

Geosci. Model Dev. Discuss., referee comment RC2  
<https://doi.org/10.5194/gmd-2022-49-RC2>, 2022  
© Author(s) 2022. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## Comment on gmd-2022-49

Anonymous Referee #2

---

Referee comment on "Transient climate simulations of the Holocene (version 1) – experimental design and boundary conditions" by Zhiping Tian et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2022-49-RC2>, 2022

---

Review of the paper by Zhiping Tian et al. on Transient climate simulations of the Holocene (version 1) – experimental design and boundary conditions. The paper describes a model configuration for Holocene climate using CESM1.2.1. Compared to other PMIP participants it exploits a surprisingly high resolution of  $\sim 2^\circ$  in the atmosphere and  $1^\circ$  in the ocean and does not employ any acceleration techniques. Using this setup the authors conducted some transient runs from 11.5ka until the pre-industrial era.

I like the paper in general but feel that the authors need to add more substantial material before it can be accepted by GMD. Hence I recommend the authors to resubmit the paper after significant revision.

- The introduction leaves an impression that the authors know their topic and what they are doing. However, it consists of 3 parts and takes more than 30% of the paper. I see the authors need to make it more concise and keep only the relevant for the motivation parts.
- I agree with the first reviewer that the model description lacks some details. Which parameterisations are used in the ocean and atmosphere, which coefficients etc. all this is not there. The presented setup does not really follow the PMIP4 protocol, doesn't it? I also wonder how the ocean was initialized for the spinup?
- line 295: Is it a standard practice in PMIP? I expect the effect is enorm!
- For a GMD paper on the model setup description it would be necessary to present the model performance, scalability and throughput for different model components

distinguishing between IO, ocean and atmosphere costs etc.

- My main comment is that the preliminary results are limited by one page and one plot which sounds a bit poor (10% of the paper). In the discussion of Fig.5 the authors try to interpret the drivers of Holocene climate GMST anomaly by using the additional 4 experiments where they sequentially turn off different boundary conditions. If I interpret Fig. 5 correctly I see that the anomalies in these 4 experiments do not sum to the reference experiment (HT-ALL) containing all boundary conditions. This points to the high level of nonlinearity in the system and reduces the confidence of the discussion in the results section.
- I would like to see some key diagnostics from PMIP4 and how the HT-ALL experiment aligns with the other models. As an example one could look into Chris M. Brierley 2020 (<https://doi.org/10.5194/cp-16-1847-2020>, 2020) and do the comparison.