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Climate of the Past Discussions

Interactive comment on "The PMIP4-CMIP6 Last Glacial Maximum experiments: preliminary results and comparison with the PMIP3-CMIP5 simulations" *by* Masa Kageyama et al.

Anonymous Referee #1

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First of all, I apologise to the authors for my late review.

The article presents preliminary results of the new PMIP4-CMIP6 exercise, compare it to the former PMIP3-CMIP5 simulation ensemble, along with new LGM temperature databases for continental and oceanic temperatures. It is an important and interesting update of the LGM experiments, well organized, and provides a state-of the art on recent advances in climate modeling and data compilation. It is very descriptive but I enjoyed very much reading it. Sometimes, figures are not easy to read, but I don't see how it could be improved given all the dimensions the model/data comparison requests. I recommend publication with some minor rectifications that do not, in my

opinion, require any further round of review.

I list these minor remarks below, in order of appearance in the article, and do my best to help the authors dealing (or not dealing) with them as fast and easily as possible.

Introduction:

Lines 74-77: those two sentences imply that a shallower NADW & expanded AABW seen in data are not compatible with an increase in AMOC seen in models. My understanding is that both are not necessarily incompatible. It is e.g. discussed in Sigman et al., 2010, Nature (doi:10.1038/nature09149) in the chapter 'polar ventilation of the deep ocean', please check and verify that using the formulation 'This is at odds' is appropriate here.

3.3. Hydrological cycle:

Figure 6 indicates a large zonal changes btw PMIP4 & PMIP3 in the equatorial zone with increased (decreased) precip in PMIP4 wrt PMIP3 around Indonesia and Atlantic sectors (equatorial Pacific and western Indian ocean). It is complex, I agree, but I find this pattern very interesting, could you discuss a little bit more such pattern?

Sentence lines 282-284: again this feature is very interesting. Do you have any idea about what could cause that at first order? (shifts in ITCZ patterns, Sunda-Sahul shelf shelf exposure, ice sheet geometry?)

4 Data-model comparison:

I think you should immediately start by emphasizing that the new data reconstruction overall agree better with model outputs than the Bartlein and MARGO ones.

Lines 294-295: it seems that summer temperatures at high latitudes as estimated in the old and new datasets have nothing in common, please point it out.

Lines 318-319 (whereas the currently available PMIP4-CMIP6 simulations tend to be warmer than the reconstructions.): I don't really see that

Line 323: iLOVECLIM, did you mean AWIESM?

Lines 328-329 (However, the simulated change in winter temperature is smaller than indicated by the reconstructions (Fig. 12, top line).): not everywhere when the new datasets are considered

Lines 365-373 & Figure 13: Why such a figure? To me, it only shows improvement of Tierney in how the SST databasemight be representative if models get it right. I am not sure this last figure and paragraph is really helpful here.

Conclusion (The MARGO (2009) data set does not provide a strong constraint on the upper limit of the cooling because no model simulates warmer temperature anomalies than these reconstructions.): even if I completely agree with you, you could not say that the MARGO data are wrong because models can't reproduce it. Please reformulate.

'Volodin 2018' in Table 1 is missing in the references

Figure 3: correct the caption 'same as Figure 1' (not 2). Also, is there something going on in the Walker circulation?

Figure 7, top left: please add the zero horizontal line, this will help

Figure 9: please add the 1:1 line and indicate which axis corresponds to which dataset

Figure 12: I wonder if is it technically manageable (and useful to the reader) to make a clear figure with the same Y axis for the same regions (and/or seasons), so we could appreciate the magnitude of the differences btw them?

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