

FW: [External] Your Submission to the British Journal of Political Science: Decision

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Date: Sunday, July 16, 2023 at 4:17 AM
To: Solt, Frederick <frederick-solt@uiowa.edu>
Subject: [External] Your Submission to the British Journal of Political Science: Decision

Ref.: Ms. No. BJPOLS-D-23-00349
Macrointerest Across Countries
British Journal of Political Science

Dear Frederick Solt,

We have now received two reviews on your letter “Macrointerest Across Countries” submitted to the BJPS. One reviewer is very positive and one reviewer is more skeptical.

I would like to offer you the opportunity to address open points.

The reviewers all raise reasonable points and I would like to ask you to respond to all of them. Here are some thoughts on R2’s points: Point 1 is hard to fully address within the word limit but you should try and provide a little bit more. Point 11 is not possible as we do not have the procedures in place.

While I think that a revised letter could be a very good publication for the BJPS I also would like to point out that there is no quasi-automatic acceptance. There is always the risk that a manuscript is rejected after revisions.

If you decide to revise the article, please preface the revised manuscript with a brief memo (not (much) longer than five single-spaced pages, please) detailing how you have dealt with each of the suggested revisions. Highlighting the changed parts in the manuscript will make it easier for the reviewers and myself to see all the changed and new parts. Please also provide a word count (counting everything except reference section and appendix).

Best wishes,
Lucas Leemann
Associate Editor BJPS

Reviewer #1: # Review "Macrointerest Across Countries"

I have reviewed the manuscript titled "Macrointerest Across Countries" and find it to be concise, technically sound, informative, and highly valuable for the research community. I believe it presents an excellent use of the letter format. However, I would like to offer a few points for your consideration before publication.

1. **Other Approaches to Macrointerest**: It would be beneficial to discuss how your approach compares to other studies, such as Peterson et al. (2022), in terms of the countries studied and the differences in methodology more explicitly and outside of a footnote. Specifically, you could highlight the contrast between the dyad-ratios model and the model used in your study (maybe in the Appendix). If feasible, including a comparison of the macrointerest estimates with those of Peterson (2022) in the Appendix would further strengthen the contribution of your work.
2. **Interpretation of Macrointerest Values**: The appendix mentions using a logistic function to transform to scale the latent variable on the unit interval. An alternative approach could be considered. Instead of using the logistic function, you might explore utilizing the cumulative distribution function (CDF) of the normal distribution to scale the responses on the unit interval. This would allow for easier interpretation as percentiles of the normal distributed macrointerest distribution (e.g., 0.6 representing the 60th percentile). But maybe I am mistaken. It would be valuable to discuss whether the logistic function provides an equally valid interpretation, or if it has any limitations.
3. **Two-Step Approach to Test Hypotheses**: Your use of the hierarchical within-between model effectively demonstrates the utility of your approach. However, it may be worth exploring whether this could be achieved in a single step by integrating hierarchical priors into the model. If implementing such a modification is not straightforward, you could discuss it as a potential point of consideration in the conclusion or as a footnote. Additionally, providing more detail or clarification on how uncertainty from the measurements is incorporated into the brms model would be helpful.

Minor Points:

- * Consider to restructure the introduction into **three paragraphs** to address the relevance of Macrointerest, the challenges faced by previous research, and the specific contribution and findings of your letter.
- * Clarify the statement regarding the use of the DCPOtools R package for automated data collection and avoid data-entry errors. Provide more information on which part of the data collection process is automated and how the package is utilized. **I would even suggest to move this to the appendix** as it is not essential for the research letter.
- * In the sentence mentioning DCPO modeling, there appears to be a **copy-paste error** with the term "civil servants." Please review and correct as needed.
- * Footnote 1 in the appendix is **the same as** Footnote 2 in the main text. It may be preferable to remove one of them to avoid duplication.
- * Consider revising "impressively well in validation tests" to simply "well in validation tests" since the tests' standards are not **explicitly high**.
- * Also provide clarification on how **the uncertainty in the measures** is accounted for in the estimation of correlations.
- * Consider **rephrasing** "good times versus bad times" to a more precise statement, such as "increasing economic growth, not worsening economic growth" or "good economic times, not bad ones," to accurately reflect the analysis' findings.

* The meaning of "and so makes a poor choice for cross-national data (see Caughey, O'Grady, and Warshaw 2019, 686)" is unclear. It may be best to **remove this sentence**, or rewrite it.

Reviewer #2: The research note estimates country time series of "macrointerest", i.e. country-year level of interest in politics, provides some evidence of the validity of this time series, and analyzes correlates of macrointerest distinguishing between effects of over-time changes and between-country differences in the hypothesized causes of macrointerest.

Being up to date with the techniques used in the paper, I can safely say that the analysis is state of the art. While the paper does not propose new methods, it uses most recent developments in dynamic cross-national latent variable models to estimate aggregated interest in politics (instead of the outdated dyad-ratio algorithm), and appropriately models the resulting time series in a way that accounts for measurement uncertainty. I think this is the only paper I have seen so far that accounts for measurement uncertainty in the income inequality (SWIID) measure. From the point of view of the analysis, the paper sets a solid standard for others to hopefully follow. I also found the paper well-written and well-illustrated with informative graphs and supplementary materials. Still, I do have a few concerns regarding other elements of the paper.

1. I think the theoretical discussion of the relevance of studying political interest (or macrointerest) could be **substantially strengthened**. The main argument seems to be that political interest is a form of political engagement, and political engagement is important, so political interest is also important. But does political interest convey any other information than other forms of engagement? Is political interest important because it predicts political behavior? A stronger theoretical discussion would clarify the concept, justify theoretical expectations and make the results for informative. Without it, it is hard to evaluate the significance of the subject for political science research.

2. The paper does a great job discussing how the DCPO model deals with differences in measurement characteristics of the political interest items in different survey projects. What about the other part of the Total Survey Error (and, consequently, comparison error) framework, i.e. those related to representation? The surveys included in the analysis differ in terms of definitions of the population of interest (e.g., minimum and maximum age, whether various minorities are included in the population, exclusion of some territories) and in the sample design (probability and non-probability samples, with some, especially older, surveys relying on quota samples or random route samples without enumeration), to name just a couple. Some surveys document sample coverage. E.g. the documentation of Latinobarometro states that some samples cover less than 20% of the given country. In some surveys, weights are available to correct from deviations from sample representativeness based on a few selected socio-demographic characteristics. In others there are no weights, or weights are not accompanied by adequate documentation. **The paper completely ignores representation errors**, while biased samples can have a direct impact on the quality of estimates of macrointerest: e.g., under- or over-representation by education will bias mean estimates of interest in politics, as education is a strong predictor of political interest.

3. I am not sure that I learned from the **validation exercise**, because evaluations of the size of the correlation coefficients between macrointerest and (a) other measures of political interest, and (b) theoretically related concepts, seem arbitrary. Is $r = 0.71$ really high, if it's a correlation between two differently processed versions of the same data? I'm not saying that it definitely is not high enough, but because of the lack of benchmarks, this number is not really informative. $r = 0.89$ for the ESS part seems high, but this is likely because ESS has more surveys and had more influence in the overall macrointerest estimates. For small projects with few surveys, the correlation is smaller, which questions the utility of the method.

4. Validation is based on known associations between political interest and e.g. news consumption and others, on the individual level. It is not clear that these individual-level relationships translate, or should translate, to the macro level. Consequently, it is hard to evaluate the observed correlations as **consistent with expectation**.

5. The time series of Allbus and Macrointerest don't actually seem to covary so well. Most notably, macrointerest

has a sharp dip in 2000, which is absent from the Allbus time series. This is of course concerning, as one would expect smoother trajectories for the macrointerest time series, given that ideally with more data sources some errors would cancel out. This discrepancy should be investigated and explained e.g. by checking which survey/project causes the deviation **in the two time series**.

6. The application tests theories of **political participation** and how they explain macrointerest. To me this looks like an attempt to check whether macrointerest is similar or different from other forms of participation/engagement. I would have liked to see this discussed in the conclusions.

7. Why are only 37 OECD countries included in the analysis? The main advantage of creating the country-year estimates based on multiple survey sources is to broaden the scope of research beyond the well-surveyed countries. Restricting the analysis to **just 37 countries**, and ones that are the most frequently surveyed, contradicts this purpose. Why not just take ESS data, which include most of the OECD countries, and save ourselves all the harmonization effort? I think the otherwise interesting analysis does not very well demonstrate the unique research opportunities created by the harmonized macro-interest time series. This unique opportunity is, to my understanding, to compare countries which do not participate in the same survey projects.

8. I am not sure that **single-country and single-wave** projects should be included, if the model relies on estimating between-country and over-time "conversion rates".

9. Based on Figure A1 it looks like the least frequently surveyed countries in the dataset (Kosovo, Azerbaijan, Cambodia) will likely have huge uncertainty around the estimated trajectory of macrointerest. I think the appendix should show the trajectories for all countries (an extension of Figure 4 to all countries), so readers can assess the benefits of having very **rarely surveyed countries in the analysis**. E.g. Puerto Rico has surveys from 1995, 2001 and 2018, so there is a lot of imputation going on there.

10. Figure A1: Why is **Northern Ireland** separate from the United Kingdom?

11. I would have liked to see, already at the review stage, a replication package with all the code, from reading in the publicly available survey data, to obtaining the final tables and figures.

12. "statistically defensible" sounds like an attempt to talk about statistical significance using non-frequentist language. **Perhaps** using posterior probabilities instead of credible intervals would be a better option for discussing results.

13. "Taken as a whole, this evidence indicates that it is good times, not bad ones, that yield higher macrointerest." (p. 12) sounds a bit like an overstatement, given that only the time-varying effect of GDP per capita predicts macrointerest, and none of the other time-varying **economic variables**.

14. Table A1: abbreviations of names of survey projects are not explained. I was mostly able to make out their meaning, **but not for all** (e.g., autnes?).

15. **Figure A1** is hard to read and should be split it into two parts.

16. **Figure 5** is nice, but I would have liked to see the full regression table too.

17. **The paper should provide references** to all survey datasets used in the analysis, including their versions. This will take many pages, so should probably be put in the appendix, but I think it's important to (a) give credit to the data creators, and (b) document data versions for the readers and potential replicators.