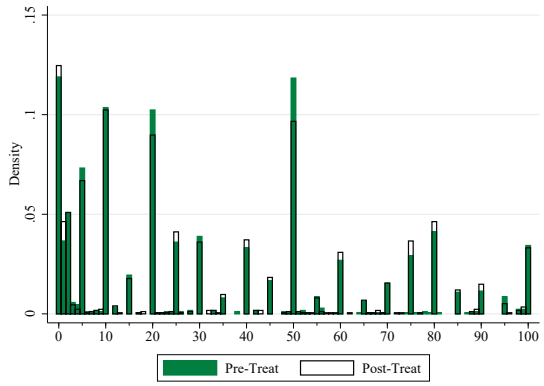
(b) Predicted Margin, Close Poll



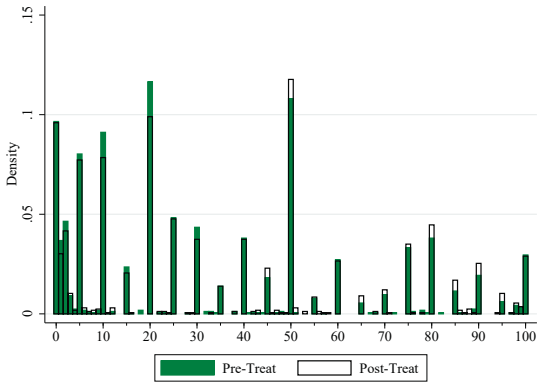
(c) Probability of margin less than 100 votes, Not Close Poll



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



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



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

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

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

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

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

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

Panel A: Provided polls and poll averages in 2010 RCT

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

Panel B: Provided polls and poll averages in 2014 RCT

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

Panel C: Provided electorate size predictions in 2014 RCT

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

Notes: Panels A-B lists the polls that were used in the 2010 and 2014 RCTs (as well as the poll averages at the time of the experiment). For example, for CA in the 2010 RCT, the close poll was "50-50," whereas the not close poll was 57% Democrat vs. 43% Republican. For the 2010 poll averages, we report the average of state polls during Sept. 10-Oct. 17. For the 2014 poll averages, we report the average of state polls (excluding Fox News polls, partisan polls, and a 56-44 MA poll during 9/16-9/21) during Sep 18 - Oct 18. These dates roughly correspond to the periods over which we searched polls to select "close" and "not-close" polls. The sample over which the poll averages are calculated may not correspond exactly to the sample from which polls were selected for the RCT, as the averages taken here are based on poll lists collected after the RCTs. Panel C lists the predicted electorate sizes that were provided in the 2014 RCT. As mentioned in footnote 29 in Section III of the main text, these are based on the predictions of 7 election experts. The numbers here represent the most extreme predictions. Election experts provided expected turnout rates. We converted these predictions into an expected turnout level, which we then rounded. For KS, the rounded "small electorate" expert prediction was actually 1,000,000, but due to a research assistant error, what we implemented in the RCT was 1,100,000, so that is what is listed.

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

Table C3: Summary Statistics for 2010 Experiment

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

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

Table C4: Comparing Experimental Samples and CPS Voter Supplements

Panel A: 2010 Election		
	2010 RCT	2010 CPS
Male	0.39	0.46
Black	0.08	0.10
Hispanic	0.06	0.10
Other	0.03	0.04
Mixed	0.02	0.01
Age	53.33	50.10
Less than high school	0.03	0.02
High school degree	0.13	0.27
Some college or associate degree	0.34	0.30
Bachelor's degree	0.29	0.22
Master's or PhD	0.21	0.13
Household income 25k to 50k	0.23	0.24
Household income 50k to 75k	0.23	0.20
Household income 75k to 100k	0.18	0.14
Household income 100k and up	0.24	0.24
Panel B: 2014 Election		
	2014 RCT	2014 CPS
Male	0.47	0.46
Black	0.13	0.12
Hispanic	0.05	0.06
Other	0.02	0.02
Age	49.90	52.37

Notes: The 2010 RCT sample is people who responded to the online survey and thus received the information treatment. The 2014 RCT sample is all people who were randomized to receive or not receive a postcard. The CPS samples restrict to registered voters in the states where the RCT was conducted. Thus, CPS demographics may differ between the 2010 and 2014 samples due to differences in state composition. "Other" race is defined as not being White, Black, Hispanic, or mixed. In addition, in the RCT data, Middle Eastern people are counted as "Other," whereas the CPS classifies Middle Eastern people as "White." This should not cause a significant discrepancy, however, as estimates of the Middle Eastern population in the U.S. are relatively small.

Table C5: Comparing Means between Non-Responders Assigned Close Treatment vs. Non-Responders Assigned Not Close Treatment: 2010 Experiment

	Close ($N = 2,065$)	Not Close ($N = 2,030$)	t-test
Registered Democrat	0.48	0.47	0.47
Registered Republican	0.33	0.36	0.18
No party affil/decline state/indep	0.16	0.14	0.13
Other party registration	0.03	0.04	0.14
Affiliate w/ Democrat party (1-7)	4.32	4.27	0.54
Ideology (1-7 Scale, 7=Ext Liberal)	3.89	3.86	0.61
Share voted previous 5 elections	0.60	0.60	0.69

Notes: This table is similar to Table 1 in the main text, but compares means for non-responders instead of for responders. There are 2,312 observations for the political registration variables, 3,975 observations for the Democrat party affiliation variable, 3,930 observations for the ideology variable, and the full 4,095 observations for the share voted variable.

Table C6: Predicting Pre-treatment Beliefs (2010 Experiment)

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

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

Table C7: The Effect of the Close Poll Treatment on Vote Margin Predictions: Robustness Check where Main Regressor is Continuous (2010 Experiment)

Dep. var = Predicted vote margin, post-treat	b_{post} (1)	b_{post} (2)	b_{post} (3)	Δb (4)	b_{post} (5)	b_{post} (6)	b_{post} (7)	b_{post} (8)
Margin in viewed poll	0.42	0.22	0.22	0.21	0.22	0.35	0.24	0.30
	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.09)	(0.06)	(0.05)
Pred vote margin, pre-treat			0.54					
			(0.02)					
Viewed marginInterest in politics (1-5 scale)						-0.03		
						(0.02)		
Viewed marginIdentify Nancy Pelosi as Speaker							-0.02	
							(0.06)	
Viewed marginShare voted previous 5 elections								-0.13
								(0.06)
Interest in politics (1-5 scale)					-0.02	0.30	-0.02	-0.01
					(0.21)	(0.28)	(0.21)	(0.21)
Identify Nancy Pelosi as Speaker					-1.53	-1.53	-1.33	-1.53
					(0.54)	(0.54)	(0.77)	(0.54)
Share voted previous 5 elections (administrative)					-1.13	-1.13	-1.14	0.05
					(0.56)	(0.56)	(0.56)	(0.77)
State FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demog Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,650	6,650	6,612	6,612	6,529	6,529	6,529	6,529

Notes: This is a robustness check to Table 2. The difference is that the main regressor is continuous instead of discrete. That is, instead of looking at whether a person received the close poll (instead of the not close poll), we examine the vote margin they observed in the poll. For example, if the voter was shown a 55-45 poll, the margin in viewed poll is equal to 10.

Table C8: The Effect of the Close Poll Treatment on the Perceived Likelihood of the Election Being Decided by Less than 100 or Less than 1,000 Votes: Robustness Check where Main Regressor is Continuous (2010 Experiment)

	Prob < 100 votes			Prob < 1,000 votes			< 100 or 1,000 votes		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Margin in viewed poll	-0.10	-0.13	-0.14	-0.39	-0.19	-0.14	-0.24	-0.16	-0.14
	(0.06)	(0.03)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.02)	(0.02)
Prob <100 votes, pre-treat		0.87	0.85						
		(0.01)	(0.01)						
Prob <1,000 votes, pre-treat					0.88	0.86			
					(0.01)	(0.01)			
Prob <100 or 1,000 votes, pre-treat								0.88	0.86
								(0.01)	(0.01)
Demog Controls	No	No	Yes	No	No	Yes	No	No	Yes
State FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	3,286	3,282	3,282	3,407	3,406	3,406	6,693	6,688	6,688

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

Table C9: The Effect of the Close Poll Treatment on Beliefs: Robustness Check where Restrict to Cases where Beliefs Change (2010 Experiment)

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

Notes: This is a robustness check to Tables 2 and 3. Columns 4-12 here are analogous to columns 1-9 of Table 3. The difference is that we restrict attention to individuals who change their beliefs. For IV results of turnout on beliefs, while also restricting to cases where beliefs change, see Table C21.

Table C10: Beliefs About the Closeness of the Election and Voter Turnout, OLS Results (2010 Experiment)

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

Notes: The dependent variable is turnout (0-1) from administrative voting records, with coefficients multiplied by 100 for ease of readability. Robust standard errors in parentheses. All specifications are OLS regressions. All regressions include state fixed effects and past voting controls (5 dummies for having voted in the general elections in 2000, 2002, 2004, 2006, and 2008). The voting rate based on administrative data is 72% (71.9% for close poll, 72.1% for not close poll). Demographic controls are as listed in Table 2.

Table C11: Replicating the Literature: Correlation between Actual Ex-post Vote Margin and Turnout (2010 Experiment)

	(1)	(2)	(3)	(4)	(5)
Actual vote margin in state	-0.34	-0.39	-0.26	-0.29	-0.13
Clustered SE by state	(0.17)	(0.17)	(0.12)	(0.10)	(0.05)
Block bootstrap SE	(0.31)	(0.29)	(0.29)	(0.23)	(0.11)
Wild bootstrap p value	[0.07]	[0.17]	[0.02]	[0.04]	[0.30]
What is an observation?	State	State	Person	Person	Person
Demographic Controls	No	No	No	Yes	Yes
Control for past voting?	No	Yes	No	No	Yes
Observations	13	13	6,705	6,705	6,705
R-squared	0.14	0.39	0.00	0.10	0.46

Notes: The dependent variable is turnout (0-1) from administrative voting records, with coefficients multiplied by 100 for ease of readability. The vote margin is the difference in percentage points between the winner and loser among the Democrat and Republican shares of the two-party vote. Columns 1-2 are cross-state regressions where each observation is a state (i.e., a gubernatorial election). In columns 1-2, the sample from columns 3-5 is collapsed by state. In contrast, in columns 3-5, an observation is a person in the 2010 experiment. Demographic controls are as listed in Table 2. In column 2, "Control for past voting?" means that we control for a person's average voting rate over the general elections in 2000, 2002, 2004, 2006, and 2008, whereas in column 5, we control for the 5 past voting dummy variables. There are 13 states (clusters). The first row of standard errors presents standard errors clustered by state. The block bootstrap is executed using "vce(bootstrap, cluster(state))" in Stata 14 and using 500 replications. The wild bootstrap is executed using "bootwildct" (Malde and Scott, 2012) in Stata 14 and using 2,000 replications. In columns 1-2, clustering by state is the same as robust standard errors (because an observation is a state). The non-robust standard errors are larger for both columns, and are equal to 0.25 in column 1 and to 0.22 in column 2. Thus, with regular / non-robust standard errors, the column 1 and 2 coefficients lose statistical significance.

Table C12: Demographics and Turnout (2010 Experiment)

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

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

Table C13: Robustness: Beliefs about the Closeness of the Election and Voter Turnout, IV Results (2010 Experiment), No Past Voting Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pred vote margin, post-treat	0.13	0.13	0.05									
	(0.39)	(0.40)	(0.39)									
Pred vote margin, pre-treat		-0.30	-0.12									
		(0.22)	(0.21)									
Pr(Marg <100 votes), post				-1.30	-0.51	-0.49						
				(2.26)	(0.62)	(0.59)						
Pr(Marg <100 votes), pre					0.31	0.36						
					(0.54)	(0.50)						
Pr(Marg <1,000 votes), post							0.03	0.03	0.35			
							(0.54)	(0.61)	(0.63)			
Pr(Marg <1,000 votes), pre								-0.06	-0.26			
								(0.54)	(0.54)			
<100 or 1,000 votes, post										-0.28	-0.20	-0.08
										(0.66)	(0.44)	(0.43)
<100 or 1,000 votes, pre											0.10	0.06
											(0.39)	(0.37)
F-stat on excl instrument	56.33	86.85	86.65	0.726	22.76	23.25	7.384	21.97	19.86	5.199	43.42	42.89
Demographic Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	6,650	6,612	6,612	3,286	3,282	3,282	3,407	3,406	3,406	6,693	6,688	6,688

Notes: This table is similar to Table 4. The difference is that we do not include Past Voting Controls (i.e., dummies for whether someone voted in the 2000, 2002, 2004, 2006, and 2008 general elections).

Table C14: Comparison of Means for 2014 Follow-up Experiment: Balance Test

Closeness:	control	close	close	notclose	notclose	close	notclose		
Electorate size:		big	small	big	small			big	small
Male	.467	.469	.471	.468	.468	.47	.468	.469	.468
Black	.131	.131	.135	.134	.132	.133	.133	.133	.132
Hispanic	.049	.048	.048	.047	.047	.048	.047	.048	.048
Other race	.023	.02	.023	.023	.023	.022	.023	.023	.021
Age	49.90	49.96	49.77	49.99	49.78	49.86	49.88	49.77	49.97
Democrat	.258	.253	.257	.258	.258	.255	.258	.258	.256
Republican	.233	.234	.234	.231	.238	.234	.234	.236	.232
Other party	.509	.513	.508	.511	.504	.511	.508	.506	.512
vote2008?	.66	.662	.658	.659	.659	.66	.659	.659	.66
vote2010?	.492	.491	.492	.491	.491	.492	.491	.492	.491
vote2012?	.714	.715	.715	.714	.712	.715	.713	.714	.714

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

Table C15: Comparison of Means for 2014 Follow-up Experiment: Balance Test, p-values

	close/notclose	close/control	control/notclose
Male	.551	.204	.656
Black	.882	.228	.319
Hispanic	.653	.226	.068
Other race	.096	.106	.505
Age	.845	.573	.769
Democrat	.37	.291	.857
Republican	.98	.741	.715
Other party	.421	.519	.641
vote2008?	.66	.789	.382
vote2010?	.904	.951	.82
vote2012?	.52	.581	.736

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

Table C16: Robustness: Beliefs about the Closeness of the Election and Information Acquisition, IV Results (2010 Experiment)

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

Notes: This table is similar to Table 4. The difference is that the dependent variable is whether an agent started to pay less attention to the campaigns (coded as -1), more attention to the campaigns (coded as +1), or an unchanged amount of attention to the campaigns (coded as 0) after being exposed to a poll, as reported in the post-election survey. As in Table 4, coefficients are multiplied by 100 for ease of readability.

Table C17: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Weight by Day of Survey (2010 Experiment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pred vote margin, post-treat	-0.29	-0.34	-0.38									
	(0.38)	(0.42)	(0.42)									
Pred vote margin, pre-treat		0.13	0.17									
		(0.23)	(0.22)									
Pr(Marg <100 votes), post				0.04	0.10	0.21						
				(0.61)	(0.68)	(0.69)						
Pr(Marg <100 votes), pre					-0.18	-0.23						
					(0.59)	(0.58)						
Pr(Marg <1,000 votes), post							0.65	0.59	0.67			
							(1.05)	(0.85)	(0.92)			
Pr(Marg <1,000 votes), pre								-0.49	-0.52			
								(0.75)	(0.80)			
<100 or 1,000 votes, post										0.33	0.33	0.42
										(0.64)	(0.57)	(0.59)
<100 or 1,000 votes, pre											-0.32	-0.36
											(0.50)	(0.51)
F-stat on excl instrument	34.82	45.12	44.72	3.395	9.664	9.545	1.444	7.328	5.992	3.255	14.48	13.24
Mean DV	72.14	72.19	72.19	72.25	72.33	72.33	71.94	71.93	71.93	72.09	72.13	72.13
Demographic Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	6,650	6,612	6,612	3,286	3,282	3,282	3,407	3,406	3,406	6,693	6,688	6,688

Notes: The table is similar to Table 4 in the main text, but we weight each observation by the day of survey response. The idea is that any washing away of beliefs would be lessened for those taking the survey last. The first day of survey response (day 14) is Wednesday, October 20, 2010. The last day of survey response is Election Day, or Tuesday, November 2, 2010. The weighting is done using "aweights" in Stata.

Table C18: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Restrict to Half of the Sample that Took the Survey Closest to the Election (2010 Experiment)

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

Notes: The table is similar to Table 4 in the main text, but we restrict to half of the sample that took the survey closest to the election. The idea is that any washing away of beliefs would be lessened for those taking the survey later. The first day of survey response (day 14) is Wednesday, October 20, 2010. The last day of survey response is Election Day, or Tuesday, November 2, 2010.

Table C19: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Sample Restricted to People Who Don't Always Vote (2010 Experiment)

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

Notes: The table is similar to Table 4 in the main text, but the sample is restricted to voters who don't always vote. That is, we drop people who voted in all 5 general elections in 2000, 2002, 2004, 2006, and 2008.

Table C20: Robustness: Impact of Close/Not Close Postcard Treatments on Turnout, Sample Restricted to People Who Don't Always Vote (2014 Experiment)

	(1)	(2)	(3)	(4)	(5)
Close poll (vs. not close poll)	0.40	0.39		0.39	
	(0.35)	(0.34)		(0.34)	
Close poll (vs. control)			0.37		
			(0.25)		
Not close poll (vs. control)			-0.02		
			(0.25)		
Small electorate likely				-0.21	
				(0.34)	
Close poll X Small electorate					0.18
					(0.48)
Close poll X Large electorate					0.28
					(0.48)
Not close poll X Small electorate					-0.31
					(0.48)
F(Close vs. NotClose)			0.254		
Mean DV if not close poll=1	29.43	29.43		29.43	29.43
Mean DV if control=1			29.42		
Additional controls	No	Yes	Yes	Yes	Yes
Observations	71,385	71,385	782,677	71,385	71,385

Notes: This table is similar to Table 6 in the main text, but the sample is restricted to voters who don't always vote. That is, we drop people who voted in all 3 general elections in 2008, 2010, and 2012.

Table C21: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Sample Restricted to People Who Change their Closeness Beliefs (2010 Experiment)

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

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

Table C22: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Using Log Beliefs and Sample Restricted to People Who Change their Closeness Beliefs (2010 Experiment)

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

Notes: The table is similar to Table C21, but we the logarithm of all the belief measures (instead of beliefs in levels). The log is calculated with 1 added inside. That is, for column 1, the regressor of interest is log(1+ post-treatment predicted vote margin). We use a 1 inside because beliefs can be 0. Recall that IV coefficients are multiplied by 100 for ease of readability. To interpret the first stage, for example, the coefficient in column 1 means that the close poll decreased believed vote margin by roughly 37% (among those in our sample of people who change their beliefs).

We also made Table 4 using log beliefs instead of beliefs in levels. The conclusions were unchanged. In terms of precision, we found that decreasing the perceived vote margin by 10% (i.e., 0.1 log points) is predicted to increase turnout by no more than 0.78pp (column 3). Increasing the perceived chance of a very close election (i.e., less than 100 or less than 1,000) by 10% is predicted to increase turnout by no more than 1.37pp (column 12). The column 1 first stage coefficient was -0.27, meaning that the treatment decreased the predicted vote margin by roughly 27%.

Table C23: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Sample Restricted to Voters with Strong Ideology (2010 Experiment)

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

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

Table C24: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Sample Restricted to Voters with Higher Interest in Politics (2010 Experiment)

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

Notes: The table is similar to Table 4 in the main text, but the sample is restricted to voters who indicated a high interest in politics. Subjects are asked about their interest in what's going on in politics and government. We define "high interest" as choosing "extremely interested" or "very interested." Those selecting "moderately interested," "slightly interested," or "not interested at all" are defined as not having high interest. For the analysis in Table 2 in the main text, we convert these 5 categories into a 5-point scale.

Table C25: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Drop Larger States (2010 Experiment)

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

Notes: The table is similar to Table 4 in the main text, but we drop individuals from larger states. To define a large state, we calculate the median electorate size in our sample. Then we drop individuals from states where the electorate is above the median.

Table C26: Robustness: Impact of Close/Not Close Postcard Treatments on Turnout, Drop Larger States (2014 Experiment)

	(1)	(2)	(3)	(4)	(5)
Close poll (vs. not close poll)	-0.00	0.02		0.02	
	(0.33)	(0.32)		(0.32)	
Close poll (vs. control)			0.30		
			(0.24)		
Not close poll (vs. control)			0.29		
			(0.23)		
Small electorate likely				0.25	
				(0.32)	
Close poll X Small electorate					0.27
					(0.46)
Close poll X Large electorate					-0.05
					(0.46)
Not close poll X Small electorate					0.18
					(0.45)
F(Close_vs_NotClose)			0.960		
Mean DV if not close poll=1	60.37	60.37		60.37	60.37
Mean DV if control=1			60.28		
Additional controls	No	Yes	Yes	Yes	Yes
Observations	73,418	73,418	804,537	73,418	73,418

Notes: This table is similar to Table 6 in the main text, but we drop individuals from larger states. To define a large state, we calculate the median electorate size in our sample. Then we drop individuals from states where the electorate is above the median.

Table C27: Robustness: Beliefs about the Closeness of the Election and Voter Turnout, IV Results (2010 Experiment), Restrict Attention to People who Don't Change their Intended Probability of Voting Democrat

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

Notes: This table is similar to Table 4 in the main text, but the sample is restricted to individuals who do not change their intended probability of voting for the Democratic candidate after observing the poll information in the pre-election survey.

Table C28: Robustness: Impact of Close/Not Close Postcard Treatments on Turnout, Restrict to People with Name on Postcard or whose Name Would Have been on Postcard (2014 Experiment)

	(1)	(2)	(3)	(4)	(5)
Close poll (vs. not close poll)	0.39	0.40		0.40	
	(0.28)	(0.28)		(0.28)	
Close poll (vs. control)			0.43		
			(0.20)		
Not close poll (vs. control)			0.03		
			(0.20)		
Small electorate likely				-0.16	
				(0.28)	
Close poll X Small electorate					0.23
					(0.39)
Close poll X Large electorate					0.15
					(0.39)
Not close poll X Small electorate					-0.41
					(0.39)
F(Close vs. NotClose)			0.153		
Mean DV if not close poll=1	51.51	51.51		51.51	51.51
Mean DV if control=1			51.45		
Additional controls	No	Yes	Yes	Yes	Yes
Observations	78,838	78,838	868,112	78,838	78,838

Notes: This table is similar to Table 6 in the main text. The difference is we restrict attention to the person to whom the postcard is addressed (or to whom the postcard would have been addressed in cases where the household did not receive a postcards). In contrast, in our main results, we include all voters in the household as being treated, both the person to whom the postcard as addressed and the potential others to whom the postcard is not addressed. In column 3, we include individuals who would have received a postcard had they been randomly assigned to receive either the close or not close treatment arms.

Table C29: Reduced Form: Impact of Close/Not Close Treatments on Turnout (2010 Expt)

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

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

Table C30: Robustness: Beliefs About the Closeness of the Election and Voter Turnout, TSIV Estimates (2014 Experiment)

Assumption regarding belief impacts of 2014 postcard:	Assume same beliefs impacts as the 2010 RCT				Assume 20% beliefs impacts relative to 2010 RCT			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predicted vote margin, post-treat	-0.10				-0.52			
	(0.09)				(0.45)			
Pr(Marg <100 votes), post		0.11				0.57		
		(0.10)				(0.50)		
Pr(Marg <1,000 votes), post			0.12				0.62	
			(0.11)				(0.55)	
<100 or 1,000 votes, post				0.12				0.60
				(0.10)				(0.52)
Observations	126,126	126,126	126,126	126,126	126,126	126,126	126,126	126,126
First Stage Results:								
Close poll treatment	-2.79	2.54	2.33	2.43	-0.56	0.51	0.47	0.49
	(0.36)	(0.53)	(0.52)	(0.37)	(0.07)	(0.11)	(0.10)	(0.07)

Notes: This table is similar to Table 7 in the main text. The difference is that we show columns 5-8 while assuming that the 2014 RCT postcards only had 20% of the impact on beliefs as the 2010 RCT online intervention.

Table C31: Testing for the Bandwagon Effect: The Effect of Beliefs about Democrat Likely Vote Share on Voting for the Democratic Candidate, IV Results (2010 Experiment)

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

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

Table C32: Robustness: Testing for Bandwagon Effects using Intended Probability of Voting Democrat (2010 Experiment)

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

Notes: The table is similar to Table C31. The difference is that we look at post-treatment intended probability of voting for the Democratic candidate as the dependent variable (as opposed to whether someone actually voted for the Democratic candidate).

Table C33: The Relevance of Perceived Closeness for the Observational Relationship between Actual Closeness and Voter Turnout: Robustness, where Combine Two Belief Measures (Predicted Margin and less than 100/1,000 combined measure)

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

Notes: This table presents a robustness check for columns 4-5 in Table 8 for the overall estimates of s. Table 8 used three belief measures to create the estimates there: Predicted vote margin, Pr(Marg <100 votes), and Pr(Marg <1,000 votes). In contrast, this table uses two belief measures: Predicted vote margin and the perceived probability of less than 100 or 1,000 votes (as people are only asked about 100 or 1,000 words). This is calculated assuming that the 2014 RCT postcards had 100% of the impact on beliefs as the 2010 RCT online intervention.

D Documents for the Experiments

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

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

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

Select one answer only

- ○ Yes
- ○ No

Next

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

Select one answer only

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

Next

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

Select one answer only

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

Next

What job or political office is held by Nancy Pelosi?

Select one answer only

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

Next

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

Type in the answer into each cell in the grid

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

Please make sure these numbers add up to 100%.

Next

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

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

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

Next

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

Type in the number for the answer

[ ] %

Next

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

Type in the answer into each cell in the grid

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

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

Next

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

Type in the number for the answer

[ ] %

Next

Below are the results of a recent poll about the race for governor. The poll was conducted over-the-phone by a leading professional polling organization. People were interviewed from all over the state, and the poll was designed to be both non-partisan and representative of the voting population. Polls such as these are often used in forecasting election results.

Of people supporting either the Democratic or Republican candidates, the percent supporting each of the candidates were:

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

Next

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

Next

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

Type in the answer into each cell in the grid

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

Recent Poll Results:

John Kitzhaber (Democrat): 51%

Chris Dudley (Republican): 49%

Next

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

Type in the number for the answer

[ ] %

Recent Poll Results:

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

Next

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

Type in the answer into each cell in the grid

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

Recent Poll Results:

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

Next

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

Type in the number for the answer

[ ] %

Recent Poll Results:

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

Next

Thinking about this topic, do you have any comments you would like to share?

Any comments welcome!

Next

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

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

Poll Results:

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

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

Imagine you had a fair coin that was flipped 1,000 times. What do you think is the percent chance that you would get the following number of heads:

Type in the answer into each cell in the grid

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

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

Next

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

Select one answer only

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

Next

Did you vote for governor in the November 2010 election?

Select one answer only

- Yes
- No

Next

Which candidate did you vote for?

Select one answer only

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

Next

Did you vote for senator in the November 2010 election?

Select one answer only

- ○ Yes
- ○ No

Next

Which candidate did you vote for?

Select one answer only

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

Next

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

Select one answer only

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

Next

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

Select one answer only

- Yes
- No

Next

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

Type in the answer into each cell in the grid

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

Please make sure your answers add up to 100 percent.

Next

Thinking about this topic, do you have any comments you would like to share?

Any comments welcome!

Next

D.4 Postcard for the 2014 Experiment

VOTING 2014 COUNTS

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

PRESORTED
FIRST CLASS
US POSTAGE
PAID
XXXXXXXXX
XXXXXXXXX

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

VCT14_010

THE ELECTION ON NOVEMBER 4 IS COMING UP

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

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

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

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

We hope you decide to participate and vote this November!

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

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