

Online Appendix

Pivotality and Turnout: Evidence from a Field Experiment in the Aftermath of a Tied Election

Ryan D. Enos and Anthony Fowler

Note on the Inclusion of Articles in the Literature Review of Articles on Turnout

We reviewed all articles published on voter turnout since 1980 which appear in five leading political science journals: *American Journal of Political Science*, *American Political Science Review*, *British Journal of Political Science*, *Journal of Politics*, and *Quarterly Journal of Political Science*. 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 the importance of electoral competition or pivotality.

Review of Previous Empirical Evidence on Pivotality and Turnout

Despite the widespread belief that electoral competition increases voter turnout, the empirical evidence is weak. The existing literature asserts the importance of close elections and pivotality but almost all previous studies suffer from one or more shortcoming. Observational studies suffer from the confounding of close elections or individual perceptions of closeness with other, unmeasured variables. Experimental studies, whether in the laboratory or field, lack external validity. In short, despite the heavy scholarly attention to P , existing studies have yet to find a satisfactory way to measure the influence of close elections on voters. Using a multi-method approach and harnessing unique and original data, we 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, Petrocik, and Enokson 1975) other studies find no such correlation (Ferejohn and Fiorina 1975; Matsusaka 1993). In U.S. presidential elections where the probabilities of pivotality vary

significantly across states, turnout is only minimally higher in battleground states compared to non-battleground states (Gerber et al. 2009). 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 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 weak correlations between competitiveness and turnout to overstate the true effect of P .

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.

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, Yound, and Lapp 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 may influence both turnout and respondents' propensity to say an election will be close. For example, campaign activity increases citizens' perceptions of competition (Bowler and Donovan 2011). 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 would expect a positive correlation between citizens' perceptions and turnout even if electoral competition does not drive turnout. See below for our own analysis of survey data, suggesting that these correlations are largely explained by omitted variables, reverse causation, and cognitive dissonance.

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.¹

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;

¹ 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.

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. 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. In the other two studies, the experiments took place in non-competitive elections, 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 would have responded to these interventions in the real-world situation of a close election.

A Reevaluation of Survey Data on Pivotality and Turnout

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 Aldrich (1976) – adding 1972 to the analysis – 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 A1, 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-known 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.

Table A1. Perceived Closeness and Turnout in Presidential Elections, ANES 1952-2008

| | 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 | |
| N-Observations | 27,725 | 27,725 | 25,429 | 27,725 |
| N-Elections | 15 | 15 | 15 | 15 |
| R-squared | .005 | .217 | .272 | .005 |
| SER | .47 | .42 | .40 | .45 |

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

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 that 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.

Voter Data

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 connected with 9,318 unique phone numbers. To verify the quality of numbers, we used a robo-call service and removed 84 invalid phone numbers from our list. Then, we removed 369 phone numbers that had more than four registered voters 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 (e.g., Nickerson 2008; Huber, Gerber, and Washington 2010; Nickerson and Rogers 2010).

After the election, lists of special election voters were obtained from the five different town clerks in the district and matched to the list of treated individuals and treated phone numbers. 97 percent of the subjects were successfully matched to the towns' records of registered voters. Importantly, rates of attrition are the same for voters in both treatment groups.

Randomization Procedures

In order to ensure greater balance between treatment groups and achieve greater precision with our subsequent estimates, we first stratified the population of phone numbers and then randomly assigned treatment conditions within each stratum. Based on previous literature, previous turnout would likely be a good predictor of turnout in the special election. Therefore, we divided phone numbers into three categories: (1) those where at least one person had voted in a low-salience special election in 2009, (2) those where at least one person had voted in the 2010 general election but no one voted in 2009, and (3) numbers where nobody voted in either election.

Next we divided phone numbers into three additional categories depending on the number of voters listed: (1) 1 registered voter at that number, (2) 2 registered voters, and (3) 3 or 4 voters. Remember that we omitted numbers with more than four voters.

Then, we decided that town of residence may be a good predictor of turnout in the special election. In this district, roughly one-third of the voters live in Charlton, one-third live in Southbridge, and one-third are distributed across three smaller towns, East Brookfield, Oxford, and Spencer. We divided the phone numbers into three categories accordingly. Post-election interviews with the candidates revealed that their voter contact strategies were heavily influenced by town-based geographic strategy (author interviews, July and August 2011), so stratifying on town was likely a good decision on our part.

Lastly, we stratified based on our data collected from robo-calling. To ensure that similar numbers of calls in each condition are answered, we separated the numbers where a live person had answered the phone from all others.

As a result of these categorizations, an individual phone number would fall into one of 54 unique categories (i.e. 1 registered voter, voted in 2010, lives in Southbridge, failed to answer robo-call). Since we expected many calls to be unanswered, we wanted to ensure that each stratum had at least 100 phone numbers. Most of the 54 categories had more, but we combined some of the smaller categories to meet this criterion. In 9 cases, we combined categories that shared everything in common except their answers to the robo-call. In 1 additional case, we also combined categories from different towns. This resulted in 44 unique strata with at least 100 phone numbers. Within each strata, we randomly assigned one-third to the reminder condition, one-third to the pivotal condition, one-thirtieth to the survey condition, and the remainder to the no contact condition; always rounding to the nearest integer when necessary.

This procedure generates near-perfect balance across previous turnout, town, household size, and propensity to answer the phone. Moreover, random assignment guarantees balance over all observable and unobservable pre-treatment characteristics in expectation. Nonetheless, there could be slight imbalances by chance between conditions over other variables of interest. To avoid this, we generated 500 different randomization schemes according to the protocol above and then selected the one scheme that generated the best balance on other variable such as party registration, Hispanic ethnicity, and turnout in other elections.

After numbers were assigned to conditions, they were randomly sorted before being 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.

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. By 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. Table A2 below 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 2, 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. None of the 45 coefficients are statistically significant at the 5% level. Moreover, none of our results in Table 1 are meaningfully 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.

Table A2. Placebo Tests²

| | Dependent Variable | | | | | | | | |
|--|--------------------|-----------------|-----------------|-----------------|-------------------|-----------------|-----------------|-----------------|-----------------|
| | Voted '10 | Voted '09 | Voted '08 | Hispanic | Age | Dem | Rep | Absentee | Unmatched |
| Intention-to-Treat | .000 (.006) | .003 (.004) | -.002 (.008) | -.004 (.006) | .195 (.345) | .001 (.010) | -.002 (.007) | -.003 (.003) | .001 (.004) |
| Contacted Individuals | .003 (.022) | -.001 (.018) | -.046 (.025) | -.004 (.011) | -.937 (1.072) | -.038 (.031) | .032 (.021) | -.006 (.010) | -.001 (.010) |
| Contacted, Uninformed Individuals | .057 (.042) | .029 (.026) | -.046 (.051) | -.036 (.020) | 1.646 (2.021) | -.042 (.056) | -.015 (.037) | -.001 (.010) | .002 (.015) |
| Contacted, Uninformed, voted in > 2 recent elections | .071 (.054) | .054 (.046) | -.007 (.047) | -.033 (.023) | -1.054 (2.743) | -.138 (.094) | .076 (.050) | .019 (.017) | .002 (.024) |
| Contacted, Uninformed, voted in ≤ 2 recent elections | .039 (.049) | .009 (.016) | -.067 (.077) | -.020 (.044) | 4.844 (2.816) | .056 (.074) | -.072 (.054) | -.014 (.010) | -.003 (.016) |

Standard errors in parentheses

² These results arise from 45 separate regressions which follow the same specification as those in table 3. The only exception is that the dependent variable is a pre-treatment variable, so we expect (and find) these coefficients to be close to zero.

Wording of Scripts Given to Callers

Reminder Treatment:

Hello, this is XXXX from Harvard University. We are calling registered voters to provide information about an upcoming election in your town.

Am I speaking with XXXXXXXXX?

Did you know that there is a special election coming up?

[IF YES] Do you know when it is?

We just want to remind you that there's a special election on Tuesday, May 10th to fill the seat of your representative in the Massachusetts State House. For more information on the election you can visit the website of the Secretary of the Commonwealth.

Goodbye

Pivotal Treatment:

Hello, this is XXXX from Harvard University. We are calling registered voters to provide information about an upcoming election in your town.

Am I speaking with XXXXXXXXX?

Did you know that there is a special election coming up?

[IF YES] Do you know when it is?

We just want to remind you that there's a special election on Tuesday, May 10th to fill the seat of your representative in the Massachusetts State House. For more information on the election you can visit the website of the Secretary of the Commonwealth.

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.

Goodbye

Table A3. Mean Turnout among each Subset of Experimental Subjects

| | Reminder | Pivotal | Obs | P-value |
|---|----------|---------|------|---------|
| Intention-to-Treat | .311 | .318 | 5771 | .467 |
| Contacted Individuals | .456 | .472 | 489 | .615 |
| Contacted, Uninformed Individuals | .180 | .237 | 167 | .209 |
| Contacted, Uninformed, voted in > 2 recent elections | .364 | .532 | 77 | .047 |
| Contacted, Uninformed, voted in \leq 2 recent elections | .022 | .033 | 90 | .652 |

References

- Aldrich, John. 1976. Some Problems in Testing Two Rational Models of Participation. *American Journal of Political Science* 20(4):713-733.
- Ansolabehere, Stephen and Shanto Iyengar. 1994. Of Horseshoes and Horse Races: Experimental Studies of the Impact of Poll Results on Electoral Behavior. *Political Communication* 11(4):413-430.
- Ansolabehere, Stephen and James Snyder. 2000. Soft Money, Hard Money, Strong Parties. *Columbia Law Review* 100(3):598-619.
- Barzel, Yoram and Eugene Silberberg. 1973. Is the Act of Voting Rational? *Public Choice* 16(1):51-58.
- Bennion, Elizabeth. 2005. Caught in the Ground Wars: Mobilizing Voters During a Competitive Congressional Campaign. *Annals of the American Academy of Political and Social Research* 601:123-141.
- Berch, Neil. 1993. Another Look at Closeness and Turnout: The Case of the 1979 and 1980 Canadian National Elections. *Political Research Quarterly* 46(2):421-432.
- Blais, André, Jean-Benoit Pilet, Karine van der Straeten, Jean-François Laslier, and Maxime Héroux-Legault. 2011. To Vote or To Abstain? An Experimental Study of First Past the Post and PR Elections. Working paper.
- Blais, Andre, Robert Young, and Miriam Lapp. 2000. The Calculus of Voting: An Empirical Test. *European Journal of Political Research* 37(2):181-201.
- Bowler, Shaun and Todd Donovan. Electoral Competition and the Voter. *Public Opinion Quarterly* 75(1):151-164.
- Clarke, Peter and Susan Evans. 1983. *Covering Campaigns: Journalism in Congressional Elections*. Stanford University Press.

- Cox, Gary and Michael Munger. 1989. Closeness, Expenditures, and Turnout in the 1982 U.S. House Elections. *American Political Science Review* 83(1):217-231.
- Dale, Allison and Aaron Strauss. 2009. Don't Forget to Vote: Text Message Reminders as a Mobilization Tool. *American Journal of Political Science* 53(4):787-804.
- Duffy, John and Margit Tavits. 2008. Beliefs and Voting Decisions: A Test of the Pivotal Voter Model. *American Journal of Political Science* 52(3):603-618.
- Erikson, Robert and Thomas Palfrey. 2000. Equilibria in Campaign Spending Games: Theory and Data. *American Political Science Review* 94(3):595-609.
- Ferejohn, John and Morris Fiorina. 1975. Closeness Counts Only in Horseshoes and Dancing. *American Political Science Review* 69(3):920-925.
- Festinger, Leon. 1957. *A theory of cognitive dissonance*. Stanford University Press.
- Gerber, Alan, Gregory Huber, Conor Dowling, David Doherty, and Nicole Schwartzberg. 2009. Using Battleground States as a Natural Experiment to Test Theories of Voting. APSA 2009 Toronto Meeting Paper.
- Gerber, Alan and Donald Green. 2000. The Effects of Canvassing, Telephone Calls, and Direct Mail on Voter Turnout: A Field Experiment. *American Political Science Review* 94(3):653-663.
- Gimpel, James G., Karen M. Kaufmann, and Shanna Pearson-Merkowitz. Battleground States versus Blackout States: The Behavioral Implications of Modern Presidential Campaigns. *Journal of Politics* 69(3): 786-797.
- Hill, David and Seth McKee. 2005. The Electoral College, Mobilization, and Turnout in the 2000 Presidential Election. *American Politics Research* 33(5):700-725.
- Jackson, Robert. 1996. The Mobilization of Congressional Electorates. *Legislative Studies Quarterly* 21(3):425-445.

- Kim, Jae-On, John Petrocik, and Stephen Enokson. 1975. Voter Turnout among the American States: Systematic and Individual Components. *American Political Science Review* 69(1):107-123.
- Levine, David and Thomas Palfrey. 2007. The Paradox of Voter Participation? A Laboratory Study. *American Political Science Review* 101(1):143-158.
- Matsusaka, John. 1993. Election Closeness and Voter Turnout: Evidence from California Ballot Propositions. *Public Choice* 76(4):313-334.
- Matsusaka, John and Filip Palda. 1993. The Downsian Voter Meets the Ecological Fallacy. *Public Choice* 77(4):855-878.
- Panagopoulos, Costas and Donald Green. 2008. Field Experiments Testing the Impact of Radio Advertisements on Electoral Competition. *American Journal of Political Science* 52(1):156-168.
- Patterson, Samuel and Gregory Caldiera. 1983. Getting Out the Vote: Participation in Gubernatorial Elections. *American Political Science Review* 77(3):675-689.
- Riker, William and Peter Ordeshook. 1968. A Theory of the Calculus of Voting. *American Political Science Review* 62(1):25-42.
- Seidle, Leslie and David Miller. 1976. Turnout, Rational Abstention, and Campaign Effort. *Public Choice* 27(1):121-126.
- Shachar, Ron and Barry Nalebuff. 1999. Follow the Leader: Theory and Evidence on Political Participation. *American Economic Review* 89(3):525-547.
- Tucker, Harvey. 1986. Contextual Models of Participation in U.S. State Legislative Elections. *The Western Political Quarterly* 39(1):67-78.