Journal of Ethics and Social Philosophy
vol. 26, no. 1 · october 2023
https://doi.org/10.26556/jesp.v26i1.2915

THE CASE FOR VOTING TO CHANGE THE OUTCOMES IS WEAKER THAN IT MAY SEEM

A REPLY TO ZACH BARNETT

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You are unlikely—*really* unlikely—to cast a deciding vote in the next election. Why vote at all, then? You may have reasons to vote that are not sensitive to how likely you are to change the outcomes, but let us put them to one side for now.[1] Do you have a reason to vote to change the outcomes? Whether you do depends, of course, not only on the probability of affecting the outcomes but also on the payoff if you do. For it to be rational to vote (to change the outcomes), it has to be the case that the expected value of voting (roughly, your chances of changing the outcomes multiplied by the significance of the change you will make) is higher than the cost of voting. It is a very common view that this is hardly ever the case and that you are almost always in a position to be all but certain that this will not be the case.[2] Disappointingly, this is said to be so even if you care about the common good so that the size of the payoff in the above inequality, if you do end up making a difference, includes the good outcomes for all—not just for you.

In a recent paper, Zach Barnett forcefully argues that this is a mistake. He shows how it follows, from rather conservative assumptions, that in many real-life cases, the expected social value of voting is higher than its (personal, and so presumably also social) cost, so that at least for a voter who is motivated to promote the common good, it does make sense to vote in order to change the outcomes.[3] Barnett is successful, we believe, in showing that the commonly held belief (that voters, because so unbelievably unlikely to make a difference,

1 For an initial discussion, and for references, see Brennan, "The Ethics and Rationality of Voting," sec. 1.3.

2 See Brennan, "The Ethics and Rationality of Voting," sec. 1.1., and the many references there. See also the quotes from Brennan in Barnett, "Why You Should Vote to Change the Outcome."

3 Barnett, "Why You Should Vote to Change the Outcome."

do not have a reason to vote in order to change the outcomes) is way too hasty. And this, despite our criticism below, is, of course, a significant achievement.

However, Barnett is—we argue here—too quick on one key premise, and once this is noticed, it is not clear how often Barnett's reasoning can point to a justification of voting to change the outcomes. Indeed, the problem facing Barnett here is very similar to what is arguably the underlying problem with the more pessimistic models he rejects. In this way, Barnett's reasoning may apply to significantly fewer real-life scenarios than he suggests.

1. Barnett's Argument

It is important for Barnett (for reasons we return to below) to avoid relying on too theory-driven modeling assumptions here. Instead, his argument relies on rather specific and arguably plausible conditions:

> *Stakes Condition*: The average social benefit (b) per citizen of electing the better candidate is more than twice as great as the cost (c) of voting (in short: $b > 2 \times c$).

> *Chances Condition*: The probability of casting the deciding vote (d) is at least one divided by the number (N) of citizens (in short: $d \geq 1/N$).[4]

Given these two conditions, it trivially follows that the expected value of voting is higher than its cost (in short: $\frac{1}{2} \times b \times d > c$).[5] We will not question this derivation, of course. Nor will we question the Stakes condition: it probably does not hold in full generality (nor does Barnett argue that it does), but it does seem to hold for many people, in many elections, in at least reasonably well-run democracies, and we are happy to constrain the discussion to just those.[6] The question for us, then, is why should we accept the Chances condition?

Barnett relies on the following two premises:

> *Partial Unimodality*: The leading candidate is at least as likely to earn exactly half of the vote as she is to earn any precise share of the vote smaller than this.

4 Barnett, "Why You Should Vote to Change the Outcome," 427.

5 Unless, that is, one is uncertain regarding the right vote. The less certain one is that the difference one's vote will make (if indeed it makes a difference) is for the better, the less confident one should be that the expected value of one's vote is higher than its cost (for it may be negative). For a critique of optimistic suggestions about voting to change the outcomes (including Barnett's) that emphasizes this point, see Brennan and Freiman, "Why Swing-State Voting Is Not Effective Altruism." We put this kind of consideration to one side here.

6 Barnett's relevant section is titled "The Stakes Condition: A Qualified Defense."

> *Narrow Upsets*: If the leading candidate fails to earn a majority, then the likelihood that she comes within ten percentage points of her opponent is at least ½.[7]

With these two assumptions in place, Barnett argues that as long as both candidates have at least a 10 percent chance of winning (surely, an easily met condition in real-world elections), the Chances condition follows—namely, that the probability of your vote being the deciding vote is greater than (or equal to) one divided by the number of voters ($d \geq 1/N$).

We will not take issue with this derivation. Nor will we doubt the Narrow Upsets premise, which—while, of course, is not a necessary truth—seems empirically very plausible, at least for the vast majority of elections.[8] The problem lies, we proceed to argue, with Partial Unimodality.

2. How Partial Unimodality May Fail

Partial Unimodality is, as Barnett notes, intuitively plausible. Suppose Daisy is projected to receive 52 percent of the votes. If so, then an outcome of 50 percent for Daisy is closer to the projection than any outcome where she receives fewer votes. Assuming that the likeliest outcomes are clustered together and that outcomes become less and less likely as one moves further away from the likeliest cluster, Partial Unimodality follows. This is especially clear if we assume (for now) normal distribution around the projected result.[9] As can be seen from figure 1, the further left from the 50 percent line, the lower the probability, so that Partial Unimodality is guaranteed to be true. And Barnett does not need something as strong as normal distribution. As long as the distribution of likelihood of results is sufficiently similar to the one in figure 1, Partial Unimodality is guaranteed to be true.[10]

7 Barnett, "Why You Should Vote to Change the Outcome," 434.

8 Of course, Barnett's reasoning applies—at least as-is—only to voting systems of the kind he describes. Whether the reasoning can be extended to other voting systems is an open question, to be answered piecemeal. We do not challenge Barnett on this front (and we thank an anonymous referee for relevant discussion here).

9 Barnett does not assume anything as strong as normal distribution. In fact, he rejects this (when criticizing Brennan and the binomial model). We return to this. We rely here on the case of normal distribution for heuristic purposes alone.

10 And there may be some case of a distribution that satisfies Partial Unimodality even though it is quite far from a normal distribution—like, for instance, uniform distribution. But we can safely ignore such cases here.

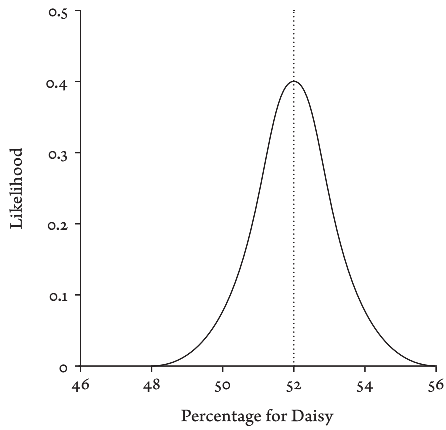

FIGURE 1 Normal distribution

Now, we are doing empirical predictions here, not mathematics, so Partial Unimodality is not a necessary truth. And Barnett himself acknowledges the possibility of cooked-up cases where Partial Unimodality fails (his example is one where one relies on two polls, suspecting that one of them is fraudulent).[11] We want to now suggest that the problem is more serious than that and that cases where Partial Unimodality fails need not be all that cooked up. Consider, then, the following examples. In all of them, Daisy is still projected to get 52 percent of the vote.

Systematic Mistake: There is a part of the voting population that is tricky to capture in polls. In all likelihood, either the pollsters overcame this problem, in which case there will be no systematic error here, or they did not, in which case a rather chunky mistake is present. Perhaps, if the pollsters did not overcome the problem, Daisy is likely to get around 48 percent of the vote. In such a case, a 48 percent outcome for Daisy may be more likely than a 50 percent one.

Last-Minute Event: The polls do a very good job at reflecting the voting plans of most at the time of conducting the poll, but some last-minute event may bring about a change in the vote of some 4 percent of the population from Daisy to her rival (Donald). If, for instance, Daisy is perceived as the more dovish candidate, perhaps a last-minute terrorist attack may have such an effect. If there is no terrorist attack, the outcome is likely to be very close to 52 percent for Daisy. If there is a terrorist

11 Barnett, "Why You Should Vote to Change the Outcome," n27.

attack, though, it is likely to be around 48 percent for Daisy. But around 50 percent is an unlikely result on either scenario.

Guru: About 4 percent of the voting population will vote according to what the Guru will tell them. Right now, the Guru tells them to vote for Daisy. But he may change his mind. If he does, this will bring about a "chunky" change in voting—rendering 48 percent a more likely outcome than 50 percent.[12]

It may be argued that even if we are right about these cases, a restricted version of the Chances condition would still hold—not for very competitive elections, but for those in which the expected error in the polls (in our examples, 4 percent) is approximately equal to the leading candidate's advantage in the polls.[13] We are not sure how many real-world cases will survive this restriction.[14] In addition, there are plausible scenarios where the expected error of the poll cannot match the advantage of the leading candidate:

Close-Call Incentive: Donald represents a suppressed minority that traditionally has low election turnout. Given that the latest polls predict that Donald is at least 48 percent likely to win, this very fact—the polls' projections—gives a weighty incentive for members of Donald's minority to vote. Such influence may predictably lead to Donald getting 52 percent of the vote and may make a 52 percent outcome likelier than a 50 percent one.

In this case, if the leading candidate has a large advantage, the Close-Call Incentive will not be activated, so a large error can only occur when none of the candidates has a significant edge. Therefore, there cannot be a situation where the anticipated error is close to the advantage of the leading candidate.

None of these cases, it seems to us, is too far-fetched. Indeed, the first three cases are loosely based on plausible descriptions of real elections where we come from. And as we show in a brief appendix, all cases find support in the empirical literature. When our predictions for the most likely election outcome

12 We thank Dor Mitz for this example.

13 Barnett suggested this response in correspondence.

14 Remember that the Chances condition also requires that the likelihood that the leading candidate comes within ten percentage points of her opponent is at least one-half. In the case described in the text, where the elections are less competitive, this requirement is more restricting.

have a risk of systematic failure, Partial Unimodality may fail. In these cases, the likelihood distribution of outcomes looks more like it does in figure 2.[15]

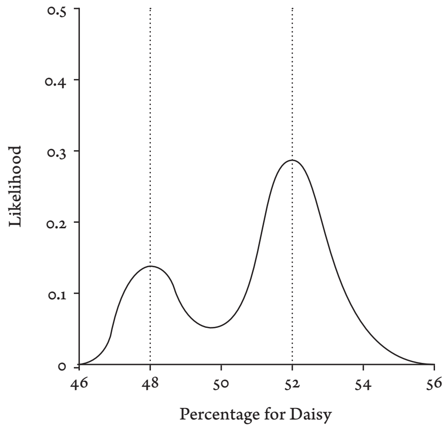

FIGURE 2 A violation of Partial Unimodality

What can be said about the conditions in which Partial Unimodality stands (or falls)? For one, if we can assume that each of the votes is probabilistically independent of any other (i.e., a premise of Independence), then we are left with a normal distribution of likelihoods around the projected results, as in figure 1, and then, of course, Partial Unimodality holds. But Independence is a very strong premise (and a highly implausible one empirically), and it is one that we have reason to believe that Barnett rejects (because he rejects the binomial model—see below). So it is important to note that he does not need Independence. He can settle for weaker premises that will nonetheless guarantee that there are no local maxima on the distribution, no "hills" of the kind that appear in figure 2 around the 48 percent line.

This, while not as strong a premise as Independence, still amounts to a highly nontrivial empirical hypothesis. As the (not-too-cooked-up) examples above show, there are quite realistic scenarios in which the no-hills hypothesis is false and, furthermore, knowably false. But, of course, in order to make more progress on Partial Unimodality, what is needed is not more *a priori* reflection, but empirical analysis.[16] In the appendix, we provide evidence that there

15 There may be other types of realistic cases where Partial Unimodality fails, cases with a very different graph as well.

16 *A priori* speculation can get us some of the way there, of course. We hope that the (hypothetical, if actual-world-inspired) examples above are not entirely useless. But it is not remotely enough. Perhaps, for instance (as Barnett suggested in correspondence), the effects present in Systematic Mistake, Last-Minute Event, and Guru are likely to be rather

may be instances where partial unimodality fails. However, we do not provide evidence regarding the frequency of such occurrences. We are not aware of any empirical study that is directly focused on the likelihood of Partial Unimodality. The one influential study we did find seems to indicate failures of Partial Unimodality.[17] That study is limited—partly because it focuses on very close elections, and there are not sufficiently many of those to support strong conclusions—but until stronger empirical analyses are presented, the bottom line remains the same: cases in which Partial Unimodality fails are not too far-fetched, and the speculation that they are quite common is plausible enough to pose a problem for Barnett's argument.

To the extent that Barnett's is the best case for a vindication of voting to change the outcomes, much more work needs to be done before this vindication is complete.

3. CONCLUDING OBSERVATIONS

We want to conclude with several brief observations.

First, failures of Partial Unimodality may be interestingly distributed. For instance, they may not be distributed symmetrically—perhaps, for instance, Guru-like cases are more likely among Daisy's voters, or perhaps systematic mistake is more likely among Donald's. This may result in different verdicts for different voters regarding whether or not they have a reason to vote to change the outcomes. We take this to be a plausible result.

Second, there is an interesting relation between the problem with Partial Unimodality (and therefore also with Barnett's argument for the Chances condition and, with it, his argument for the conclusion that we very often have a reason to vote to change the outcomes) and Barnett's own criticism of Brennan's use of the binomial model in generating his (Brennan's) overwhelmingly small expected value for voting ("Under a binomial model, an *N*-voter election is modeled as *N* tosses of a biased coin, where the coin's bias is fixed by the specifics of the case.")[18] Barnett does not explain *what* is wrong with the binomial model, but he does give reasons—conclusive reasons, we think—to believe *that* something is wrong with it as an attempt to model real-world voting.[19] We do not need all the details here, but it is safe to say that the problem with the

rare and small, and perhaps to (pretty much) cancel each other out when present. These too are legitimate speculations (though we do not find them plausible). But we need empirical analysis to decide.

17 Mulligan and Hunter, "The Empirical Frequency of a Pivotal Vote."

18 Barnett, "Why You Should Vote to Change the Outcome," 431.

19 Barnett, "Why You Should Vote to Change the Outcome," 440.

binomial model is that the distribution of likelihoods it predicts is clustered—as a normal distribution or something very close to it—around the projected outcome. In fact, the binomial model *does* presuppose Independence, and it is a plausible hypothesis that the binomial model's failure is due precisely to the empirical implausibility of Independence. But even if Independence is not the whole story of the binomial model's failure, still it is clear that something in the vicinity is—the fact that (for instance) Brennan assumes that the distribution of likelihood of outcomes clusters nicely around the projected outcome is what spells the model's doom.[20]

However, Barnett does not settle for merely showing that Brennan's model is unrealistic. He also puts forward a model seemingly showing a reason to vote to change the outcome. So, while Barnett is correct to assert that Brennan's assumption of voter independence is unrealistic, our criticism of Barnett's use of Partial Unimodality—somewhat ironically—shows that Barnett, too, falls prey to a rather similar (if less acute and conclusive) flaw.[21]

Last, we want to tentatively suggest a methodological point, for Barnett may respond by insisting that even if Partial Unimodality often fails as a matter of objective reality, still voters are rarely if ever in a position to know this.[22] As the examples above and the empirical data in the appendix indicate, this may not be so, but let us suppose that it is. Seeing that the mission Barnett has embarked on is precisely to show that it often makes sense—from a voter's perspective—to vote in order to change the outcomes, unknown "hills" in the distribution of likelihood of outcomes do not seem to matter. So, on the assumption that known "hills" are very rare, our objection to Barnett's argument seems to fail. Now, this line of thought is surely right when emphasizing that the nature of the mission here is not one that allows talk of the objective "ought" or some such. (Presumably, whether I objectively ought to vote in order to change the outcomes simply depends on whether or not, as a matter of objective fact, I end up casting the deciding vote.) So it is very tempting to add the uncertainty about potential failures of Partial Unimodality to the general uncertainty mix. But—and here is our tentative suggestion—we are not sure this is so. The

20 In correspondence, Barnett suggested that the issues with the binomial model might be more complicated.

21 As already briefly noted, Barnett explicitly says that he does not want to rely on any elaborate model (let alone the binomial one), and instead hopes to rely solely on specific highly plausible premises ("Why You Should Vote to Change the Outcome," 441). So it bears reemphasizing that his premises—or anyway, Partial Unimodality—smuggle back in one of the main causes for concern regarding the binomial model.

22 In correspondence, Barnett suggested this response. The wording in the text here is sloppy for a reason we return to shortly.

uncertainty about whether or not one will cast the deciding vote is the uncertainty that defines the problem and, indeed, the mission Barnett has embarked on. Uncertainty about Partial Unimodality—that is, about possible "hills" in the outcome-likelihood distribution—seems to be of a different kind, perhaps because it is second-order (being already *about* likelihoods, presumably understood subjectively). Furthermore, such second-order uncertainty may have unique characteristics. Perhaps, for instance, while there is some reason to think that the possibility of "hills" in different places and of different "heights" along the distribution can be safely ignored when drawing conclusions about elections in general, still in many real-world cases the specific voter will have much richer, more specific information about the specific election they are facing (those in their state, say, or in their county), such that in that specific case possible hills cannot be safely neglected. So if the only way of saving Barnett's argument is by adding this second-order uncertainty into the usual uncertainty mix, the stakes will have been raised. (And, to repeat, we think that there are sometimes likely to be knowable hills, and furthermore, that the naked probability that there are such hills [that is, that the relevant instance of Partial Unimodality fails] in close elections may be quite high, so that the rational voter should not take Partial Unimodality for granted).[23]

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APPENDIX: EMPIRICAL SUPPORT FOR THE FAILURE OF PARTIAL UNIMODALITY

The purpose of this appendix is limited. We do not claim to offer a comprehensive survey of the literature here, nor do we attempt an assessment of how often it is that Partial Unimodality fails. Instead, our purpose here is to show that such failures are sometimes in place, and indeed, knowably so. This appendix shows, then, that the cases in the text are not too far-fetched and that what is needed for assessing whether, in a particular case or set of cases, there is a reason to vote to change the outcomes is further empirical research (and not just more *a priori* modeling).

23 We thank Zach Barnett and Jason Brennan for very helpful and gracious comments in correspondence. We also thank two referees for this journal for valuable comments on this paper. For comments on earlier versions, we thank Ittay Nissan-Rozen.

Systematic Mistake cases are argued to be common in pre-election polling.[24] For example, one explanation for the inaccuracy of certain polls in failing to predict Trump's 2016 victory is the lack of adequate representation of non-college-educated white voters.[25] This error is paradigmatically a case of Systematic Mistake. And while such mistakes are often easy enough to recognize in hindsight, it is often very difficult for voters to determine—before an election—whether there is such a mistake, and in particular, whether the Systematic Mistake—if there is one—is similar in size to the advantage of the leading candidate.

Another explanation suggested by the Kennedy et al. analysis of the 2016 poll inaccuracies is a "late swing" of votes toward Trump. "Late swings" are used to explain the failure of polls in other cases as well.[26] The common explanation for this phenomenon is that late deciders are less politically anchored and, therefore, more susceptible to being influenced by campaign events.[27] People who are less anchored politically are also more susceptible to the impact of celebrity endorsements on their opinions.[28] Moreover, in more traditional societies, the support of traditional leaders can sway voters' decisions, particularly when goods are delivered in partnership with these leaders.[29] These studies suggest that both Last-Minute Event and Guru have empirical support in real-life cases.

Finally, it has also been observed that close elections can impact voters' incentives. According to Vogl, certain racial groups tend to be more enthusiastic about voting in closely contested elections. There are also other reasons that the closeness of the prediction may influence the outcomes in a biased way.[30] So, our Close-Call Inventive case is not too far-fetched either.

Given this evidence, it is no wonder that sophisticated statisticians incorporate measures to mitigate such systematic failures of polls in their models. In a teardown of his 2014 Senate Forecast model, Nate Silver stressed that his model must address this issue:

24 Walsh, Dolfin, and DiNardo, "Lies, Damn Lies, and Pre-Election Polling."

25 Kennedy et al., "An Evaluation of the 2016 Election Polls in the United States"; Silver, "Pollsters Probably Didn't Talk to Enough White Voters without College Degrees."

26 Durand and Blais, "Quebec 2018."

27 Fournier et al., "Time-of-Voting Decision and Susceptibility to Campaign Effects."

28 Veer, Becirovic, and Martin, "If Kate Voted Conservative, Would You?"

29 Baldwin, "Why Vote with the Chief?"; Brierley and Ofosu, "Do Chiefs' Endorsements Affect Voter Behaviour?"

30 Vogl, "Race and the Politics of Close Elections"; Grimmer et al., "Are Close Elections Random?"

> In a number of recent elections, one party has either gained considerable ground in the closing stages of the race (as Democrats did in 2006) or the polls have had a strong overall bias toward one party or another on Election Day itself (as in 1994, 1998, and 2012).[31]

In order to mitigate this problem, Silver conducted a series of simulations in which systematic biases were randomly assigned. After implementing this solution, the final forecast ended up violating partial unimodality. Based on his model, the Republicans were most likely to finish with fifty-two seats, but they were more likely to hold forty-nine or fifty seats than fifty-one (fig. 3).[32]

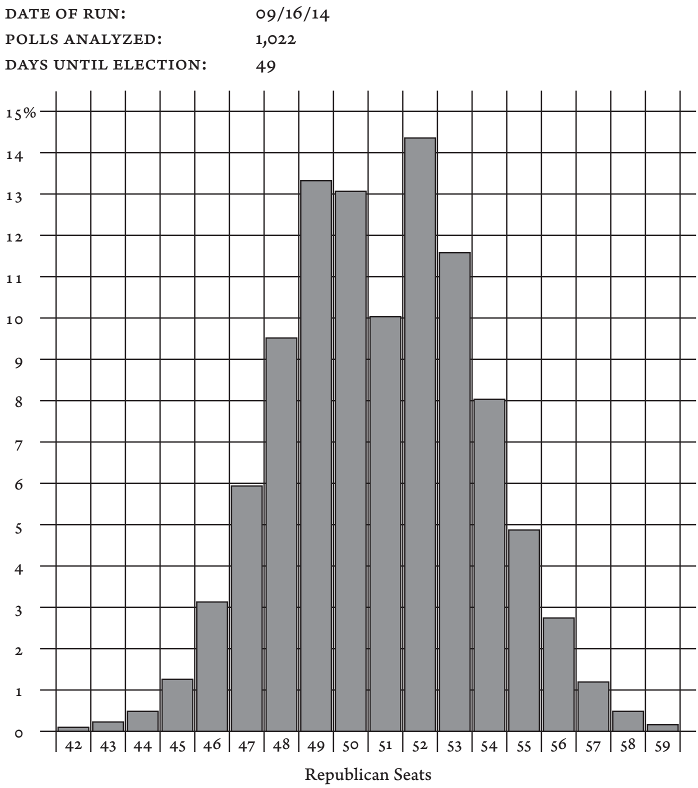

FIGURE 3 The predictions of Silver's senate forecast model
Source: Silver, "How the FiveThirtyEight Senate Forecast Model Works"

31 Silver, "How the FiveThirtyEight Senate Forecast Model Works."

32 We thank an anonymous referee for suggesting that we address Silver's models here. We were happy to find out that at the end of the day, Silver's models—and even graphs—support our main point in this paper.

REFERENCES

Baldwin, Kate. "Why Vote with the Chief? Political Connections and Public Goods Provision in Zambia." *American Journal of Political Science* 57, no. 4 (October 2013): 794–809.

Barnett, Zach. "Why You Should Vote to Change the Outcome." *Philosophy and Public Affairs* 48, no. 4 (Fall 2020): 422–46.

Brennan, Jason. "The Ethics and Rationality of Voting." *Stanford Encyclopedia of Philosophy*, (Winter 2020). https://plato.stanford.edu/archives/win2020/entries/voting/.

Brennan, Jason, and Christopher Freiman. "Why Swing-State Voting Is Not Effective Altruism: The Bad News about the Good News about Voting." *Journal of Political Philosophy* 31, no. 1 (March 2023): 60–79.

Brierley, Sarah, and George Ofosu. "Do Chiefs' Endorsements Affect Voter Behaviour?" International Growth Centre, 2021.

Durand, Claire, and André Blais. "Quebec 2018: A Failure of the Polls?" *Canadian Journal of Political Science* 53, no. 1 (March 2020): 133–50.

Fournier, Patrick, André Blais, Richard Nadeau, Elisabeth Gidengil, and Neil Nevitte. "Time-of-Voting Decision and Susceptibility to Campaign Effects." *Electoral Studies* 23, no. 4 (December 2004): 661–81.

Grimmer, Justin, Eitan Hersh, Brian Feinstein, and Daniel Carpenter. "Are Close Elections Random?" 2011. http://web.stanford.edu/~jgrimmer/CEF.pdf.

Kennedy, Courtney, Mark Blumenthal, Scott Clement, Joshua D. Clinton, Claire Durand, Charles Franklin, Kyley McGeeney, Lee Miringoff, Kristen Olson, Douglas Rivers, et al. "An Evaluation of the 2016 Election Polls in the United States." *Public Opinion Quarterly* 82, no. 1 (Spring 2018): 1–33.

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

Silver, Nate. "How the FiveThirtyEight Senate Forecast Model Works." FiveThirtyEight.com, September 17, 2014. https://fivethirtyeight.com/features/how-the-fivethirtyeight-senate-forecast-model-works/.

———. "Pollsters Probably Didn't Talk to Enough White Voters without College Degrees." FiveThirtyEight.com, December 1, 2016. https://fivethirtyeight.com/features/pollsters-probably-didnt-talk-to-enough-white-voters-without-college-degrees/.

Veer, Ekant, Ilda Becirovic, and Brett A. S. Martin. "If Kate Voted Conservative, Would You? The Role of Celebrity Endorsements in Political Party Advertising." *European Journal of Marketing* 44, nos. 3–4 (April 6, 2010): 436–50.

Vogl, Tom S. "Race and the Politics of Close Elections." *Journal of Public*

Economics 109 (January 2014): 101–13.
Walsh, Elias, Sarah Dolfin, and John DiNardo. "Lies, Damn Lies, and Pre-Election Polling." *American Economic Review* 99, no. 2 (May 2009): 316–22.