Comment on egusphere-2022-40
Anonymous Referee #2

Referee comment on "Mid-Holocene climate of the Tibetan Plateau and hydroclimate in three major river basins based on high-resolution regional climate simulations" by Yiling Huo et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-40-RC2, 2022

Review comments on EGUSphere-2022-40

Mid-Holocene climate of the Tibetan Plateau and hydroclimate in three major river basins based on high-resolution regional climate simulations
by Huo et al

Using WRF simulation, the authors tried to explore the changes to the river-headwater hydrological regimes on the TP during the mid-Holocene period. They found that dynamical downscaling enhances regional climate simulations over the TP in modern-day and MH climates and highlighted that they could overcome the cold biases, a typical issue across the Himalayas and TP region. The study demonstrated orbital factors' role in the seasonal precipitation cycle. Overall, the study is nice; there are some potentially fascinating points that they could have highlighted rather than simply summarizing the known MH climate.

Recommendation: Minor revision

1) According to the authors, the ocean component was modified to make it more acceptable for paleoclimate simulations. Is this taken into account in MH and PI simulations? That would be nice to discuss it briefly if so.

2) In Figures 5a & 5b, the authors attributed the changes to MH orbital and GHG forcings. So is this means the GHG forcing is different in MH and PI?
3) If the GS only caused a 20% difference in precipitation, is this coming from Saharan vegetation changes via ocean-atmosphere teleconnections? Is there a significant difference in SST forcing with and without GS? If so, it is better to include a brief description of this in the manuscript.

4) The river basin analysis is interesting. However, the authors did not give this section much weight in the abstract. This could have highlighted instead focusing on other well-known MH features. However, this section is too elaborate as well.

5) The authors noted the need for sufficient resolution to simulate TP on page 14, line 445. Is that, however, a huge deal in a model? even at coarse resolution, GCM is adequate to depict TP properly to a greater extent because this is a big area. Many researchers also mentioned how the Himalayas and TP play a minor role instead. How will the authors address these opposing issues? If the study does not shed light on this topic, it is preferable to omit such extraneous descriptions rather than a casual sentence.

6) Again, the conclusion section also gave the least highlight to the quantifications over Riverhead regions.

7) The main point they suppose to express through the manuscript was land surface coupling and its importance. But they have not taken care of this part properly in the manuscript. This could have been brought more interestingly in the conclusion part.