"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 4.3 and 6. Appendix B gives more details on the data. Appendix C provides additional figures and tables. Appendix D provides a formal model to accompany the discussion from Section 2. Appendix E provides documents used in the experiments.

A Additional Discussion

A.1 Discussion on IV Estimates (Section 4.3)

One seemingly non-standard feature of Table 6 is that we use the same instrumental variable to instrument different closeness variable one at a time. Our view is that the different closeness variables likely represent different forms or constructs of a person's underlying perception of election closeness. To the extent that they represent different underlying constructs, we show here that any resulting inconsistency in the IV estimates is in the direction away from 0, making the true impact of each closeness variable an even tighter zero than the one we estimate (under the assumption that the different closeness variables do not affect turnout in the unexpected direction, if they have any affect at all).[1]

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

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

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

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

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

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

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

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

Last, it is unsurprising that the IV estimates are statistical 0's, given that the reduced form relationship between getting the close poll and turnout is also zero (Appendix Table C24).

A.2 Discussion on Are Belief Levels Sensible? (Section 6.1)

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

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

To see whether this model can help explain our belief levels, we examined whether individuals with more NBLLN are more likely to over-estimate the probability of a close election. In particular, our coin experiment tests each individual's views about the aggregate result of a sample consisting of a large number (1,000) of coin flips. We suppose that individuals who exhibit greater NBLLN systematically over-estimate the probability of "extreme" samples with a large number of observations. In our case, with a fair coin, the probability of getting

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

between 481 and 519 heads is 78% (Benjamin et al., 2013).[3] Given the high true probability of 481-519 heads, we conceptualize an extreme sample as one outside this range.

Consistent with (Benjamin et al., 2013), we find that subjects substantially underestimate the probability of 481-519. In our data, the average probability assigned to 481-519 heads was 44% instead of 78%. However, there is substantial heterogeneity and it is correlated with perceived chance of a very close election. Measuring NBLLN using the probability that a person puts outside of 481-519 heads, Table C4 shows that voters with greater NBLLN assign higher probability to the election being decided by less than 100 votes (column 3), less than 1,000 votes (column 5), or less than 100/1,000 votes. This holds controlling for education, income, and other controls. Thus, individuals who overestimate the probability of extreme events in the coin-flipping domain, an easily understood stochastic process, tend to produce the highest estimates of a very close election.[4]

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

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

A.3 Additional Discussion on Section 6.2

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

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

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

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

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

Which measure of beliefs. It is not clear to us which measure of beliefs should be preferred (as perceived margin and the perceived probabilities of a very close election are different variable constructs 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.[5] Specifically, let $\hat{s}_{marg}$, $\hat{s}_{100}$, and $\hat{s}_{1,000}$ be our estimates of s based on the three belief measures predicted vote margin, Pr(Marg <100 votes), and Pr(Marg <1,000 votes), respectively. Then, our overall estimate of s is given by:

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

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

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

In forming our overall estimate of s, we choose to use the estimates of s based on the three belief measures of predicted vote margin, Pr(Marg <100 votes), and Pr(Marg <1,000 votes), as they are all based on separate data. An alternative approach is to use estimates of s based on only two belief measures, namely predicted vote margin the predicted of a margin of less than 100 or 1,000 votes. As seen in Appendix Table C28, 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.

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

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

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

One thing we can do is to ask how small would the effect on beliefs need to be for us not to be able to reject $s = 1$. For our preferred specification using the pooled data, the effects on beliefs would need to be about 8 times smaller to fail to reject $s = 1$. It seems very unlikely to us that our 2014 postcard's effect on beliefs would be 8 times smaller than the effect of the 2010 online survey. If we assume that the impact on beliefs in the 2014 experiment was only half as large as in 2010, we obtain an estimate of $\hat{s}_{overall} = 0.11$, with a 95% confidence interval of $[-0.03, 0.25]$. This evidence indicates that our conclusions are qualitatively robust to more conservative assumptions about how beliefs were affected during the 2014 experiment.

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

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

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

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

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

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

Two sample IV requires that both samples are drawn from the same overall population. While there are some differences between the 2010 and 2014 populations in observable demographics (compare Tables 1 and C11), the differences are relatively small. As discussed in Section 5, one noticeable difference between the 2010 and 2014 experiments is the voting rate, where the rate was 72% in 2010 and 53% in 2014. As argued in footnote 30, this seems likely due to the internet sample having a relatively high voting rate. Still, we believe that the 2010 and 2014 populations are broadly similar.

Another way of evidencing that the 2010 and 2014 samples are broadly from the same overall population is to compare the reduced form estimates. As noted in Section 5 of the paper, the reduced form estimates are quite similar. With full controls, the estimate is 0.29 for 2014 (Table 7) compared to 0.23 for 2010 (Table C24).[7]

Standard errors for s. In Table 8, the column 5 confidence intervals for s include estimation error from our main IV estimation (as well as from first stage estimation error for 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.4 Additional Discussion on Bandwagon Effects (Section 6.3)

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

Table C26 investigates these effects. In the first stage, column 1 shows that the randomly assigned poll-shown Democrat vote share causes an increase in a person's predicted Democratic vote share, which is unsurprising given the earlier evidence that people update beliefs. For every 1pp of the Democrat being ahead in the poll shown, people update 0.27pp in their belief. In columns 2-5, we examine the relation between a person believing the Democrat is ahead and their likelihood of voting Democrat.[9] The OLS result in column 2 suggests a positive

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

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

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

relation, with a 1pp increase in Democrat vote share associated with a 0.16pp higher chance of voting Democrat. In the IV results in columns 3-5, there is no statistically significant relation (though standard errors are larger). The OLS estimates may be biased by a number of factors, including unobserved variables (e.g., whether a person watches Fox News could affect how they vote (DellaVigna and Kaplan, 2007) and their perception of who's ahead), self-justifying beliefs (i.e., deciding to vote Democrat for another reason and then justifying the belief to themselves that the candidate is popular), and measurement error in beliefs.[10]

As discussed earlier, some theories of voting (such as common value instrumental models) predict that increased closeness beliefs should increase turnout conditional on people not changing their preferences. Thus, besides testing whether people's preferences were affected, we can also restrict to the sample of people whose preferences did not change. As seen in Appendix Table C22, our main IV results are qualitatively robust to restricting to this sample.[11]

Further Comparison of Our Results to the Literature. As noted in footnote 4 in the main text, the earlier field experiment of Ansolabehere and Iyengar (1994) found evidence of bandwagon effects as a result of randomly assigning one of two polls to around 400 voters. Given that we fail to find causal evidence of bandwagon effects with respect to actual voting, why might our results differ? One possibility is that Ansolabehere and Iyengar (1994) analyze intended vote choice, whereas we analyze actual (self-reported) vote choice. Indeed, as noted in footnote 10 in the Appendix, we do find bandwagon effects with respect to intended Democrat vote share. A prominent more recent paper finding evidence of bandwagon effects is Knight and Schiff (2010), who use a structural approach to find strong evidence of bandwagon effects in presidential primaries. One possibility for difference in results concerns primary vs. general elections. In primary elections, one is comparing among options within one's party. Because the ideological differences among candidates is presumably smaller than in a general election, voters may be more susceptible to social influences.

B Data Appendix

B.1 2010 Experiment

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

The randomization for the 2010 experiment was carried out by the statistics team at Knowledge Networks, the firm administering the experiment. Knowledge Networks conducted

[10] Appendix Table C27 shows that poll-shown Democrat vote share does lead individuals to express a greater intention of voting Democrat in our IV regression. We think that greater attention should be paid to the behavior of voting Democrat as opposed to a mere intention, as it is the behavior which is most consequential. Still, studying intentions may still be useful for us in the event that the poll information we showed was overcome by another source of information. Combining the positive insignificant impact of Democrat vote beliefs on actual voting Democrat, combined with a positive significant impact on intention to vote Democrat, we would interpret the results as limited or inconclusive support for bandwagon effects.

[11] Further corroborating evidence is also provided by an earlier considered robustness check, where we re-did our main IV results restricting to voters with a strong ideology (Table C19). Such voters seem more likely to view voting as a private values endeavor than non-ideological voters.

the randomization (as opposed to the researchers) to protect the confidential information of subjects. The randomization was conducted in SAS by sorting individuals by state, education, whether the person voted in the 2008 general election (self-reported), gender, race (white, black, hispanic, other, or 2+ race), age (breaking age into 4 categories: 18-29, 30-44, 45-59, 60+), and a random number.[12] After sorting, individuals were given a number "count" corresponding to their row number (i.e., a person in the 7th row was given the number 7). People with mod("count",3)=0 were assigned to Close Poll. People with mod("count",3)=1 were assigned to Not Close Poll. People with mod("count",3)=2 were assigned to Control. The sample was selected in the week of October 11, 2010 and assigned in the week of October 18, 2010.

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

As mentioned in footnote 20 in the main text, our past voting controls measure whether a person voted in past general elections in 2000, 2002, 2004, 2006, and 2008. However, young voters in 2010 may not have been eligible to vote in some of these past elections. This is not driving our results because the results are qualitatively similar (though less precise) without past voting controls. We have also repeated 6 while additionally including a control for being age 27 or younger, and the results were very similar.

Our analysis of the experiment is focused on comparing individuals receiving either the Close Poll or Not Close Poll treatments. In addition, there are individuals who were assigned to the Close or Not Close treatments (but who didn't respond to our survey), as well as individuals assigned to Control (who received no survey from us). Though we have fewer variables covering all 3 groups (the 3 groups being assigned to Close, assigned to Not Close, and Control), we also made summary statistics comparing across the 3 groups. Those assigned to the Close and Not Close treatments are well balanced. Among the 3 groups, the Control condition had a lower voting rate in the past 5 elections than those assigned to the Close or Not Close groups, as well as a slightly higher chance of being registered Democrats instead of Republicans.[13] On further investigation, we discovered that this was entirely driven by the state of California. Removing California, the 3 groups are well balanced. In Appendix Table C24, 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

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

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

survey was being launched shortly before 9pm on Tuesday, October 19th, 2010. However, the first responses in our data are time stamped as occurring shortly after midnight on Wednesday, Oct. 20th, 2010. We believe that this includes people who took the survey after midnight on the East Coast, as well as those who took it before midnight in the Central and Pacific time zones.

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

B.2 2014 Experiment

As mentioned in footnote 28 in the main text, the anonymous vote validation company imposed a number of sample restrictions to create the voter lists for the experiment. These were:

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

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

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

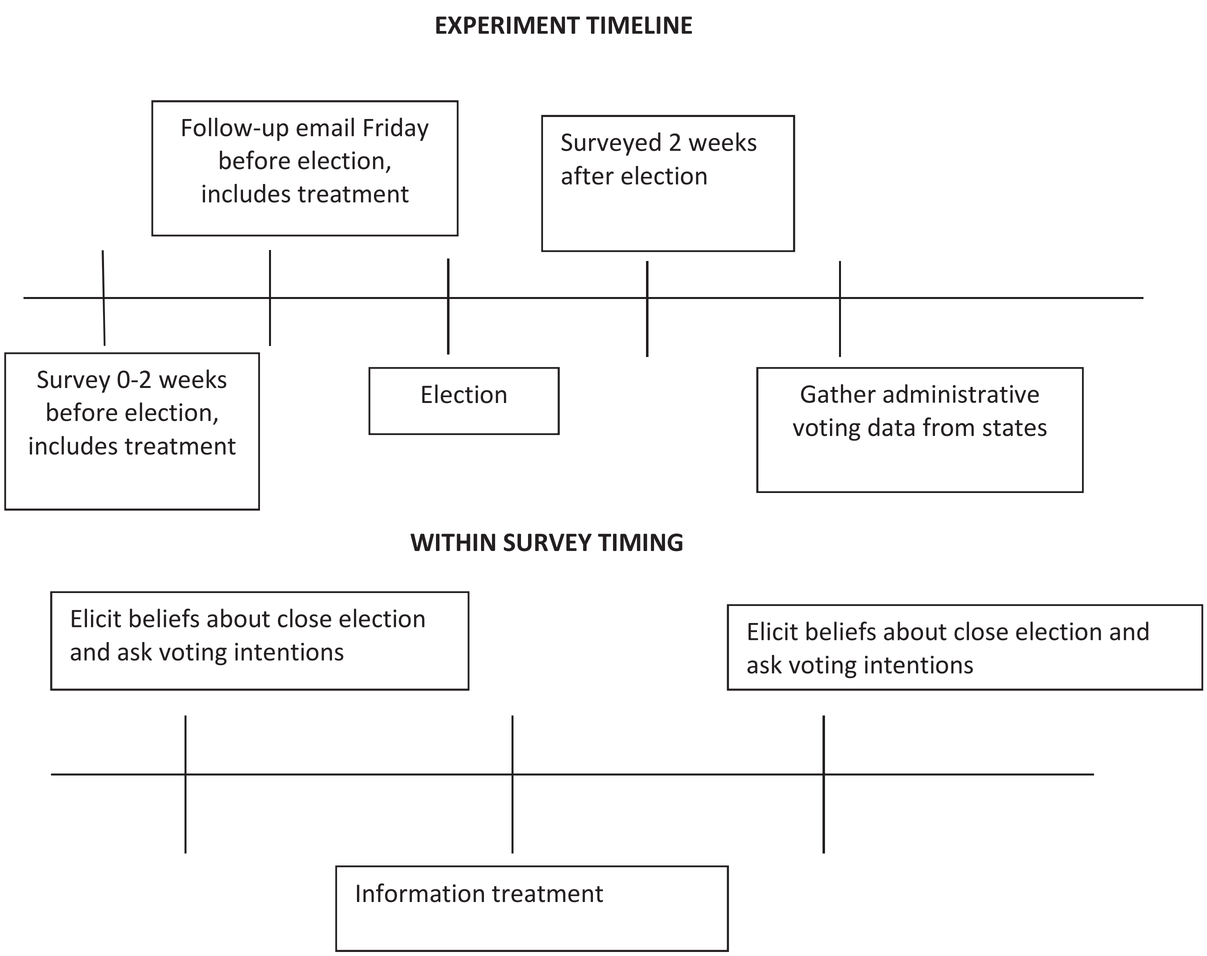
As described in the main text, in choosing polls, we first selected the most close and least close polls within the last 30 days. Because Fox News is often considered a contentious news source, we limited ourselves to non-Fox News polls (this caused us to exclude only two polls). The polls are a collection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

B.3 Additional Data

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

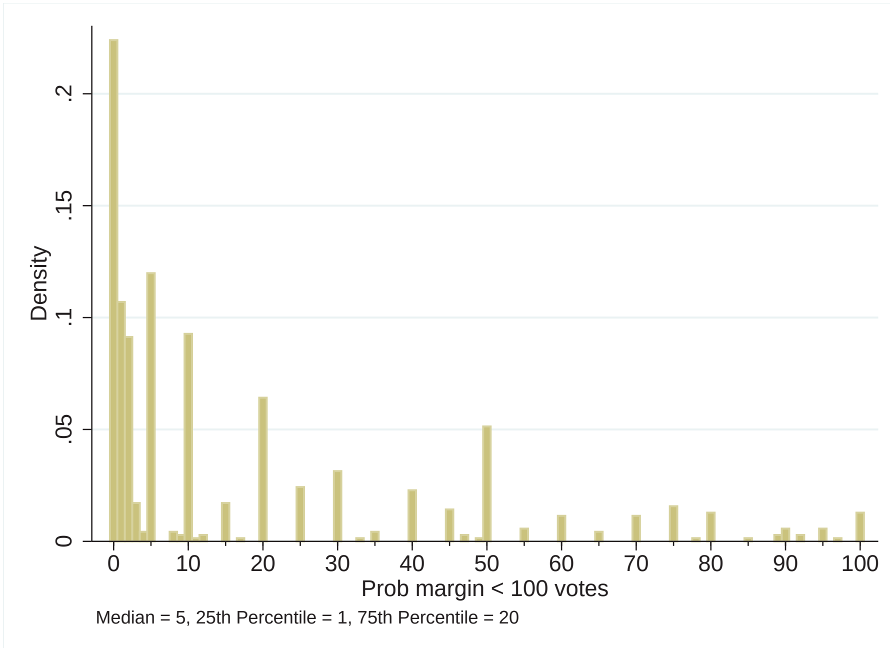
C Additional Figures and Tables

Figure C1: Timeline for the 2010 Experiment

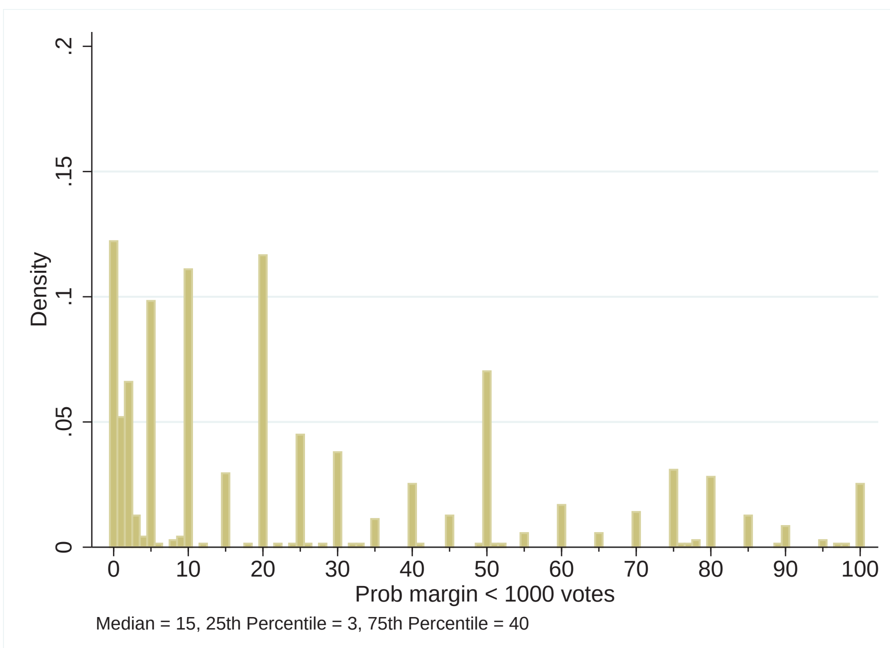


Notes: This is a timeline for the 2010 experiment.

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



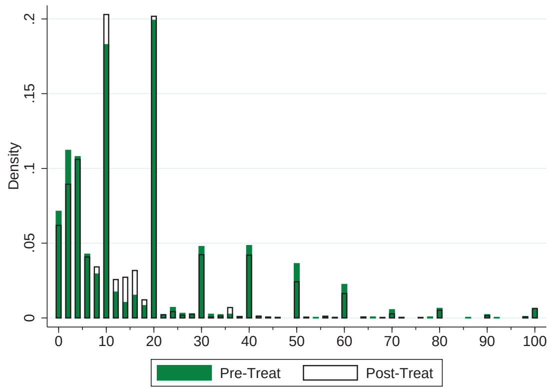
(a) Less than 100 Votes



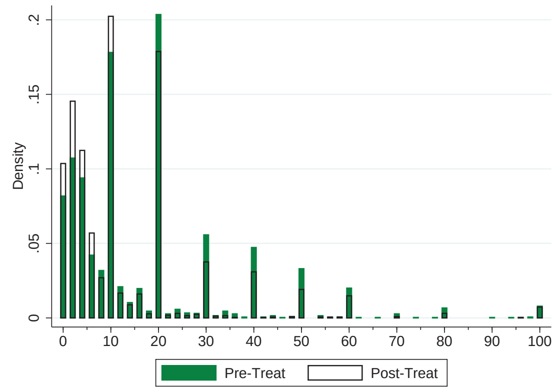
(b) Less than 1,000 Votes

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

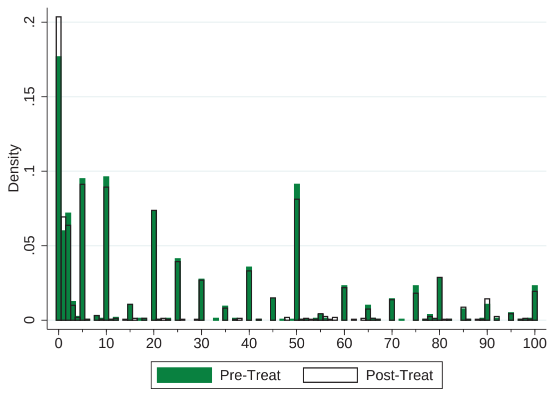
Figure C3: Distribution of Closeness Beliefs Before and After the Close and Not Close Treatments (2010 Experiment)



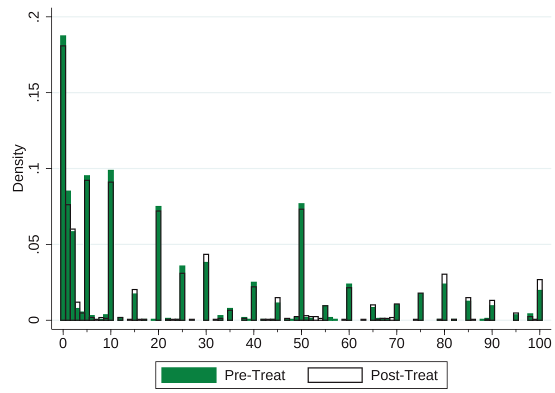
(a) Predicted Margin, Not Close Poll



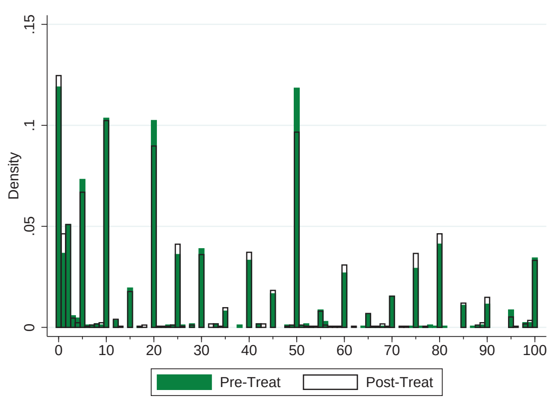
(b) Predicted Margin, Close Poll



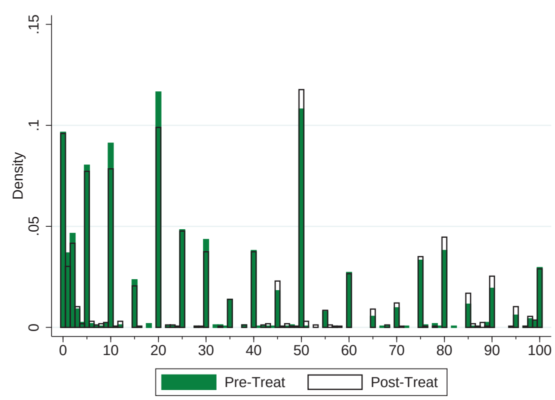
(c) Probability of margin less than 100 votes, Not Close Poll



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



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



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

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

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

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

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

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

Panel A: 2010 experiment, polls provided

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

Panel B: 2014 experiment, polls provided

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

Panel C: 2014 expt, electorate sizes provided

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

Notes: Panels A-B lists the polls that were used in the 2010 and 2014 experiments. For example, for California in the 2010 experiment, the close poll was "50-50," whereas the not close poll was 57% Democrat vs. 43% Republican. Panel C lists the predicted electorate sizes that were provided in the 2014 experiment. As mentioned in footnote 26 in Section 5 of the main text, these are based on the predictions of 7 election experts. The numbers here represent the most extreme predictions.

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

Table C3: Summary Statistics for 2010 Experiment

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

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

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

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

Notes: This table presents OLS regressions of voters' pre-treatment beliefs on various covariates. It covers voters' perception the election is decided by less than 100 or 1,000 votes, as well as voters' predictions of the vote margin and vote share for the Democrat. Standard errors are in parentheses, and account for clustering by state using a block bootstrap (500 replications). We account for clustering by state because actual margin and electorate size vary at the state level, and we use a block bootstrap because we only have 13 states. The block bootstrap is executed using "vce(bootstrap, cluster(state))" in Stata 14. The vote margin is the difference in percentage points between the winner and loser among the Democrat and Republican shares of the two-party vote. The subjective prob that the number of heads in 1000 flips would be outside of 481-519 is our measure of non-belief in the law of large numbers (NBLLN), and is discussed further in Appendix A.2. This number is calculated as 100 minus the probability expressed for 481-519. This number is defined as long as someone gives a non-missing answer for 481-519 heads. The correlation here becomes stronger if we restrict attention to people giving non-missing answers on all 7 bins. * significant at 10%; ** significant at 5%; *** significant at 1%.

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

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

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

Table C6: The Effect of the Close Poll Treatment on the Perceived Likelihood of the Election Being Decided by Less than 100 or Less than 1,000 Votes: Robustness Check where Main Regressor is Continuous (2010 Experiment)

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

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

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

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

Notes: This is a robustness check to Tables 3 and 4. Columns 4-12 here are analogous to columns 1-9 of Table 4. The difference is that we restrict attention to individuals who change their beliefs. * significant at 10%; ** significant at 5%; *** significant at 1%

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

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

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

Table C9: Demographics and Turnout (2010 Experiment)

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Table C16: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Sample Restricted to People Who Don't Always Vote (2010 Experiment)

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

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

Table C17: 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)
Close poll (vs. not close poll)	0.43	0.42		0.42
	(0.35)	(0.34)		(0.34)
Close poll (vs. control)			0.38	
			(0.25)	
Not close poll (vs. control)			-0.03	
			(0.25)	
Small electorate likely				-0.21
				(0.34)
F(Close vs. NotClose)			0.228	
Mean DV if not close poll=1	29.43	29.43		29.43
Mean DV if control=1			29.42	
Additional controls	No	Yes	Yes	Yes
Observations	71,385	71,385	782,677	71,385

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

Table C18: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Sample Restricted to People Who Change their Closeness Beliefs (2010 Experiment)

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

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

Table C19: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Sample Restricted to Voters with Strong Ideology (2010 Experiment)

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

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

Table C20: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Drop Larger States (2010 Experiment)

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

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

Table C21: Robustness: Impact of Close/Not Close Postcard Treatments on Turnout, Drop Larger States (2014 Experiment)

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

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

Table C22: Robustness: Beliefs about the Closeness of the Election and Voter Turnout, IV Results (2010 Experiment), Restrict Attention to People who Don't Change their Intended Probability of Voting Democrat

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

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

Table C23: 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)
Close poll (vs. not close poll)	0.39	0.40		0.40
	(0.28)	(0.28)		(0.28)
Close poll (vs. control)			0.43**	
			(0.20)	
Not close poll (vs. control)			0.04	
			(0.20)	
Small electorate likely				-0.16
				(0.28)
F(Close_vs_NotClose)			0.155	
Mean DV if not close poll=1	51.51	51.51		51.51
Mean DV if control=1			51.45	
Additional controls	No	Yes	Yes	Yes
Observations	78,838	78,838	868,112	78,838

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

Table C24: Reduced Form: Impact of Close/Not Close Treatments on Turnout (2010 Expt)

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

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

Table C25: Beliefs About the Closeness of the Election and Voter Turnout, TSIV Estimates

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

Notes: This table shows two-sample IV (TSIV) estimates of how beliefs about the closeness of the election affect turnout. The dependent variable is turnout (0-1) from administrative voting records, with coefficients multiplied by 100 for ease of readability. Turnout is defined at the individual level, and is based on merging by date of birth. The reduced form (estimated using the 2014 experiment) is from column 4 of Table 7 and is based on the coefficient "Close poll (vs. not close poll)". The first stage (estimated using the 2010 experiment) is based on column 2 of Table 3, as well as columns 3, 6, and 9 of Table 4. Standard errors are calculated by the Delta Method (see Appendix A.3). * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C26: Testing for the Bandwagon Effect: The Effect of Beliefs about Democrat Likely Vote Share on Voting for the Democratic Candidate, IV Results (2010 Experiment)

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

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

D Appendix: Theory

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

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

Table C27: Robustness: Testing for Bandwagon Effects using Intended Probability of Voting Democrat (2010 Experiment)

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

Notes: The table is similar to Table C26. 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 C28: The Relevance of Perceived Closeness for the Observational Relationship between Actual Closeness and Voter Turnout: Robustness, where Combine Two Belief Measures (Predicted Margin and less than 100/1,000 combined measure)

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

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

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

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

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

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

D.1 Information and Beliefs

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

and

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Proof: The proof proceeds in three steps.

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

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

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

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

Observe that

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

if and only if

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

which is the same as

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

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

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

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

and after the not-close poll is

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

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

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

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

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

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

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

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

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

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

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

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

becomes

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

Equivalently, this is

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

or

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

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

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

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

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

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

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

D.2 Beliefs and Actions: Instrumental Private Values Models

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

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

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

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

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

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

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

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

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

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

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

Likewise, the average participation rate for B supporters is

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

D.3 Information and Common-Values Instrumental Voting

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

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

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

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

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

A more general approach contemplates that voters have both ideological and valence elements to preferences, as in Feddersen and Pesendorfer (1997). Here, voters receive (private) signals about the valence (i.e., quality) of candidates and vote based on their assessment of ideology, candidate quality differences, and, of course, the likelihood of affecting the outcome. Observing a poll showing one candidate leading strongly then has two effects–it potentially informs voters about quality differences and about the likelihood of being decisive. The former effect raises the value of voting, as voters are now more certain of the quality of the leading candidate. The latter effect reduces the value of voting, since 1 vote is less likely to be decisive.

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

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

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

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

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

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

D.4 Information and Signaling

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

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

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

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

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

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

D.5 Information and Ethical Voting

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

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

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

D.6 Calibration

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

E Documents for the Experiments

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

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

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

Select one answer only

- Yes
- No

Next

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

Select one answer only

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

Next

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

Select one answer only

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

Next

What job or political office is held by Nancy Pelosi?

Select one answer only

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

Next

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

Type in the answer into each cell in the grid

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

Please make sure these numbers add up to 100%.

Next

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

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

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

Next

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

Type in the number for the answer

[ ] %

Next

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

Type in the answer into each cell in the grid

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

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

Next

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

Type in the number for the answer

[ ] %

Next

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

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

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

Next

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

Next

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

Type in the answer into each cell in the grid

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

Recent Poll Results:

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

Next

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

Type in the number for the answer

[ ] %

Recent Poll Results:

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

Next

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

Type in the answer into each cell in the grid

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

Recent Poll Results:

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

Next

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

Type in the number for the answer

[ ] %

Recent Poll Results:

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

Next

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

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

Poll Results:

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

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

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

Type in the answer into each cell in the grid

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

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

Next

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

Select one answer only

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

Next

Did you vote for governor in the November 2010 election?

Select one answer only

- ○ Yes
- ○ No

Next

Which candidate did you vote for?

Select one answer only

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

Next

Did you vote for senator in the November 2010 election?

Select one answer only

- ○ Yes
- ○ No

Next

Which candidate did you vote for?

Select one answer only

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

Next

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

Select one answer only

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

Next

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

Select one answer only

- Yes
- No

Next

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

Type in the answer into each cell in the grid

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

Please make sure your answers add up to 100 percent.

Next

E.4 Postcard for the 2014 Experiment

VOTING
2014
COUNTS

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

PRESORTED
FIRST CLASS
US POSTAGE
PAID
XXXXXXXXX
XXXXXXXXX

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

VCT14_010

THE ELECTION ON NOVEMBER 4 IS COMING UP

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

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

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

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

We hope you decide to participate and vote this November!

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

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