EGUsphere, referee comment RC1
https://doi.org/10.5194/egusphere-2022-1281-RC1, 2023

Comment on egusphere-2022-1281

Anonymous Referee #1

Referee comment on "The hydrological cycle and ocean circulation of the Maritime Continent in the Pliocene: results from PlioMIP2" by Xin Ren et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1281-RC1, 2023

This manuscript examines simulations of the mid-Pliocene from the PlioMIP2 models. The authors focus on the mean climate features in the maritime continent. They find a warmer and wetter mid-Pliocene climate, with a lower sea surface salinity and in general a stronger Indonesian Throughflow (ITF) in the PlioMIP2 simulations. The author also explore the use of multi-cluster mean in summarizing multiple model results and noted the advantage of this method over the traditional multiple model mean.

The manuscript is very well-written and easy to follow. The manuscript fits well with the scope of *Climate of the Past*. I have a few comments that need to be addressed before publication. Please see details below.

- Results on ITF are not well connected with the rest of the manuscript. In other words, why should we care about the ITF in the Pliocene simulations (considering that we do not have proxy data to provide sufficient constraints on the model results)? In the current form of the manuscript, ITF is described separately from the SST and hydroclimate variables. Although, in the introduction, the author did cite literation on how the ITF is linked to coupled ocean-atmosphere variability and how the ITF may influence the monsoons. However, the authors results on ITF do not make any of the connection or mechanistic analysis. Given this disjoint, I am wondering whether the author should consider cutting the ITF results and focus on the regional SST and hydroclimate over the Maritime Continent instead.
- In the Discussion (Section 4.3), the authors stated that "but even models of the same model family may still produce different climatic signals depending on the analysis region or the studied climate characteristic." Can you provide explanation for this interesting result? Is it because of the potentially different model resolution, or details of the boundary condition implemented by different authors, or internal variability?
- Are there available proxies on the hydroclimate (precipitation /evaporation and sea surface salinity) and ITF in the region? If yes, please include results and discussion on these comparisons. If no, please state it explicitly in the manuscript (that there is no available proxy for benchmarking models).

- Please consider adding a summary of model-proxy comparison of SST in the abstract.

Minor comments

- Lines 23–25: Rewrite and change into "A large amount of rainfall releases large quantities of latent heat into the atmosphere, which is an important driver of global atmospheric circulation".
- Many of the multi-panel plots are not labeled with subplot label (such as (a) and (b). Please check and make sure all the subplots are properly labeled.
- Information should be provided on how the ocean salinity was initialized in the simulations. This information is needed because the authors examined the sea-surface salinity changes in the PlioMIP simulations (e.g., Figure 5d), and it is not clear whether the ice-volume effect has been accounted for in the simulations and has an imprint in Figure 5d.
- Line 266: "the relationship is not exactly linear."
- Figure 10: cluster 5 (GISS) looks weird. The model resolution is ~2 degree (Table 1). It is hard to believe the precipitation anomaly has such a rich fine structure. Please double check and make sure calculation has been done correctly.