Memorandum to Editor

We were very pleased with the positive response to our paper, and with the helpful suggestions of the reviewers and the editors.  We have been able to respond to all of them, and in doing so we feel as if the paper is greatly improved.  We first note the overall changes we have made in our manuscript, and then we discuss we discuss our response to each specific suggestion.

We agree that the essay was quite dry and have added some anecdotes from the 2016 election to illustrate two observations in the paper, namely the failure of journalists to consider the importance of the non-competitive states for EC outcomes, and the focus of campaigns on the battleground states. We have also expanded the literature review, both in the text and in the concluding discussion, so that the essay builds more generally off the Electoral College literature, and more specifically on work in Public Choice (including an additional two references that appeared in this journal).

The most important improvements we have made involve new data now reported in the Appendix and summarized briefly in the text. We have added several additional robustness checks, including an examination of what changes if we define a competitive state in terms of as +/- 1.5% vote margin, and how our ex-post measures of competitiveness compare to the Shaw and Althaus’s (2017) data on *ex ante* battleground status for the post-WWII era that is based on observed campaigning. These additional robustness checks validate the usefulness and credibility of our non-competitive advantage measure. We are deeply indebted to the Reviewer who suggested we contact Professor Shaw, who was kind beyond all expectations in allowing us access to his data set even before he had published his own work using that data.

Below we respond to each of the reviewers concerns one by one.

Response to Reviewer #1  
  
p. 2, line 16: there ARE m states.

*Done*

p. 3, line 29: 53% is misleading; the author should say 6% greater than the other major-party candidate, because other candidates may prevent the winner from getting 53%.

***We added in text to make clear that this is the two-party vote share, and added a footnote that the margin can be no larger than 6%.***

p. 3, line 39: The correlation analysis (and later the regression analysis) does not seem to add much, because virtually all the correlations are high (and the regression coefficients are in the expected direction).

***Because there are five different variables being compared, and the correlations among each pair of them are of interest (and not contained in the Brams and Kilgour analysis because of the limited number of years they had to work with) we would prefer to keep Table 1 in the paper. If the editor wished us to remove it, to save space, of course we would do so, and move it to the appendix.***

p. 9, line 9: better THAN the Brams-Kilgour variables.

***Done***

p. 9, line 43: What the author's measure of "non-competitive advantage," while simple, hides is whether mostly large, medium, or small states--or some combination--are the source of a candidate's strength or weakness.  This might be useful information to unpack when the index fails to make a correct prediction.

***This is a very nice suggestion, but, this type of analysis fits better in a paper on campaign strategies and takes us well beyond the scope of the present note.***

p. 11, line 7: Some speculation on the failure of the index in these two elections would be useful here.    
***As we now note in the text, close elections nationally bear many resemblance to a flip of a coin, so that a candidate who does unexpectedly well in the close states can overcome his opponent’s lead in non-competitive states, especially if that lead is not that large.***

The on-line appendix tests robustness by changing +/-3% to +/-5%; why not go in the other direction to +/-1 1/2%, as did Brams and Kilgour.

***We have added to the appendix a section explains the results of a robustness check involving changing the competitiveness measure downward to +/- 1.5%. In doing so, we now have results for +/- 1.5%, 3%, 5%, and using Shaw and Althaus’s ex ante identification of battleground states. All the models lead to essentially the same conclusions about Non-Competitive Advantage.***

Response to Reviewer #2  
  
Now, I do wish that the authors were a little more skeptical (or discerning) with respect to the Brams and Kilgour theory. Most notably, their acceptance of a post-hoc definition of competitiveness is not compelling. For example, in 2008 the state of Florida was considered a pre-eminent battleground state. McCain and Obama spent millions of dollars there, and all four presidential and vice-presidential candidates made multiple stops in the Sunshine state. In the end, Obama won by over 5 points. So it wasn't competitive?

***The ex post definition of competitiveness we took from Brams and Kilgour, and it is needed for time periods for which pre-election survey data is unavailable at the state level – i.e., for most of the period we are dealing with. We believe that we have addressed the concern about the link between ex ante and ex post measures with our robustness checks , especially that using Shaw and Althaus’s ex ante campaign strategy data (see below). But we have also expanded our discussion of the point (also made by Shaw and Althaus) that closeness is not the only factor affecting campaign decisions as to where to campaign.***   
  
The authors may want to see if they can get a recent paper from Scott Althaus and Daron Shaw on the candidates' actual Electoral College strategies from 1952 through 2016. This would be a nice check on what they use here.

***We thank the reviewer very much for this suggestion. We did in fact contact Profs. Althaus and Shaw, who were kind enough to share their unpublished paper and their data with us. In Appendix C, we have reproduced the results from the main text using their ex ante measure of battleground state, which distinguishes states viewed as battlegrounds by each of the two campaigns, though there is considerably cross-party overlap. Regardless of whether we look only at the battleground states that both parties classified as battlegrounds, or include the states that either campaign so classified, or look separately by party, campaigning and post-hoc competitiveness are now (esp. post 1988) very closely linked.***

The authors might also consider that underdog candidates do not simply compete in competitive states. Because they have to win 270 electoral votes, they have to compete in states where they are decidedly behind. (A and S also have an interesting take on this.)

***We agree with this point, and have now made appropriate citations to previous literature (including. Stromberg 2008, and Althaus and Shaw 2017).***

I would also like to see the authors leverage their time series a little more effectively. Do relationships between winningness, vulnerability, and EC results fluctuate at all as we move across different party systems (1828-1856, 1860-1892, 18986-1928, 1932-1964, 1968-2016)? Since different party systems reflect coalitional changes in the parties' constituencies, they might influence the relative import of these different factors, or at least their distribution across states.

***We have added a plot, now labeled “Figure II”, which shows a time series between percentages of competitive and uncompetitive EC votes. We tie this to some literature that says that the number of battlegrounds have decreased over time, but we concur with Shaw and Althaus’s findings that any decline is slight. We have chosen not to add any other material on periodization effects, although, following the reviewer suggestion, we did some analyses directly bearing on this point. Basically, the correlations were strong throughout and we did not find realignment era distinctions that were worth discussing.***

The authors' distinction between close and uncompetitive elections is interesting, and ought to be extended. Perhaps in conjunction with a by-era analysis, as described above.

***We took this point very seriously and, as noted above, thanks to the kindness of Shaw and Althous in sharing their unpublished data on campaigning in presidential elections from 1952 to the present, we were able to test the claim that competition ex post can be used as proxy for battleground states, while recognizing the point made strongly in Shaw and Althaus’s unpublished working paper that closeness is not the only factor affecting campaign decisions as to where to campaign. Also as noted above, we now have a plot, labeled “Figure II”, which shows a time series between percentages of competitive and uncompetitive EC votes.***

There is, as mentioned earlier, an extensive literature beyond B and K. I don't know that the paper needs to slog through the dozen or so studies that might be relevant, but it is a rather skimpy set-up that could benefit from a broader review of the extant literature. (I can't believe I'm asking for more gratuitous literature review...I feel hell freezing over...)

***We appreciate the need for a slightly longer literature review, which we were happy to provide. There are now about a dozen articles not cited in the submitted version which are referenced in the current version.***

The paper is generally well-written, but the current manuscript feels a little dry. The authors might want to consider connecting it up more directly to the 2016 and (speculatively) the 2020 elections. Bring some politics into it!

***We now begin the paper with a discussion of red state vs. blue state America that segues into a discussion of why presidential candidates and the media tend to focus on the so-called purple states. To enliven the paper (and make clearer its relevance to highly contested elections, such as the 2016 contest, we have added some information and anecdotes about the 2016 election.***