Manuscript Review
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"Legal, political science and economic approaches to measuring malapportionment"

The authors present different techniques (from law, political science, and economic) for measuring malapportionment in representative institutions. They apply them to the U.S. House, U.S. Senate, and the U.S. Electoral College.

I was quite excited to read this manuscript. Although there are occasionally discussions in the literature of malapportionment in general, a deeper understanding (mathematically and constitutionally) of the comparative metrics of malapportionment is sorely missing.

Unfortunately, I was left under-whelmed with the manuscript in its present form. I do think, however, that reasonable adjustments can be made to enhance and clarify the manuscript's contributions.

My general take-away from the manuscript is this: There are about seven main measures of malapportionment across these disciplines; They all measure slightly different things; But, when applied to the "history" of these three institutions, the U.S. Senate is always the worse offender. Maybe it was my excitement for the essay, but I was left feeling like I was given a bait-and-switch and then undersold. I'll explain.

My expectations for the manuscript was a presentation of the main measurements of malapportionment and a serious discussion of the differences among them. As some of the manuscript's cited works discuss, the raging debate (more in terms of which apportionment method for the House should be used, but it is equally applicable here too) is about what type of malapportionment should be minimized – for instance, absolute difference or relative difference. But, when the SCOTUS stepped into the mix with Baker, they simplified the analyses significantly. The work of Balinski and Young (the book is cited, but there also some other relevant articles by Young) have some discussion of the former debate, and Paul Edelman (2006) has an interesting argument about why the Court discourages the reliance on averages, standard deviations, etc. Anyway, I was expecting a rich conversation that compared the meaning behind all of the malapportionment measures and how they relate to our multiple understandings of malapportionment. And, maybe – just maybe – make an argument for which is best measure for the United States and why. I understand that this would have been a heavy lift, but this is what I thought/hoped that I was going to get.

Instead, these seven measures are introduced, there is a little comparative discussion about them (but not too much), and then they are "historically" applied to the three institutions. I put "historically" in quotes, because – despite the text repeatedly referring to "historical data" and "historical perspective" – the empirical portions of the manuscript are not "historical". I fully understand needing to make some compromises especially with this kind of work, but on P. 9 the authors make two that undermine the value of this work being described as "historical". They are (as the authors note): the Hill method is used to calculate all of the House apportionments – even though is only applicable from 1930 on, and total state populations are used despite the 3/5th compromise.

Again, I fully understand why some compromises are made, but these deviate significantly from US history. Even though I do agree with the authors, that the conclusions would not be fundamentally changed with the actual historical data; the results would nonetheless be different. And, yes, these accommodations do provide a more direct (but ahistorical) comparable standard, but if that is what is wanted, then why not just use Monte Carlo simulations or something on three theoretical institutions that meet these criteria – that way, the n, at least, could be greatly expanded.

Finally, there two main conclusions: 1) some of the malapportionment metrics track similarly, and 2) the U.S. Senate has the worse malapportionment – to quote p. 17 "The U.S. Senate presents a more serious challenge to our understanding of the majoritarian principle of democracy." The first is fine, but it still doesn't provide much meaning. The second is odd – no one is surprised by this, precisely because the U.S. Senate was never supposed to represent people; it, of course, represent states. And, to use the outrageous, but by-design, malapportionment of the Senate as a reason to dismiss the malapportionment of the U.S. House seems to be comparing apples and oranges (e.g., fn1 and p. 14). The whole argument behind the Connecticut Compromise is that the equitable apportionment of the House is constitutionally meant to off-set the malapportionment in the Senate (Ladewig 2011 expands a bit on this).

There are few other points that I think are more minor, but are still important.

1. On p. 6, the authors note that "parliamentary constituencies has also adopted a TPD based measure, though widely differing thresholds, most far higher than the ones adopted in the U.S." I don't know the comparative cases, so I am not exactly sure what type of legislatures they are referring to. But for the US comparison, this statement seems partially true – but only for state legislative districts. My understanding is that the Court generally has a TPD threshold of 10% for intra-state malapportionment of state legislative districts. The Court's intra-state malapportionment threshold, though, for congressional district is basically 0: they need to be "as mathematically equal as possible". The Court has never ruled on inter-state malapportionment, though it did say that the intra-state argument of Baker seem to be applicable in the same way (I would argue that they apply specifically and directly to inter-state malapportionment, but that is me) – see U.S Commerce v. Montana (1991). When the argument was directly offered to the Court in Clemons v. U.S. Commerce (2011) (see www.apportionment.us for the briefs, etc), however, the Court declined to hear it (though, it did vacate the District's court ruling against the plaintiffs). The point is that if, say, the 30% threshold in Germany is for inter-state malapportionment in the federal legislature (which is what this manuscript is about), then we have no definitive comparison to the US except that the current 67% in the U.S. is well above the German threshold and the Court is mute.

2. This is more or an aside, but one issue with malapportionment studies is the lack of a common vocabulary. We all conduct many of the same tests and measurements, but we often call them different things. The authors, for instance, measure the Max/Min Ratio; so do I, but I have adopted the term Voter Equivalency Ratio (VER). It is not perfect, but it seems to more descriptive connotes – at least to me - what is being compared, why it matters, and is more similar to the "one person, one vote" mantra. For example, currently in the US the VER is 1.88, or one voter (individual) in Rhode Island is equivalent to 1.89 voters (individuals) in Montana.

3. On p. 7, the authors note that the Loosemore-Hanby Index is "closely related" to the Gallagher Index. But, the data analyses does not show that. Fn. 29 blames the admittance of states like Nevada, with incredibly small populations, as the reason for this. This may be true, but I'm not ready to completely buy it yet. It would seem that a similar distortion would be apparent, as stated on p. 7, in the Loosemore-Hanby Index; yet, the correlation is still strongly negative and nearly as much as the other measures. Also, the example of Nevada is a little misleading. Yes, in the 1860 Census it had 6,857 people, but it wasn't admitted to the Union until 1864; so, its 1870 Census population (which was much larger at 42,491 people – though, still the smallest state in the Union) was the first to actually get mathematically apportioned (well, as least as much as the 1870 apportionment was actually mathematically calculated). Also, I've just now calculated some of the Loosemore-Hanby and Gallagher numbers (based on how I think that you apportioned), and they seem different than those presented. Can you verify the numbers and describe the units of the y-axises?

4. On p. 9, the authors say, in part, that "after Amendment XXIII was ratified in the 1960s given D.C. three electoral votes (regardless of its population)". This is not quite right, though it has so far been accurate in practice. D.C. just cannot receive more EC votes than the smallest state. So, if that state population distribution became more even and all of the 1-seat states received 2-seats and if DC also had a large enough population, it could also receive 4 EC votes.

5. On P. 14, the authors state "But the Max/Min ratio, in contrast, shows a cyclical pattern, albeit one with the present values still considerably higher than those in the United States' earliest history." This sentence 1) demonstrate my above hesitation to call this actual historical analysis, and 2) I'm not sure what is meant by a "cyclical pattern". Are the authors' suggesting that there is some sort of natural rhythm to the state populations and their changes over time?

6. On P. 16, the authors say that these two measures [TPD and Max/Min Ratio] "focus on the same two states (the largest and smallest)". Later in the paragraph they argue that "the reduced correlation in the Senate between the two measures is due, we believe, to the admission of extremely small state into the union in the mid nineteenth century". This is certainly part of it. But, as this paragraph is worded, it seems to me that the authors may also be confusing how these two measures work in the House versus the Senate. For example, in 2010, the MAX (over-represented) state in the House is Rhode Island (with a State Ideal District Size of 527,624), and the MIN (under-represented) state is Montana (with a State Ideal District Size of 994,416). The House Max/Min Ratio, thus is = 1.89. But, for the Senate the MAX (over-represented) state is Wyoming (with a State Ideal District Size of 284,150), and the MIN (under-represented) state is California (with a State Ideal District Size of 18,670,995). The Senate Max/Min Ratio, thus is = 65.71. In other words, the Max and Min states are often different for the House analyses and the Senate analyses. This difference in the trends are due, at least as much, to this.

7. On P. 17, the authors argue that "the discrepancy between popular vote outcome and EC outcome that occurred in 2000 and 2016 cannot be blamed on an increasing EC malapportionment in recent decades." This is clearly true for the 2016 presidential election, but not necessarily true for the 2000 election. The only non-constitutional way to significantly decrease malapportionment in the House is to increase the size of the chamber. And, as the authors cite, Neubauer and Zeitlin (2003) show that a House size of just about 600 seats or more would have given Gore the presidency – even with FL going to Bush. Taagepera's cube-root of the population for determining the size of the lower chamber would have put the House at about 629 seats in 2000. So, if the malapportionment of the House was seriously addressed before the 2000 election, it is likely we would have had a President Gore.

8. The Figure 1s and the Figure 2s both have the same titles. This and the very limited description of what is being presented in the Figure 2s (on P. 21), leave me … still … wondering what the Figures 2s are. The text says that it is "the same data" and the patterns look the similar, but what is the difference. Why the 0% line with trends above and below it? Are these standardized or what?

In sum, I fully support the idea of this manuscript. From the Title and Abstract, I didn't get the essay that I was hoping for – but I also realize that I can't ask the authors to write the paper that I would like. Still, there are a number of parts in this draft that should be (and can reasonably be) fixed. Some adjustments to the framing of the essay, the descriptions, and the analyses – which should also provide the authors with more of an argument & conclusion as well – should bring the manuscript much closer to being able to be published at journal like SSQ.