

Interactive comment on “Mid-Holocene climate change in Europe: a data-model comparison” by S. Brewer et al.

Anonymous Referee #3

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The authors use statistical techniques to assess the skill of various PMIP participant models in simulating mid-Holocene climate. I am not an expert in these techniques, but they seem new and interesting and may provide a more quantitative approach toward evaluating model skill.

General comments:

1. Technique

- (a) The techniques and much of the terminology presented in this paper are ***not*** common ones used in paleoclimate studies, which puts a burden on the

authors to describe their research specifically to the audience of this journal, and justify the use of a new (to the field), more complex technique.

- (b) The authors *must* present enough background information in this paper for the reader to assess the knowledge gained by the application of a more obscure approach.
- (c) The authors set up their study saying that their approach is better than previous ones, but do not follow up on this assertion in their discussion.
 - i. What insight beyond other (simpler) methods is obtained?
 - ii. In the model-data comparison section (as it is in the title, should be the crux of the paper) one whole paragraph (of only 3 total) in the discussion is done 'by-eye' - isn't the point of the paper to use this new technique to do the comparison?
 - iii. Is the way that this technique deals with uncertainty in the proxy data really better than other techniques?
 - iv. Can they provide a comparison to a standard assessment (mean-proxy versus mean-model) to their approach and to illustrate some of their assertions ("A model that is able to simulate an enhancement of the monsoon but in the wrong location should perform better than one without ... " : it would be useful to specifically illustrate this point with the models and data used here)?

2. Model-Data Comparison

- (a) This study uses PMIP2 data - and does not have some of the required references
 - i. It is worth mentioning how other patterns of change typically ascribed to paleo-data in Europe relate in this new analysis... does one of the clusters look like a NAO+/- pattern, or is this a mute question since the two things cannot be compared?

- ii. In particular, Gladstone et al 2005 talks about paleo-model NAO comparison... this seems pertinent background... likewise the Masson-Delmotte et al 2006 paper, etc...
- (b) Scaling: The comparison of very local paleo-data to model and data and finding: are these two variables really comparable? How does this dealt with in this cluster analysis?
- i. The authors discuss a bit in section 4.1 on how adjacent proxy data imply a different 6K climate signal... but it would be even more useful if they followed up with how this 'quite noisy' data affects the cluster analysis.
 - ii. What happens when the model grid box says (1 deg cooler) - one proxy point says .2 cooler, the other says .7 warmer... : Is there an average data cluster response for each grid box at the various resolutions? The discussions says that it the cluster analysis would underestimate agreement... so, how is this better than a visual comparison?
 - iii. P. 1165-6: it is not clear how an interpolation would really negatively impact the comparison, particularly if one of the data sites is clearly influenced by some sub-grid-scale phenomenon not realized in model grid boxes (like pollen changes in a lake on top of major topography, etc.) Couldn't the authors try doing this and see what happens?
- (c) Error
- i. The estimated error in the proxy data is not the same as the intrinsic variability of the model.
 - ii. Paleo-data is 6000+/- 250 years, while PMIP1/2 data is 50 years... is the standard deviation comparison between the two datasets equivalent since the intrinsic/ inter-annual variability (what the models give) need not equal the centennial-scale variability (what the paleo data gives)...

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- iii. Why would one expect these two to be equal? More discussion on this point is merited.

3. Presentation

- (a) The paper is too narrative in parts, without providing additional insight.
- (b) The intro should probably be broken up into separate sections, or statistical techniques should appear in their own section ... the second paragraph p. 1158 is long and rambling, where it could be providing important insight into this study
- (c) Referring to "cluster 1" or "cluster 2" in the discussion section is somewhat confusing, and requires too much of the reader to recall which one is which (referring to a pattern by title 'cluster 1' is fundamentally different than referring to - for instance - a NAO+, or other, pattern, which has a meaning beyond any single study). The authors need to find another concise way of referring to these patterns.
- (d) 'Meaning' without excess terminology (something like: when this parameter increases, it implies that the pattern/correlation/... is robust/bad/etc.) should be ascribed to the various statistics; e.g., what does it mean if the Hagan distance is large; what does it mean (p. 1161) if the gain was less than 0.05? What is a centroid?
- (e) Is there an alternative description/title for the "Inflated climate changes" the authors refer to, and could they make it more clear how they obtained these?
 - i. The conclusions finally clearly puts forward that two tests were made... it would help if section 2.3 had mentioned that this was coming...
 - ii. The method for doing this is mentioned in passing on p. 1164 - perhaps a bit more discussion on how this was done would be useful!

- (f) Parts read like rote instructions without providing insight into how this particular step is crucial: I suggest either omitting the extraneous, or giving detail into why each step is useful.
- (g) The results section could be reworked a it to avoid what seems to be figure captions (e.g., p. 1164: ‘...the open symbols show the ...’)
- (h) Has a cluster analysis been performed on the modern data? What does it reveal? (see below... perhaps an extra techniques section)
- (i) A narrative of how a technique is applied is more useful when there are equations to follow
- (j) Section 2.4 would be improved with a few extra sentences in the beginning about what cluster analysis is, and why it will be useful here.
- (k) Abstract could be reworked for clarity

Minor comments:

1. p. 1156: How does a better simulation of orbital changes relate to a better simulation of GHG changes?
2. p1156, line5 only 3 parameters available?
3. P1156, Line 7: direction => sign
4. P1157, have there been any PMIP2 model-data comparison studies?
5. P1157, line17: precedent => previous
6. P1157, l20: Can't high resolution be averaged into low resolution?
7. Whilst (arcane) where ever it appears should be changed => while, despite, even though, etc.

8. P. 1157-8: style: avoid 'should' => does not perform better in a K est..."
9. P. 1164: Some language is too casual: ' we wish ...'
10. P. 1164: line 17-19: belong in figure caption
11. Are all acronyms defined?
12. Figure 5: would be clearer if the perfect fit could be illustrated as well as mentioned in the caption.

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