

## ***Interactive comment on “Large-scale features and evaluation of the PMIP4-CMIP6 midHolocene simulations” by Chris M. Brierley et al.***

### **Anonymous Referee #2**

Received and published: 11 February 2020

Review of manuscript entitled “Large-scale features and evaluation of the PIM4-CMIP6 midHolocene simulations, by Brierley et al.

This manuscript presents the very recent simulations performed within the PIM4-CMIP6 project and aiming at reproducing the climate of the mi-Holocene (6000 years ago). This climate was characterized by specific external forcing (greenhouse gases concentrations) and orbital parameters, which have been prescribed in a consistent way in the different models, following a previous study (Otto-Bliesner et al. 2017). At the time of the publication, twelve models have performed these simulations and are analyzed here, which is equivalent to the number of models having performed the previous coordinated exercise. The manuscript proposes a first analysis of four specific features of the midHolocene climate: surface temperature, monsoons, extratropical

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hydrological responses and the ocean circulation. A final section discusses general assessments of the midHolocene climate.

The paper is in general very well written and presented, and figures are of high quality. References are thoroughly cited. This paper is clearly an important milestone for the PIMP4-CMIP6 intercomparison process, benchmarking the publication of the simulations and probably calling for subsequent more original analysis. I only have a few major comments and a series of minor ones listed below. I want to indicate to the authors that I am not a paleo-expert, I come from the climate modeling community, typically rather focusing on the historical and future periods.

Main comments:

1. On the calendar adjustment( section 2.2)

- I am surprised that this calendar adjustment has to be performed offline. Is there a fundamental reason that the protocol does not include a change of the online calendar?

- Could you provide a comparison of the effect of the calendar adjustment for a variable for which it is proven useful (temperature?). Here, you only give numbers for the monsoon system, which you claim is not suited for this adjustment.

- It sounds illogical to me to assess (in this case to reject) the reliability of the calendar adjustment for the monsoon system using a fixed domain (lines 95-98) and just after (lines 99 and following) explain that the spatial extent of the monsoon varies so that the latter has to be adjusted for future assessments. Could you justify or modify?

2. Section 3.5 is relatively confusing.

- L. 304-307: I have to say that I don't understand the point that is made here. First, grammatically, I don't understand the "they" in L. 305. And logically, I don't understand the link of what is said here with the beginning of the paragraph.

- L. 309 "noisy" would be more accurate than "chaotic" I think.

- Paragraph beginning l. 320: I find this paragraph difficult to follow and perhaps not so well organized:

o L. 322-323: it is not clear to me how Fig. 1 shows that the protocol is responsible for the detected differences.

o The fact that “there is no inherent relationship between climate sensitivity and seasonality is repeated twice (l. 324 and l. 326) with no obvious demonstration in-between.

o It could/should be said more explicitly that Fig. 11 is just an example and that checks have been performed for other features of the midHolocene climate features (have they?)

o I don't fully understand the last sentence of the paragraph: in what sense could it provide a constrain then?

#### Minor comments

- l. 129: I don't know what rechrine (212) is, please explain.

- lines 35-38: the increased climate sensitivity in the CMIP6 generation of climate models is still under investigation and I think it is worth specifying: add “The reasons for this increase is still under investigation, but it may influence also the sensitivity of the models to the midHolocene external forcing.

- End of section 2.1: on the specificities of the protocol and the models differences, I would like to advertise the site <https://es-doc.org/> that provides a detailed documentation of all these points, with a full documentation of each model.

- L. 138: as “as compared to piControl conditions” after “Europe” and specify Fig. 1a.

- L. 155-156: From Fig. 1, I am not completely sure that I can state that “the change in MAT with respect to the piControl in the PIMP4-CMIP6 ensemble is less than in the PIM3-CMIP5: I see indeed a weaker warming at high latitudes but a stronger cooling in the tropics. Please correct or clarify.

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- L. 166: I would suggest naming explicitly the C20 reanalysis in the brackets.
- L. 170-171: as shown in Fig. 1(e), the PIMP4-CMIP6 models are generally colder than the PIMP3-CMIP5 ones. The reduced warm bias along the EBUS could simply be a consequence of this. Thorough analysis of the evolution of this bias would require the use of relative temperature (e.g. Hourdin et al. 2015, GRL)
- L. 175: I don't understand how this correlation coefficient is computed.
- L. 190: I would rather use the term "intermodel spread", as in several other places of the manuscript. Standard deviation typically relates to temporality in my view. Changes in the legend of Fig. 1, 2 and 6 are also needed.
- L. 233-235: the term "the changes of precipitation" is sometimes confusing: I think you speak of the changes of each PIMP exercise with respect to piControl, but one could also read as the changes from PIMP3 to PMIP4. I suggest clarifying.
- L. 239: remove the word "indeed"
- L. 250 this sentence does not mean much to me. Large spread with respect to what?
- L. 277 I would rather write " a decline of up to  $\sim 15\%$ "
- L. 280: It should be made clear that the magnitude of the AMOC computed at  $50^\circ\text{N}$  in z-coordinates is a little bit misleading, since in fact intense transport of water mass occurs at similar depths, but with very different densities (e.g. Zhang GRL 2010).
- L. 286: I don't clearly understand the link that is made with the magnitude of low frequency internal variability in AMOC. Clarify.
- Conclusion section will have to be changed accordingly to previous remarks (on section 3.5 in particular
- L. 365: I have no proof at this stage that the changes in the implementation of aerosols in CMIP6 is an improvement.

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