Comment on egusphere-2022-40
Anonymous Referee #1

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-RC1, 2022

Comments on EGUSPHERE-2022-40:

Title: Mid-Holocene climate of the Tibetan Plateau and hydroclimate in three major river basins based on high-resolution regional climate simulations

Authors: Yiling Huo, William Richard Peltier, and Deepak Chandan

In this manuscript, Huo and coauthors conducted a series of dynamically downscaled high-resolution simulations to analyze hydroclimate responses over Tibetan Plateau (TP) under the Pre-industrial (PI) and mid-Holocene (MH) conditions with and without a green Sahara condition. In particular, results from a fully coupled global-scale climate model (the University of Toronto version of CCSM4) are downscaled to 10 km resolution using four different cumulus parameterization schemes in the Weather Research and Forecasting Model coupled with the hydrological model WRF-Hydro. The authors made great efforts to reproduce characteristics of the TP's hydroclimate in the WRF-Hydro of which spatial resolution is competent in representing orographic impacts on precipitation and its seasonal variability. However, the validation against historical observation and the demonstration of MH climate is insufficient. Nevertheless, the study could be the first step to simulate MH-TP hydroclimate change in a high-resolution regional climate model, and hence I recommend acceptance for publication after considering the following comments.

Major comment:
The land surface has significant impact on climate and hydrology. For example, Yue et al. (2021) found that different types of underlying surfaces affect the partitioning of sensible and latent heat fluxes, causing different local circulations and further impacting precipitation and temperature over the southern TP. Implementation of more accurate soil texture can lead to reduced biases in simulated soil moisture and impact simulated runoff and evaporation (De Lannoy et al., 2014). In the manuscript, the same land surface on the TP was used in both PI and MH simulations. During the Holocene, Chen et al. (2020) revealed that the maximum forest extent was reached in the MH. That may have some impact on climate and hydrology. To some extent, the evolution process of vegetation on TP should be considered. Li et al. (2019) has already reconstructed pattern of vegetation evolution for China since the Last Glacial Maximum by pollen dataset. Therefore, given the main goal of this study, it is necessary to consider changes in the land surface of TP during the MH too.


**Minor comments:**

- The definition of TP in the Introduction is inconsistent with the WRF inner domain in the main text, which is misleading. Please clarify this as well as the relationship between the TP and the WRF inner region.
- The authors provided a proper data-model comparison regarding precipitation to assess the performance of the experiments. Since there is also abundance of temperature records in the studied area and temperature is an important atmospheric parameter for
hydroclimate (Zhang et al., 2022; Kaufman et al., 2020), it is necessary to include it in the comparison.


- There is no doubt that WRF is competent in simulating regions with complex terrain than GCM. However, the biases between WRF and observation are obvious in the simulation of temperature and precipitation. Authors shall adequately discuss this weakness and its potential role in their results.
- The results in Section 4 are too detailed and unfocused, it is hardly to catch the points. Can you shorten the results to be more readable? Section 5 also exists the same problem. Please briefly summarize the conclusions.
- The CRU dataset is selected as observation dataset to verify the results in historical period (1980-1994). However, it is hard to say that CRU has a well performance in describing the precipitation on the TP. A more convincing dataset or evidence showing the validation of the CRU should be mentioned in the manuscript.
- Lines 99 and 221: Please confirm the expression “and. Since” and “half. Reconstruction”.
- Rewrite the last sentence of Abstract.
- Lines106 and 212: Please show the full name before using the abbreviation.
- Lines 145-147: The description way is weird here. Can you give a better way? For example, the dynamical downscaling methodology employed here is a somewhat further developed version of the dynamical downscaling “pipeline” originally introduced in Gula and Peltier (2012) and then widely applied in recent studies.


- The interval of color bar is too large to indicate the anomalies between simulations and observation in Fig. 2 and Fig. 3. Please redraw the figures.
- The legend of “WRF1/2/3/4” in the figures might be replaced by WRF and the abbreviation of cumulus parameterization or specific name.
- Given that the line of “WRF ensemble” in the figures is overlayed by the lines of single experiments, it is hard to define the relation among different lines sometimes.