

From: Bernie Grofman bgtravel@uci.edu
Subject: what to do after rejection at AJPS
Date: August 22, 2017 at 11:09 AM
To: Jonathan R. Cervas jcervas@uci.edu

BG

Jonathan I think the reviewers are on target and we need to do all the things they want, including shortening this considerably to make this a research note, and either deleting the observations the reviewers find doubtful or sticking them into footnotes with a sentence or two of additional reasoning added.

//Bernie

On 8/22/2017 4:36 PM, Jonathan R. Cervas wrote:

BG, one was attached as a word doc, the other two in the text of the email. Here they are again for your convenience. Let me know if we want to submit *as is*, or if I should work on some of the changes suggested in these reviews. If we are going with *as is*, which journal do you think we should try at next? I believe the review in the .doc file might actually be Zingher, since I requested him as a reviewer and it cites his paper.

REFEREE REPORTS FOLLOW

Reviewer #2: This paper compares the various ways to measure malapportionment in the Electoral College, and concludes that if one measure's it in a particular manner current levels of disproportionality are not so bad. While I don't have many major quibbles with the mechanics of the research, I'm not convinced it belongs in a journal like AJPS. The authors do a nice job of justifying the measure of disproportionality they support and providing extensive historical evidence to give long-range view of EC malapportionment. However, as the paper is written - with extensive detail on the various ways to calculate malapportionment indices and little in the way of testing causal relationships - it would fit better as a book chapter where there is more room to explore the various arguments and measurements. Alternatively, the authors might look to a more specialized journal, such as Electoral Studies. While I think the findings should be available to scholars and practitioners, the discipline's leading journal is probably not the right place for this treatment.

Furthermore, a sizable distraction in reading this paper is the writing. There are a number of confusing and run-on sentences. This one, for example, is taken from the abstract: "We show that the common wisdom about the extent of malapportionment in the EC is exaggerated once we distinguish between large differences in population share and EC vote share between the most under represented and the most overrepresented state, since the states with the greatest malapportionment do not make up a large share of the EC vote." If the authors are looking to place this in a general interest field journal, I would recommend that they tighten up the writing and make this a considerably shorter piece - a research note.

Reviewer #3: Manuscript Review

"How Malapportioned is the Electoral College? A Multiple Indicators Historical Perspective: 1790-2016"
AJPS-40238

The author(s) presents several different measures of malapportionment for the Electoral College since the founding of the country. After comparing them across time and measure, the author concludes that malapportionment in the Electoral College is getting less severe than it was and is comparable to other western democracies.

The topic of the manuscript certainly deserves more attention in the literature than it currently has. And, the author does a good job at offering different views of malapportionment and demonstrating the interesting statistics across time. Still, the manuscript does not bring much that is theoretically new. And, the overall points are grounded well enough as well as leave more questions than answers.

To my reading, the main thrust of the manuscript can be summed into four statements.

First, malapportionment measures that utilize all the data (totals and averages) produce results that are less extreme and more stable than measures that utilize just the extreme outliers (max and min). This obviously is not surprising at all. The author does not directly rely on this point too much, but it is indirectly important in that only these all-data measures are used in the international comparison.

Second, almost all these measures indicate more stability and less severity since the U.S. House was frozen at 435 seats (1929) and since Baker v. Carr (1962). These results are simultaneously not-surprising, a bit surprising, and not relevant. They are not surprising because since 1929 only Alaska and Hawaii have been added as states (both in 1959), and both had decently large populations (though, admittedly, both smaller than the national ideal district size for U.S. House seats). In the mid- to late-19th century, several states were added to the Union that were much smaller than the national ideal district size - and a few even had a smaller number of residents than the federal regulations for statehood (60,000 residents). For example, Nevada gained statehood in 1964 even though it only had about 7,000 residents; but it did have lots of silver! These types of statehood additions will, of course, produce extreme malapportionment statistics.

They are surprising - at least to some - that they have not gotten worse with the U.S. House not gaining any new seats since 1929 (actually when AZ and NM became states in 1912 the total number of seats became 435 and has stayed at that size since then, except for the years between AK and HI's statehood and the 1962 reapportionment). Anyway, it is intriguing that capping the U.S. House seats has not jacked-up malapportionment. The author has an interesting paragraph or so on this. The author posits that it is

an artifact of the exogenous distribution of population growth. I buy this; but as it is exogenous, it could just as easily have been or will become much worse. So, that in of itself does not say much about the theoretical aspects of malapportionment in the Electoral College; it might just be a historical quirk.

They are, somewhat, not relevant in terms of Baker v. Carr. The Baker decision is used in two ways in the manuscript. First, it provides the legal precedent for concentrating on (or in this case comparing to) the measure of Total Population Deviation (one of the two "extreme" measures). This is relevant, but more on that below. Second, the author includes in all the Figures and in the text a pre- and post-Baker comparison. This is not relevant. Baker solely dealt with intrastate malapportionment. It has had no effect on interstate malapportionment, which is the subject of this manuscript.

Third, the malapportionment results for the U.S. Electoral College are about the same as for parliamentary systems in western democracies and much less than the inequality produced in capitalism. As for the western democracy comparison, they are only briefly mentioned in the main text and a couple statistics are cited in fn 16-18. If this is an important point - as the Abstract, Intro, and Conclusion suggest - more needs to be done in the analysis sections. Also, if I'm reading the analyses correctly, the author is comparing the institutional malapportionment of the Electoral College with the partisan malapportionment of western democracies. If so, this entirely misses the author's main goal - to be comparing the malapportionment inherent in the institutional design while leaving aside how the (potentially more interesting) partisan distributions are accentuated or not by the institutional design. And, comparing the Gini measure of malapportionment (which I like) to the Gini measure of income inequality just doesn't make sense to me. Democratic processes and institutions are supposed to be largely fair; the outcomes of capitalism are something entirely different.

Fourth, if malapportionment is not worse, than it is good. As noted above, the main baselines that the author is using are historical trends in the U.S. and comparisons to the western democracies. Ignoring the above issue with these, there is still little basis for arguing that the malapportionment that we do have is acceptable. This begs questions of democratic norms and U.S. legal history - both of which are largely missing from the manuscript. The U.S. Courts, as the author notes, focuses on Total Population Deviation - and for a reason: it focuses on individual effects, averages and the like do not (see, e.g., Edelman (2006)). So, why should we emphasize averages and totals when U.S. jurisprudence doesn't? Furthermore, in many of the pre-Baker intrastate malapportionment cases, the challenged apportionment was not as bad as some of the previous ones (see, e.g., Ansolahere and Snyder (2008)) - many times if the plaintiffs won, the Court would simply revert to the previously most recent apportionment, which made things worse. So, was Baker not justified either? Finally, the author states on p. 4 that the results confirm that the presidential elections in 2000 and 2016, in which the President won the Electoral College but lost the popular vote, cannot be blamed on increasing institutional malapportionment. This statement is odd: can only increasing institutional malapportionment produce such result? And, does this not also require some analysis of the partisan distributions too? And, how does it square with Neubauer and Zeitlin's (2003) analysis of the 2000 election? As a side, I agree - President Trump's victory was not due to institutional malapportionment - that was purely about partisan distributional issues.

Two smaller points:

1. Large states are rarely malapportioned. As Ladewig and Jasinski (2008) and Ladewig (2011) point out, the problem is with population remainders (or, as the author calls it the integer allocation problem), which afflicts the smaller states much more. In other words, the most under-represented and the most over-represented states will almost always be smaller states. The author alludes to this in fn 9, but there are other parts of the text that seem to suggest otherwise.
2. The calculation for the worst-case scenario in fn 7 doesn't make sense to me. Maybe I'm just missing something...

In sum, I do like this topic quite a bit. And, the author has interesting things to add to the literature. But, this manuscript is just not yet ready for a venue like AJPS. I would encourage the author to think about the above points and maybe expand the paper into the partisan distributions as well.

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On Aug 22, 2017, at 2:19 AM, Bernie Grofman <bgtravel@uci.edu> wrote:

Jonathan, I only say one review. Is there more than one? I agree that this review suggests going down a notch in where to submit. This was not unexpected but it was worth trying a top journal.. //Bernie

On 8/8/2017 6:05 PM, Jonathan R. Cervas wrote:

BG, here is the decision on the malapportionment paper at AJPS. While the result is not ideal, these reviews are much better than I would have imagined from this journal. There are very few comments from the reviewers about things they would like changed. Two of the three reviewers said it would be more warranted in the top journal if there was more on the partisan effects (which we took out to make a separate paper!). One thing we might consider is submitting as it to another, lower journal. Alternatively, we can make the few small changes they suggests such as tightening the language and submitting again. I have a feeling if we went to APSR or Journal of Politics we would have the same critiques even with the changes since

again. I have a feeling if we went to JLE or JLE or Journal of Politics we would have the same critiques even with the changes since there is not theoretical contribution to the paper, so dropping down a level might be the correct move.

Bottom line, I think the changes in this paper are more straightforward than the Alternative EC rules paper. I'm going to try and finish the edits to the Public Choice paper today so that one will be done. I started uploading the data to my new GitHub account at <https://github.com/jcervas/Non-Competitive-Advantage>. I am just cleaning up the R code so anyone that may want to can replicate.

In sum, I think these reviews are very promising, and with little change this paper should get in somewhere.

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Begin forwarded message:

From: "AJPS" <em@editorialmanager.com>
Subject: Editorial Decision on AJPS-40238
Date: August 8, 2017 at 9:44:45 AM PDT
To: "Jonathan Cervas" <jcervas@uci.edu>
Reply-To: "AJPS" <ajps@msu.edu>

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