

# Can Electoral Competition Mobilize Under-represented Citizens?

## Evidence from a Field Experiment in the Aftermath of a Tied Election

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### Abstract

Many Americans habitually abstain from the political process, allowing some citizens to achieve better representation than others. Political scientists and pundits assume that greater electoral competition will motivate these under-represented citizens and bring them to the polls. First, analyzing observational and survey evidence, we find little support for this claim. Then exploiting the rare opportunity of a tied election for major political office, we conduct a large-scale field experiment. Informing citizens that an upcoming election will be close has little mobilizing effect. To the extent that we do detect an effect of electoral competition on turnout, it is concentrated among frequent voters. Our evidence suggests that increased electoral competition is not a solution to inequality in turnout.

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Electoral competition and citizen participation are essential components of a healthy democracy (Verba, Schlozman, and Brady, 1995). In fact, theorists argue that competition and participation are complementary and that both are contributing factors to social and political equality. In this paper, we review the evidence and exploit an extremely rare opportunity to test this claim. Not only is there little support for the notion that electoral competition increases citizen participation, but, if anything, competition might enhance inequality. This paper makes two contributions: First, using multiple data sources and an unprecedented experiment, we subject the individual calculus of voting to its most thorough test to date. Second, using the evidence garnered in this analysis we test claims for how electoral competition might level inequalities in voter participation.

Many American citizens abstain from the political process. Since reliable data has become available, at least 30% of eligible citizens have failed to vote in any given presidential election (McDonald and Popkin 2000). Moreover, voting is a habitual act (Verba and Nie 1972; Miller and Shanks 1996; Green and Shachar 2000; Plutzer 2002; Gerber, Green, and Shachar 2003; Meredith 2009), meaning that those who abstain in one election are likely to continue their nonparticipation. Habitual voters are unrepresentative of the wider population (Verba and Nie 1972; Verba et al. 1995; Wolfinger and Rosenstone 1980): they are on average wealthier, more educated, older, more likely to be white, and more likely to attend church compared to regular voters.

Political participation is among the most studied phenomena by political scientists. And for good reason: inequality in voter turnout likely has tangible consequences. There is substantial evidence that higher turnout would have electoral consequences with many researchers arguing that higher turnout would benefit the Democratic Party (Citrin et al. 2003; Hansford and Gomez 2010; Hill 2010; Knack and White 1998; Martinez and Gill 2005). Beyond the partisan effects of turnout, turnout may influence the chances of interest group capture (Anzia 2011) or alter the distribution of constituency services and public goods

and affect descriptive representation (Hajnal and Trounstein 2005, Schwartz 1987). In short, inequality in voter turnout is likely related to unequal representation and has tangible policy outcomes.

A central component of dominant theories of voter participation is that close electoral competition should lead to increased participation because of the increased probability of the voter being pivotal in the election outcome. Moreover, because of the habitual nature of voter participation and evidence that the effect of competition should be concentrated among low-propensity voters (Blais, Young, and Lapp 2000) we might expect to see close elections diminish the gap in turnout between regular and non-regular voters. In this paper, we conduct the most thorough review to date of the available observational data on aggregate election outcomes and of survey questions and find no convincing causal evidence for a relationship between the closeness of an election and voter turnout. Then we conduct a field-experiment in the aftermath of a tied election to test the effect of the electoral closeness on voter behavior. We find no consistent evidence that the closeness of elections spurs higher turnout. Furthermore, we find no evidence of an effect among low-propensity voters. If anything, the closeness of elections increases the participation of already frequent participators – potentially exacerbating the gaps in participation between groups. We conclude that if policy makers hope to increase participation among nonvoters and citizens in general, they should focus on the costs of voting, civic duty, and the non-political benefits of voting because electoral competition appears not to have a leveling effect.

The closeness of elections could theoretically lead to increased turnout through two different mechanisms: individual voter psychology and the heightened activity of campaigns and media associated with close elections (Blais 2000, Cox and Munger, 1989). Here we focus on the former. The effects of media and campaigns on voter turnout are thoroughly studied. However, for a variety of reasons that we will explore in this paper, we still do not understand the effect of electoral closeness on voter psychology: do close elections increase the individual incentive to participate? Isolating the effect of closeness on voter psychology is paramount for determining how public policy can increase voter turnout. If turnout can be

affected by media and campaign functions, these activities can, of course, be increased in the absence of close elections. However, if close elections increase turnout by convincing the voter that her vote is more valuable, than the public policy solution to increasing voter turnout are structural reforms that encourage closer elections. In this paper, we undertake a significant test of the psychological mechanism of the effect of close elections.

We proceed by examining the role of electoral competition in traditional models of voter participation. Then, we examine the relationships among competition, voter turnout, and equality. We then examine the previous attempts to empirically establish a causal relationship between competition and turnout. Finally, we provide our own tests of this relationship. We close by arguing for an altered focus in voter participation research.

### **Electoral Competition and Voter Turnout**

In their classic theory, Riker and Ordeshook (1968) attempt to solve the paradox of voter turnout (Downs 1957) by asserting that citizens receive utility from the act of voting itself. In their well-known model, an individual's decision to vote is influenced by four factors: her probability of casting a decisive vote ( $P$ ), the value she would derive from her preferred candidate winning over that candidate's closest competitor ( $B$ ), her utility derived from the act of voting itself ( $D$ ), and the cost of voting ( $C$ ). Therefore, a citizen's returns from voting ( $R$ ) can be written as follows:

$$(1) \quad R = PB + D - C,$$

and a citizen will vote when

$$(2) \quad PB + D > C.$$

This model provides the basis for much of the current understanding of the psychological processes causing voter turnout. When researchers, pundits, and practitioners attempt to explain turnout, they often refer to  $P$  and  $B$ , assuming that they are as important if not more important than  $C$  and  $D$ . For example,

after New York state ranked last in voter turnout in 2010, *The New York Times* asked political scientist Michael McDonald to diagnose the problem. “Mostly, I suspect that the uncompetitive elections is the main cause.”<sup>2</sup> However  $P$  is usually infinitesimal (Gelman, King, and Boscardin 1998), so changes to  $P$  should have little effect on turnout. As Schwartz (1987) aptly points out, “Saying that closeness increases the probability of being pivotal . . . is like saying that tall men are more likely than short men to bump their heads on the moon.”

Nevertheless, the widespread belief among political scientists is that electoral competition, and thus higher  $P$ , causally influences voter turnout. Classic texts have asserted this relationship: V. O. Key (1949) attributed low turnout in the South during the early 20<sup>th</sup> century to the lack of electoral competition in general elections and Anthony Downs (1957) famously stated that citizens have no rational reason to turn out in a one-sided election. Since then, numerous scholars have argued that electoral competition is the cure for low turnout (Kelley et al. 1967; Teixeira 1992; Wattenberg 2002; Franklin 2008) or the explanation for trends in turnout over time (Burnham 1969; Piven and Cloward 2000; Franklin 2004).

The theoretical strength and intuitive appeal of this model is demonstrated by the number of scholars that assume the influence of electoral closeness on voter psychology and subsequent turnout without actually empirically supporting what is, in fact, a strong assumption. This assumption is widespread in the literature on voter turnout. To provide a recent example, Arceneaux and Nickerson (2009) state that “When the race is close and many people care about the outcome, more people decide to vote relative to races in which general interest is low.” Of course, not all researchers assume a central importance of  $P$ . For example, Gerber, Green, and Larimer (2008) note that “Because the probability of casting a decisive vote in an election is typically infinitesimal, the calculus of voting boils down to the relative weight of  $C$  and

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<sup>2</sup> Sam Roberts. “New York State Ranks Last for Voter Turnout.” *The New York Times*. November 16, 2010.

*D*.” But the widespread assumption remains that closeness of an election influences turnout. Andre Blais writes that election closeness increasing turnout “is the most firmly established result in the literature. I cannot see how this finding could be wrong” (2006, p. 119).

To illustrate the dominance of this assumption, we reviewed all articles published on voter turnout since 1980 which appear in the three leading political science journals: *American Political Science Review*, *American Journal of Political Science*, and *Journal of Politics*. An article was included if the title indicated the study of voter turnout. After reading the papers, a research assistant recorded any clear appeals to *P*. There were 53 papers published since 1980 in these three journals which addressed the causes of voter turnout. Of those, more than half, 30, made a clear appeal to the importance of *P* or electoral competition. Political scientists continue to put electoral closeness at the center of models of turnout.

### **Electoral Competition and Inequality**

According to the dominant models of the psychology of voter turnout, electoral competition is causally connected to turnout. However, these models do not imply that closeness should have a uniform effect across all voters. In the Riker/Ordeshook model, because *P* is so small, closeness should be most influential when *D* is slightly smaller but almost equal to *C*. In this case, a voter is almost indifferent between voting and abstaining, and electoral closeness might be the deciding factor.<sup>3</sup> In other circumstances, when *D* is much larger than *C*, a citizen should regularly vote. When *C* is much larger than *D*

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<sup>3</sup>Blais, Young, and Lapp, 2000 find that perceptions of electoral closeness are most correlated with turnout for those individuals with a low sense of civic duty which is consistent with our assertion if the costs of voting are also low. Another circumstance leading to a strong role of *P* is one in which both *B* and *C* are low, as discussed by Aldrich (1993).

or when  $B$  is small, a citizen should regularly abstain. As such, we might expect low-political efficacy individuals, that is those with a low  $D$  or  $B$  term, to be unaffected by  $P$ .

Despite the theoretical reasons to believe that electoral competition might be inconsequential to citizens with low political-efficacy, many scholars have argued that electoral competition is centrally important to participation among those with low efficacy. Key forcefully argued that healthy party competition is necessary to increase political participation and power among the under-represented Southern poor of the 1930's (Key 1949). Subsequent scholars have focused on the closeness of electoral competition between parties as integral to increasing political power for those disengaged from the political system (for a review and critique of this literature, see Bartels 2008). E.E. Schattschneider argued for institutional reform in the way of responsible parties to increase electoral competition in order to achieve greater political representation and equality (Schattschneider 1960). Franklin (2004), in his cross-national study of voter turnout uses German election data to argue that closer elections motivate the less-regular, un-habitualized voters. Other scholars have indirectly made the connection between electoral competition and turnout among low-efficacy voters. For example, Teixeira (1992) notes that the turnout decline in the United States in the latter Twentieth Century was most prevalent “among those groups least likely to vote in the first place” (p. 104) and then goes on to prescribe more competitive electoral districts as a remedy for declining voter turnout. Piven and Cloward (2000) in their normative critique of the “demobilization” of the poor and racial minorities in the United States draw upon Schattschneider and Burnham (1965), among others, to argue that a decline in party competition caused demobilization. Wattenberg (2002) draws a similar connection in arguing that turnout could be increased by adopting an electoral system that increased perceptions that voters might affect election outcomes.

Despite these claims, there is little theoretical reason that electoral competition will improve equality in political representation through influencing the individual calculus of voting. As discussed, close elections may motivate citizens to vote who were otherwise indifferent between voting and abstention.

However, under-represented voters are consistent non-voters because  $C$  is significantly greater than  $D$ .

Electoral competition may mobilize a small subset of citizens to turn out, but it will not likely mobilize the truly under-represented. As a result, democratic competition may not provide the cure to political inequality as previous scholars have hoped.

### **Previous Attempts to Measure the Influence of Competition**

Despite the widespread belief that electoral competition influences voter turnout, the empirical evidence is weak. The existing literature asserts the importance of close elections but almost all suffer from one or more shortcoming. A portion of the literature asserts the importance of closeness without actually testing the claim. Observational studies suffer from the confounding of close elections or individual perceptions with other, unmeasured variables. Recognizing these problems, some scholars have turned to experiments, but, unfortunately, these studies, whether in the laboratory or field, often lack external validity. In short, despite the heavy scholarly attention to  $P$  and the normative concern about turnout, existing studies have yet to find a satisfactory way to measure the influence of close elections on voters. Using a multi-method approach, we harness unique and original data to overcome the limitations of previous studies and address the direct influence of closeness on behavior.

While many studies report higher turnout during close elections (Barzel and Silberberg 1973; Kim et al. 1975) other studies find no such correlation (Ferejohn and Fiorina 1975; Matsusaka 1993). More importantly, even if electoral competitiveness is correlated with turnout, these measures tell us little about the causal effect of closeness on turnout through individual voter psychology. Campaign activity (Patterson and Caldiera 1983; Schachar and Nalebuff 1999; Hill and McKee 2005, Gimpel, Kaufmann, and Pearson-Merkowitz 2007), elite mobilization (Cox and Munger 1989), campaign donations (Ansolabehere and Snyder 2000; Erikson and Palfrey 2000), and media coverage (Clarke and Evans 1983; Jackson 1996) are significantly greater during competitive elections and in competitive states which may increase turnout even

if individual citizens are unaware of or uninfluenced by the closeness of the election. Moreover, higher turnout may lead to increased electoral competitiveness instead of competitiveness driving turnout. In fact, Panagopoulos and Green (2008) demonstrate that experimental increases in turnout may lead to tighter election outcomes. Both of these confounding factors will lead the already small correlations between competitiveness and turnout to overstate the true effect of  $P$ . We will discuss these issues and provide our own observational estimates in the next section.

Some studies attempt to assess the independent psychological effect of electoral closeness over and above campaign activity by including closeness and campaign spending in the same model. The effect of closeness in these models is small (Seidle and Miller 1976; Tucker 1986; Cox and Munger 1989; Berch 1993), mixed (Matusaka and Palda 1993), or zero (Jackson 1996), and this multivariate approach does not overcome the problems of endogeneity. First, this approach does not account for the possibility of reverse causation, as discussed above. Second, the addition of control variables is unlikely to account for all differences between close and uncompetitive elections. One cannot rule out unobserved heterogeneity without making untestable assumptions.

In addition to these aggregate observational studies, researchers have attempted to assess the effects of close elections at the individual level using survey data. Respondents who think an upcoming election will be close are more likely to vote (Riker and Ordeshook 1968; Aldrich 1976; Blais et al. 2000), suggesting that a high perception of  $P$  may drive citizens to the polls. Again, however, these correlations likely overestimate the true causal effect of electoral competition on turnout. Omitted variables such as citizens' sense of civic duty may influence both turnout and respondents' propensity to say an election will be close. Moreover, the act of voting itself, may lead voters to increase their perceptions of closeness to avoid cognitive dissonance: since the act of voting is costly, voters might convince themselves that their vote is meaningful in order to justify the expense. For these reasons, we might expect a positive correlation between citizens' perceptions and turnout even if electoral competition does not drive turnout.

Lab experiments provide an additional opportunity to test for the effects of close elections. Ansolabehere and Iyengar (1994) randomly presented prospective voters with different poll results before the 1992 general election. However, subjects receiving news of a competitive race were no more likely to express an intention to vote than those receiving news of a one-sided race. Later lab experiments report that closeness increases turnout (Levine and Palfrey 2007; Duffy and Tavits 2008), but these results arise from artificial, non-political settings where the financial costs and benefits of casting a “vote” are explicitly laid out for subjects. These results show that experimental subjects can make arithmetic calculations and respond to clear financial incentives, but they suffer from a lack of external validity when speaking to causes of voter turnout in real-world elections.<sup>4</sup>

Lastly, field experiments provide a final opportunity to test for the effects of close elections. Experimental treatments telling prospective voters that an upcoming election will be close are equally if not less effective than other treatments in mobilizing voters (Gerber and Green 2000; Bennion 2005; Dale and Strauss 2009). These results suggest that priming electoral competition is not particularly effective in generating turnout, but they should be interpreted cautiously. First, in one study (Bennion 2005), the “close election” treatment was paired against a “civic duty” treatment, and both treatments may have their own independent effect. Also, each experiment took place in a non-competitive election, so the “close election” treatments may have lacked credibility among subjects. In this respect, field experiments suffer from problems of external validity similar to those of lab experiments; we do not know how voters will respond in the real-world situation of a close election.

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<sup>4</sup> Recent lab experiments by Blais, et al (2011), which arguably are designed to better approximate voter decision made during actual elections, have shown that most subjects *do not* abstain when it is rational for them to do so.

The ideal opportunity to test for the effect of electoral closeness on voter psychology arises when an election is known to be close beforehand. In this circumstance, a field experiment could credibly inform citizens of the close election and test for the effects on participation. The best circumstance being when a researcher could, unrealistically, know that an election would end in a tie or was decided by a single vote, thereby guaranteeing that each voter will be pivotal. To our knowledge, the best opportunity in this regard is a special election held after an exact tie. Later in the paper, we exploit precisely this rare opportunity. First, we turn to a reassessment and extension of the existing observational data to see if any evidence can be found for an effect of closeness on turnout.

### **Observational Evidence: Real World Turnout is Not Responsive to Electoral Competition**

U.S. presidential elections provide an opportunity to test for the effects of electoral closeness because the Electoral College effectively weights some states more than others. In the same race, a voter in Ohio may have a significantly higher pivotal probability than a voter in Wyoming. Several studies exploit this variation to argue that electoral competitiveness increases political participation (Kim et al. 1975; Hill and McKee 2005; Wolak 2006; Lipsitz 2008). However, the observed correlations between closeness and turnout are typically small and, of course, might not result from a direct effect of closeness on voter psychology. Increased campaign activity or other omitted variables may explain the results of these studies. Illustrating the substantively small size of this effect, Gerber et al. (2009) find that turnout is only 2 percentage points higher in battleground states compared to spectator states. Of course, this small difference could easily be the result of campaign activity alone considering the intensity of campaigning in these states (Shaw 1999, Shaw 2006). Moreover, to assess the relative size of this 2 percent effect, Gerber et al. (2009) point out that other election effects “dwarf the effect of being in a pivotal (battleground) state” (p. 4).

In Table 1, we present our own analysis of turnout in Presidential elections from 1960 to 2008.<sup>5</sup> We regress turnout in every state-election on the electoral competitiveness between the Republican and Democratic candidates in that state. The independent variable “closeness” is coded from 0 to 1, where 1 represents a 50/50 split between the two major candidates and where 0 represents a completely uncompetitive state-year where one candidate garners all of the electoral support. A state between-effects model shows no difference in average turnout between competitive and uncompetitive states, and a state fixed-effects model shows a substantively small correlation between closeness and turnout within states. According to the within-state analysis, a state’s shift from an uncompetitive 60/40 scenario to a competitive 50/50 scenario would correspond with a meager 1 percentage point increase in voter turnout. The between-state effect is even weaker. Moving from a 60/40 state to a 50/50 state corresponds with a tiny 0.4 percentage point increase in turnout.

[Table 1]

Previous researchers have cut the data in different ways to argue that electoral competitiveness drives turnout. This simple analysis demonstrates that the effect of electoral competitiveness is limited. Consistent with previous findings, competition accounts for a tiny percentage of the overall variation in turnout (Gerber et al. 2009). These 0.4 and 1 percentage point effects are smaller than what would be expected if *P* makes a substantial contribution to voting decisions. Moreover, even these small effects likely overstate the causal effect of electoral competition on turnout because of omitted variables and reverse causation. Higher turnout in competitive states may be driven by increased campaign activity, media coverage, or other factors unrelated to citizens’ perceptions. One way to avoid these difficulties is to use survey evidence to directly measure perceptions of closeness. In the next section, we turn to the survey evidence, examine the shortcomings and attempt to better estimate the effect of close elections.

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<sup>5</sup>Data obtained from David Leip’s U.S. Election Atlas <http://www.uselectionatlas.org/>.

## Survey Evidence: Perceptions of Closeness are Not Meaningfully Correlated with Turnout

The first and most cited evidence that close elections influence turnout comes from survey data. Citizens who say that an upcoming election will be close are more likely to vote (Riker and Ordeshook 1968; Aldrich 1976; Blais et al. 2000). For every U.S. presidential election since 1952, the American National Election Study (ANES) has asked respondents to gauge whether the upcoming election will be close. Analyzing these surveys in 1952, 1956, and 1960, Riker and Ordeshook (1968) and with the same years and 1972 Aldrich (1976) show that the prediction of a close election is correlated with turnout, concluding that perceptions of closeness causally influence individuals' decisions to turn out. However, the evidence deserves further scrutiny. The relationships reported by Riker and Ordeshook (1968) and Aldrich (1976) were not subject to the rigors of multivariate regression analysis, raising the possibility that omitted variables are driving the results. Additionally, as discussed above, the analyses might be plagued by reverse causality.

In table 2, we analyze all ANES surveys in presidential election years between 1952 and 2008. Like the previous studies, we find that the prediction of a close election is strongly correlated with turnout. Regressing turnout on a dummy variable, *Close* (1 = respondent thinks election will be close, 0 = not close), and election fixed effects, we find that turnout is 7 percentage points higher among those who predict a close election (Column 1). However, the effect shrinks to 4 percentage points when we control for each respondent's turnout in the previous presidential election (Column 2), and it shrinks further to 2 percentage points when we include additional demographic controls such as age, education, and political knowledge (Column 3).

Even these smaller estimates of the effect of  $P$  may not represent the true effect. Turnout itself could influence perceptions of  $P$  through the well-understood process of cognitive dissonance (Festinger 1957). Voters may justify their decisions to turn out by convincing themselves that their vote matters. In this way, voters may turn out for reasons unrelated to  $P$ , but increase their stated perceptions of closeness to avoid cognitive dissonance. Our analysis in Table 2 is consistent with this pattern of dissonance. In column

4, we conduct a placebo test, regressing each respondent's turnout in the previous presidential election on their perception of closeness in the current election. If dissonance is at work, we would expect that perceptions of closeness in the current election are related to higher levels of turnout in previous elections, since voters may justify their past behavior by adjusting their current perceptions. Consistent with this hypothesis, voters who think the upcoming election will be close are 7 percentage points more likely to have voted in the previous presidential election. Moreover, this placebo effect is the same size as the observed effect in column 1, where turnout in the current election is regressed on perceptions of closeness in the current election, suggesting that most, if not all, of the previously observed effects are driven by omitted variables, reverse causation, and cognitive dissonance. The observed patterns in Table 2 are unchanged if we restrict our analysis to only those years analyzed by Riker and Ordeshook (1968) or Aldrich (1976), indicating that the small effect of  $P$  on turnout is not simply a result of the expanded time-frame of our analysis.

### **Field Experiment during a Tied Election**

Using aggregate data, we have demonstrated that the effect of closeness on voter turnout is likely small and, considering the likely influence of campaign and media effects on turnout, we are left with little room for an effect of  $P$  on the individual calculus of voting. Re-examining the survey data, we have again shown that the effect of  $P$  is likely small and difficult to separate from explanations of reverse causality. We now turn to a very rare circumstance that allowed us to directly test the effect of the psychological impact of close elections in an actual election. The 2010 November election for Massachusetts State House in the 6<sup>th</sup> Worcester District ended in a dead heat. After a series of recounts and a court case, Geraldo Alicea, the Democratic candidate, and Peter Durant, the Republican candidate, had each received exactly 6,587 votes - an exact tie. A special election was scheduled for May 10, 2011, and the race was likely to be close again.

The same candidates who had just produced the tie would square off again with the same voters<sup>6</sup>. If closeness could ever directly motivate turnout in a large election, it would do so in the May special election. We took advantage of this situation to directly test  $P$  by manipulating voters' knowledge of closeness.

By experimentally manipulating the awareness of  $P$  in an actual election, we are subjecting the effect closeness to a test that avoids many of the pitfalls that have plagued previous research because we are able to avoid the contaminating effects of reverse causation and omitted variables, such as campaign effects. Moreover, because  $P$  is credibly approaching its theoretical maximum value that is achieved only in an exact tie, we are allowing for the greatest chance of observing closeness impacting the decision to vote.

Despite the interesting circumstances of the election, the average citizen in the district was relatively uninformed about the race. Conducting a brief phone survey of registered voters in the three days leading up to the election, we found that only 64% knew that a special election was coming up on May 10<sup>th</sup>, only 52% could name both candidates, and only 41% knew that the previous election had ended in a tie. These figures likely overestimate the true level of knowledge because the surveys only involved registered voters who answered the phone and agreed to answer questions about an upcoming election. Walking around the district on Election Day, we saw little campaign material aside from the polling places and the two candidates' headquarters. Subsequent interviews with the candidates and campaign workers for both campaigns revealed that the campaigns had focused their efforts on likely voters, targeting based on previous voter turnout, party registration, ethnicity, and expressed support.<sup>7</sup> One of the campaigns even

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<sup>6</sup>Two independent candidates entered the race, but this did not change the fact that the special election would also be a tight contest between Alicea and Durant.

<sup>7</sup>The Alicea campaign concentrated disproportionately on Latino voters, which may have had some residual effect in targeting low-propensity voters, but this was not the explicit strategy of the campaign.

admitted to performing no voter contact in several of towns in the district. Furthermore, neither candidate directly mentioned the tied election in their campaign materials (author interviews, July and August 2011).

In the days prior to the election, we placed phone calls to registered voters to remind them about the special election and, for some, about the exact tie in the previous election and that their vote had a relatively high chance of being pivotal. As it turned out, the special election was also an extremely tight race. Durant defeated Alicea by only 56 votes, so our “pivotal” treatment was credible in informing citizens that the race would be close. At first glance, it appears that citizens in the district failed to respond to the unique circumstances of the extremely tight election. Only 20% of the district’s residents turned out in the special election, compared to 33% for the November election. Moreover, 16% of the special election voters supported an independent candidate who had little chance to win. These observations show little support for the claim that voters are motivated by electoral competition, but our experimental design allows us to explicitly test the causal effect of closeness on the individual calculus of voting.

### Experimental Design

Given the rare circumstances around the election and the fact that most citizens were unaware of the tie, the special election in the 6<sup>th</sup> Worcester District provided the ideal opportunity to test for the effects of electoral competition on voter turnout. Some individuals in the district would receive a simple reminder that a special election is coming up, while others would receive the same reminder and also be informed about the tie and the unusually high chance that their vote could be pivotal. If the “pivotal” treatment mobilized more voters than the “reminder” treatment, then we would conclude that knowledge of electoral closeness mobilized voters. We applied the treatment through phone calls during the three days leading up to the election. The phone study allowed us to first gauge each respondent’s level of knowledge and then provide an experimental treatment. Phone experiments have been shown to substantially mobilize voters if conducted in a certain fashion.(Nickerson 2006, 2007). For this reason, our calls were conducted in a

fashion that included the elements of Nickerson (2006) and Nickerson (2007) that have been demonstrated to increase turnout: we recruited student volunteers to make the calls and we provided a script but instructed the callers to convey the appropriate information in a casual, conversational style without reading directly from the page.

We obtained the list of registered voters in the district and their phone numbers from Catalist, a for-profit political data vendor. Catalist provided us with 19,327 registered voters in the district for whom they could connect with a phone number. There were 9,318 unique phone numbers in the list. To check the quality of numbers, we used Call Fire, a robo-call service, to identify and remove eighty-four invalid phone numbers from our list. Then, we removed 369 phone numbers that had more than four registered voters associated with it to make it easier for our callers to identify the recipient of the call. This strategy of removing large households has been previously employed to improve both the administration and the interpretation of field experiments (Nickerson 2008; Huber, Gerber, and Washington 2010; Nickerson and Rogers 2010). For the remaining 8,865 phone numbers, we randomly assigned them into one of four conditions. Phone numbers were stratified according to their pre-treatment characteristics, and randomization was conducted within each stratum. We provide a detailed account of the randomization procedure in the Supporting Information.

Two thousand, nine hundred fifty-five phone numbers, one-third of the population, were randomly assigned into the “reminder” condition. In this condition, a caller would introduce herself and identify the recipient of the call. If a registered voter did not answer the phone, then the caller would try to ask for one of the voters associated with that phone number. In a casual manner, the caller would ask whether the recipient knew about the special election coming up. If the recipient claimed to know, then the caller would ask for the day that the election will take place. These two questions allowed us to identify the voters’ prior knowledge about the election, which was used in our subsequent analysis. Then, the caller would simply remind the recipient that the election would be on May 10<sup>th</sup> to fill a seat in the State House. If the recipient

asked for additional information, callers were permitted to provide the names and parties of the two candidates. However, for any additional questions, the recipients were referred to the website of the Massachusetts Secretary of the Commonwealth. The complete script given to callers is available in the Supporting Information.

Another 2,955 phone numbers were randomly assigned to the “pivotal” condition. This treatment was identical to the “reminder” condition with one exception. At the end of the call, the caller would also inform the recipient about the reason for the special election, by saying: “The reason that there is a special election is that the last election ended in an exact tie. Had one more or one less person voted in the last election, your candidate would have won. The special election on Tuesday is likely to be close again, so there is a high chance that your vote could make a difference.” The response of voters to this script, which our callers recorded, indicated a successful manipulation. For example, voters often responded by assuring the caller that they had voted despite personal hardship or relaying an anecdote about a friend or relative that had not voted and proclaiming that this had made the difference in the election.

A separate 296 phone numbers, just one-thirtieth of the population, were randomly assigned to a survey condition. This condition allowed us to better gauge the extent of political knowledge among our population. As in the “reminder” and “pivotal” conditions, we asked recipients whether they knew about the upcoming election and whether they knew the date. Then, we also asked whether they could name the candidate or whether they knew the reason for the special election. As mentioned previously, only 41% of respondents knew that the previous election ended in a tie, suggesting that the majority of our sample was unaware of the unusually high chance that their vote could make a difference. Lastly, the remaining 2,659 phone numbers were assigned to a “no contact” condition, where no phone call would be attempted. After numbers were assigned to conditions, they were randomly sorted before given to callers. Therefore, the timing of the call was also random and each caller had approximately the same proportion of reminder, pivotal, and survey calls. Prior to randomization, we stratified the voters on previous voter turnout and

other characteristics. A complete description of our randomization procedures can be found in the Supporting Information.

Our experiment allows us to assess the causal effect of being informed about electoral competition on turnout in the special election. Lists of special election voters were obtained from the 5 different town clerks in the district and matched to the list of treated individuals and treated phone numbers. 97% of our subjects were successfully matched to the towns' records of registered voters. Importantly, rates of attrition are the same for voters in both treatment groups. The difference in turnout between the pivotal group and the reminder group represents the causal effect of interest, because the conditions were randomly assigned, and the only difference between the two conditions is the information we provided about electoral closeness. If closeness has a substantial causal effect on the individual calculus of voting, then we expect a large difference. However, if the effect is negligible, as suggested by our observational and survey estimates, then we expect little difference between the pivotal and reminder groups. Our experiment also allows us to test the effects of our manipulation across voters with different voting propensities, thereby testing whether being informed of electoral closeness had the greatest effect on non-regular voters and thereby diminished inequalities in turnout or the greatest effect on regular voters and thereby increased inequalities in turnout.

### Estimation strategy

Randomization and stratification, in addition to the advantage of avoiding imbalance between treatment groups, allows more precise estimates, because we can estimate the effect of the pivotal treatment versus the reminder treatment with strata fixed effects. Looking at all individuals  $i$ , in all strata  $j$ , we employ OLS<sup>8</sup> to estimate the following equation:

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<sup>8</sup> Since the dependent variable, turnout, is dichotomous, we can also estimate the treatment effect using logit or probit. In all cases, marginal effects from logit or probit are nearly identical to our OLS

$$(3) \quad Y_{ij} = \beta * Pivotal_{ij} + \gamma_j + \varepsilon_{ij}$$

$Y_{ij}$  is a dummy variable indicating whether the individual turned out in the May 10<sup>th</sup> special election.  $Pivotal_{ij}$  is a dummy variable, taking a value of 1 if that individual is in the “pivotal” condition and a value of 0 if the individual is in the “reminder” condition. Individuals in the survey or no contact conditions are omitted from the subsequent analysis.  $\gamma_j$  represents a specific effect for each stratum. Some strata are likely to turn out at high rates, while others will not. This procedure removes that source of variation and estimates the effect of the pivotal treatment within strata. Where applicable, standard errors will be clustered by phone number, since that is the level at which individuals and households are treated.

## Results

As described earlier, we randomly assigned 5,910 phone numbers to receive either the “reminder” or the “pivotal” treatment during the three day period preceding the special election. Due to time and labor limitations, we were only able to call 5,157 (87%) of those numbers. Because the order of call attempts was randomly assigned, similar numbers of attempts were made to phone numbers in the reminder and pivotal conditions, 2,599 and 2,558 respectively. Of those attempts, only 1,021 (20%) resulted in complete treatments. This figure is comparable to response rates in previous phone experiments (Nickerson 2006). Importantly, the response rates were similar for the reminder and pivotal conditions, 20.3% and 19.3% respectively.

In Table 3, we estimate the effect of the pivotal treatment for 5 different subsets of experimental subjects. The Supporting Information provides the mean turnout rate for each relevant subset of

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coefficients. Moreover, OLS is superior to other estimators because linearity is guaranteed because all independent variables are also dichotomous (this does not imply that treatment effects are homogeneous) and OLS overcomes the incidental parameters problem which would bias the results of other estimators.

experimental subjects, showing that simple differences-in-means yield the same results as our strata fixed-effects models. Row 1 shows our intention-to-treat estimate, the average effect of being assigned to the pivotal treatment compared to the reminder treatment. The estimate of .006 indicates that after removing strata fixed effects, subjects in the pivotal treatment group were 0.6 percentage points more likely to vote in the special election compared to those assigned to the reminder group. This effect is substantively tiny and statistically insignificant, suggesting that a pivotal treatment is not an effective method for increasing aggregate turnout. In Row 2, we isolate those subjects who answered the phone and received a treatment. According to this estimate, the pivotal treatment increased turnout among this subgroup by 1.2%, but this estimate is not statistically significant. In Row 3, we hone in on just those individuals who were directly contacted and who were uninformed about the upcoming election. There are only 317 such individuals so this estimate is necessarily imprecise. Here, the estimated effect of the pivotal treatment is larger, 5.2%, but still statistically insignificant.

[Table 3]

We cannot know whether some of these positive coefficients have arisen by chance alone or through some real effect of our pivotal treatment. Our observed effects are not larger than we would expect by chance and they are smaller than we would expect if closeness really does weigh as heavily on the calculus of voting as the theoretical model suggests. Even in this case of an extremely close election, voters did not turn out at high rates, and informing them about closeness had no statistically significant mobilizing effect. However, our estimate of the average effect on uninformed voters is not zero, so we can look more closely at the effect on subsets of voters to see if closeness has a mobilizing effect on habitual non-voters.

#### The effect on infrequent voters

As previously discussed, electoral closeness has frequently been argued as a condition related to the turnout of low-participatory, often under-represented, individuals. Our experiment provides a rare

opportunity test this claim by examining the differential effects of the treatment on regular and non-regular voters. If close elections can help to equalize turnout, then we should see a stronger effect of the treatment on voters have voted infrequently in previous elections.

In rows 4 and 5 of Table 3, we estimate the effect of the pivotal treatment separately for those who voted in 3 or more of the 9 most recent elections and for those who voted in 2 or less.<sup>9</sup> We choose this arbitrary cutoff because it splits our sample roughly in half, but Figure 1 shows the effect across all levels of vote history. We find a large, statistically significant effect for regular voters (18.5%) but no effect for infrequent voters. A single interactive model yields the same result. The treatment effect is zero for those who have not previously voted, but the interaction between the pivotal treatment and previous turnout is large and statistically significant. The size of the effect for regular voters is quite large compared to the other experimental manipulations, but not unprecedented (e.g. Michelson, Bedolla, and Green 2008; Green and Gerber 2008). Of course, the experimental manipulation used here is unprecedented – never before have experimenters sent a credible signal that  $P$  is likely to be high. But more important than the exact size of the point estimate, which is, of course, subject to statistical uncertainty and may be higher or lower, is that the effect for regular voters is statistically and substantively larger than the effect for non-regular voters.

Figure 1 further demonstrates the variation of the pivotal treatment effect across prior levels of voter turnout. The figure presents separate kernel regressions of turnout in the special election across the probability of turnout in the 9 previous elections for uninformed individuals in the pivotal and reminder groups. There is no effect of the pivotal treatment for infrequent voters, but there is a sizable effect for frequent voters.

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<sup>9</sup> The 9 recent elections are the 2010 general; 2009 special; 2008 general, primary, and special; 2007 special; and 2006 general, primary, and special elections.

Because the campaigns focused their contact efforts on frequent voters, low-frequency voters should have received our treatment with less information about the campaign, relative to the frequent voters and, as such, low frequency voters should have been more susceptible to the treatment. That they showed even less response to the treatment than the high-frequency voters underscores the ineffectiveness of highly competitive elections in mobilizing habitual non-voters.

[Figure 1]

While closeness may mobilize a small subset of individuals, it has virtually no mobilizing effect for the underrepresented citizens who vote infrequently. In fact, the size of the treatment effect on regular voters was large enough to open a substantively significant gap between the turnout of regular voters and those who vote infrequently. The effect of the additional turnout stimulated by the treatment condition reaches an average of over 18 percentage points among the most frequent voters. This finding suggests that, if anything, close elections significantly widen the turnout gap between the politically active and the politically under-represented.

This result is consistent with our alternative prediction that closeness should influence those who were otherwise indifferent between voting and abstention. Habitual non-voters lack the interest or information to vote, and any information about a tight election will not push them over the brink. Increased electoral competition may exacerbate, rather than reduce, gaps in political representation.

### Placebo/Balance Tests

Although we find minimal effects of the pivotal treatment compared to the reminder condition, we might worry that a real effect has been masked or counteracted by unobserved differences between the two experimental groups. Random assignment guarantees that the two groups are asymptotically identical, but for our finite sample size, small differences may arise by chance. Our stratified randomization procedure ensured greater balance between treatment conditions for certain variables, but small differences could arise

for other variables. Moreover, the subset of households or individuals who answered the phone and received a treatment could be slightly different by chance in the two conditions.

To demonstrate that these concerns do not plague experimental results, researchers often present the difference between the two experimental conditions for a number of pre-treatment variables. Since our analysis first requires the removal of strata fixed-effects, a simple difference-in-means alone would not assure us that our estimates are valid. Instead, we present a number of placebo tests, replicating our previous estimates but substituting our dependent variable for numerous pre-treatment variables. Regressing a pre-treatment variable on a dummy for the pivotal treatment and strata fixed-effects, we can assess the extent of imbalance between treatment conditions for that particular variable. The Supporting Information presents the results of 45 placebo tests. Each cell in the table presents the coefficient on the pivotal treatment and the corresponding standard error for a particular sample and for a particular placebo outcome. As in Table 3, standard errors are clustered by phone number where appropriate.

There are few meaningful differences in pre-treatment characteristics between treatment conditions. Individuals in the two experimental groups were similar in terms of their turnout in the 2010 general election, the 2009 special election, the 2008 primary election, Hispanic racial identity, age, party registration, absentee voting in the special election, and whether we were able to match them to the town voter files. If anything, uninformed individuals in the pivotal group were, by chance, more likely to vote in previous elections, suggesting that our previous estimates may overestimate the effect of the pivotal treatment. Only 1 out of 45 coefficients is statistically significant at the 5% level. Contacted, uninformed individuals in the pivotal condition were less likely to be Hispanic than their counterparts in the reminder condition. Since only 317 individuals fall into this category, we expect some imbalances to arise by chance. However, this difference does not bias our experimental results. None of our results in Table 3 are significantly changed by the inclusion or exclusion of pre-treatment controls. These placebo tests confirm that subjects in the

pivotal and reminder conditions are comparable to each other and only differed in the extent to which we informed them about the closeness of the upcoming election.

## **Discussion and Conclusion**

In this paper, we assess the psychological effect of electoral competition on turnout. By employing several approaches, we hope to overcome the difficulties associated with studying the effects of close elections on turnout and assess the impact on inequalities in turnout. We examine closeness in an aggregate context to search for average effects that are free of dissonance-induced reverse causality on the individual level; we examine closeness using survey measures, accounting for potential confounding variables and reverse causality; and, finally, we identify and exploit a rare opportunity to credibly test for the effect of closeness in the aftermath of a tied election for a major political office. This last test exploits such a rare event that this test is unlikely to be replicable.<sup>10</sup> Through all these tests, we find little evidence that the closeness of elections motivates voters. Moreover, if it does motivate voters, it is those that already have a strong commitment to the act of voting.

Of course, we also leave many unanswered questions. To measure the overall effect of closeness on voter turnout, including both individual psychology and other effects, we would have to manipulate of the actual closeness of elections. Given the likely impossibility of such a design, we test the effect of closeness by manipulating voter knowledge of closeness in an election that is credibly close. This design allows us to separate the direct effect of electoral competition from the effects of campaigns, media, and other omitted variables. In this way, while this study does not actually measure the overall effect of close

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<sup>10</sup>Muligan and Hunter (2003) analyzed all contested state legislative elections between 1968 and 1989. In 40,036 state elections, two ended in a tie – one of which resulted in another election. In the 16,577 contested federal elections between 1898 and 1992, one ended in a tie.

elections, it isolates the effects of close elections, at least in the short-term, on individual voter psychology. The long-term effects of close elections remain to be studied, although the between-effects model in Table 1 suggests that they are minimal. Also, we cannot say whether our results would hold in a more salient election, but the inability of competition to mobilize under-represented citizens in a large state legislative election casts doubt on its potential in other electoral settings as well. Despite these remaining questions, this study brings greater understanding to the efficacy of policy prescriptions attempting to address inequality in participation – electoral closeness, in itself, has little effect.

Our findings are consistent with literature that questions the usefulness of the classic models of voting. Some individuals like voting and others do not. However, turnout decisions are rarely influenced by an individual's perception that her vote could influence political outcomes. To the extent that these considerations do matter, the effect is concentrated among regular voters. Citizens who are not inclined to vote will not be mobilized in competitive elections, even in a rare case when their vote could make a difference.

While our findings challenge the usefulness of traditional models of turnout in which  $P$  factors into the calculus of voting, they cannot distinguish between other models which exclude  $P$ . Other models, often forgotten in recent scholarship, may prove useful in understanding turnout. For example, the minimax-regret model (Ferejohn and Fiorina 1974) posits that citizens may vote to avoid the dreadful outcome of a tied election in which she failed to vote. And Schwartz (1987) offers a model in which turnout per se leads to direct benefits for an individual or her community. Progress on this question will likely arise from careful testing of these models or from the identification of the forces that factor into  $D$  and  $C$ .

Close elections are certainly different from blowouts or uncontested elections, and future researchers will continue to identify such differences. For example, Fraga and Hersh (2011) find that voters are less sensitive to changes in the cost of voting when elections are close. Also, voters are more cognitively engaged in close elections (Kam and Utych 2011). Our results suggest that these findings and other such

effects of close elections are minimally motivated by individual voter psychology. Rather, they are likely driven by the heightened campaign activity, elite discourse, or media coverage during close elections. Moreover, these phenomena do little to mobilize under-represented voters.

Voting is possibly the single most important act of democracy, and competitive elections are fundamental to a functioning democracy (Dahl 1970). Even though citizens often fail to vote or pay close attention to politics, democratic theory maintains that they will mobilize when it matters (Dahl 1961). One such event when a responsive citizenry should mobilize is a close election. However, our results suggest that voters fail to consider the chances that their vote will be pivotal and therefore fail to participate when the stakes are high. These findings cast doubt on the ability of an apathetic electorate to hold their government accountable. If the habitual voters in society are unrepresentative of the larger population, this problem will not correct itself in a particularly competitive election.

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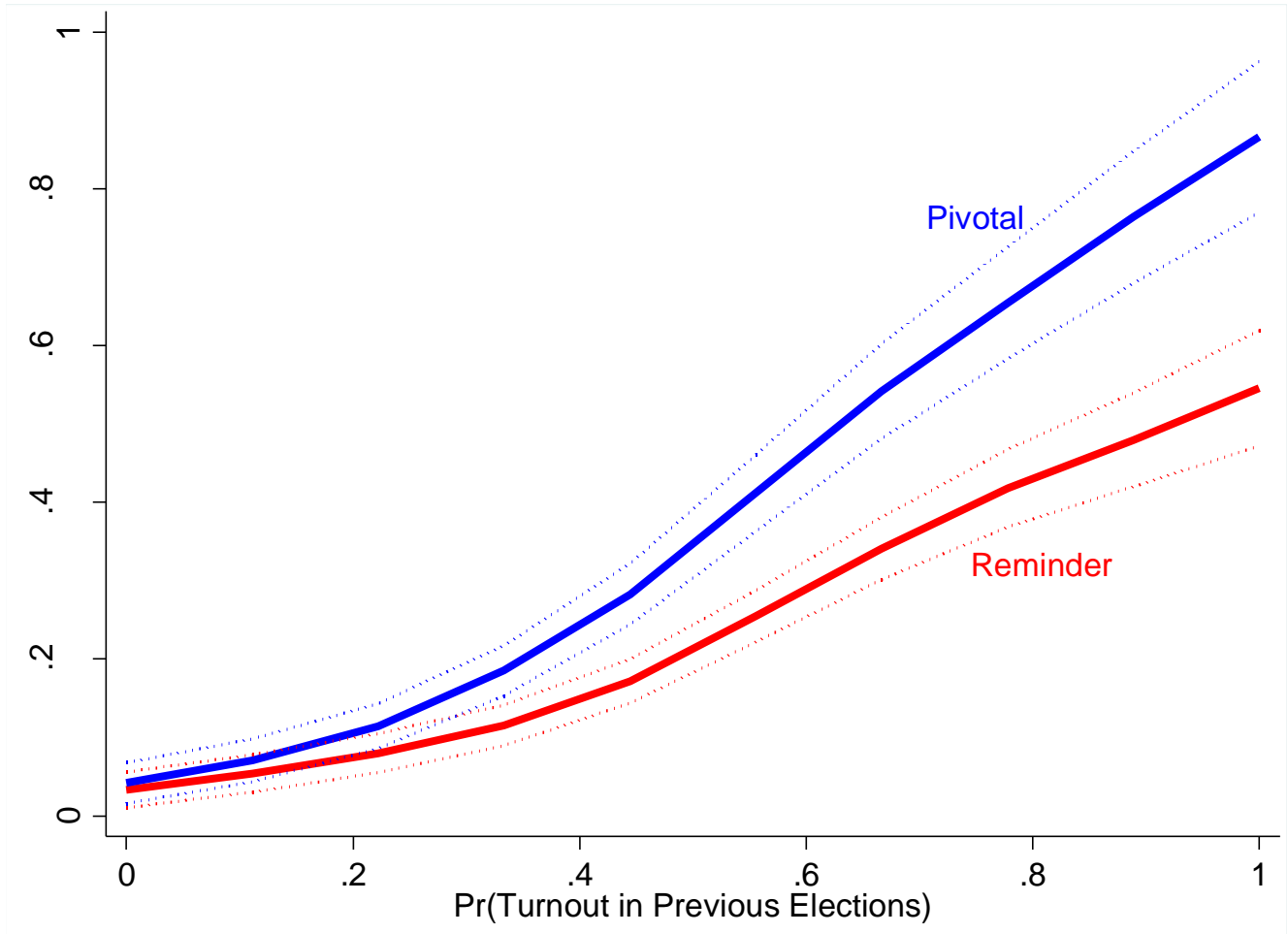
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Figure 1. Effect of Pivotal Treatment for Uninformed Subjects across Previous Turnout<sup>11</sup>



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<sup>11</sup> Solid lines represent kernel regressions of turnout in the special election across each respondent's probability of turnout in the previous nine elections. The sample includes only contacted subject who were initially uninformed about the upcoming special election. Dotted lines represent standard errors.

Table 1. Correlations between Competitiveness and Turnout across and within States<sup>12</sup>

	Between	Within
Closeness	.022 (.182)	.064 (.026)*
R-squared	<.001	.014
N-Obs	649	649
N-States	50	50

Standard errors in parentheses; \* p < .05

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<sup>12</sup> Both regressions analyze all state-years for every presidential election between 1960 and 2008.

“Closeness” is coded from 0 to 1, where 1 represents a 50-50 split between the Democratic and Republican candidates in that particular state-year, and 0 represents a hypothetical blowout, where one candidate garners all support. The “between” model (state between-effects) compares states that are continually competitive to those that are uncompetitive. This is algebraically identical to regressing the average turnout in each state on the average closeness in the state. The “within” model (state fixed-effects) compares competitive years within each state to uncompetitive years in that state.

Table 2. The Effect of Perceived Closeness on Turnout in Presidential Elections, ANES 1952-2008<sup>13</sup>

	DV = Turnout			Turnout, t-1
Close	.071 (.006)**	.039 (.005)**	.021 (.006)**	.067 (.006)**
Turnout, t-1		.483 (.006)**	.393 (.006)**	
Election Fixed Effects	X	X	X	X
Demographic Controls			X	
Constant	.621 (.005)**	.346 (.005)**	-.141 (.030)**	.569 (.005)**
N-Observations	27725	27725	25429	27725
N-Elections	15	15	15	15
R-squared	.005	.217	.272	.005
SER	.47	.42	.40	.45

Standard errors in parentheses; \* significant at 5%; \*\* significant at 1%

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<sup>13</sup> Each OLS regression includes all ANES respondents in presidential election years from 1952 to 2008. *Close* is a dummy variable indicating whether the respondent perceives the the upcoming election will be close. In the first three models, the dependent variable is reported turnout in that presidential election, and in the last model, the dependent variable is reported turnout in the previous presidential election. Demographic controls include white racial identity, gender, age, squared age, income, education, church attendance, party identity, and political knowledge.

Table 3. Experimental Effect of Pivotal Treatment<sup>14</sup>

	Estimate	S.E.	P-value	Obs
Intention-to-Treat	.006	.009	.491	11361
Contacted Individuals	.012	.029	.670	936
Contacted, Uninformed Individuals	.052	.043	.225	317
Contacted, Uninformed Individuals, voted in > 2 recent elections	.185	.088	.037	139
Contacted, Uninformed Individuals, voted in $\leq$ 2 recent elections	.018	.028	.525	178

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<sup>14</sup> The table presents the estimated effect of the pivotal treatment relative to the reminder treatment for five different subsets of experimental subjects. The estimates are OLS coefficients resulting from a regression of turnout on the pivotal treatment and strata fixed effects. Standard errors are clustered by phone number for the intention-to-treat estimate.