

Review of WCD-2022-52 by Astrid Fremme et al. 2022

This manuscript aims to examine how the hydroclimate of the Yangtze River Valley changes along different climates, from the last glacial maximum to the present, and then under a future projection scenario. This fact is based on the comparison of the moisture sources for the target region. The authors used two model outputs from climate models CAM5.1 and NorESM1-M and ran the FLEXPART model trying to obtain the moisture sources for the Yangtze River Valley. ERA-I was also used.

While the exercise of using the different datasets to run the FLEXPART model is novel, there are several major concerns about the modeling, the data used, calculation, comparison, and interpretation that lead to many doubts to this reviewer; many of them related to the robustness of the statistical analysis of this work.

I believe substantial modifications are necessary and thus I would reject this manuscript if no major changes are made.

My main comments and reasons for this decision are:

1. My biggest concern is the robustness of the statistics. Throughout the paper, there is no statistical analysis for the comparison of the simulations carried out for the present time (control period). It is needed an annual and seasonal comparison between the dataset used and derived variables from the FLEXPART outputs for the control periods. A visual comparison of the obtained fields, which are shown in the manuscript as maps, is not sufficient to conclude whether the moisture sources are similar or not. A statistical study is necessary. Typical statistics are used in this type of analysis: mean absolute error (MAE), root mean square error (RMSE), Pearson's correlation (R), Bias (B), standard deviation (STD), and coefficient of determination and variation.
2. My other big comment is about the selection of the length of the period to compare. What is the reason for the selection of these 10 years (in the table appears that is 5 years...)? Does decadal variability play some role in the results obtained? The author comment that the period is neutral, but in the selected period as control exits a strong ENSO event, 1997-98, and during the decade the sign of the Pacific Oscillation (POD) was the same, and it is known that the East Asia Monsoon precipitation is affected by ENSO, but the POD strengthens this relationship when positive. The Indian Ocean Dipole, which is the role of this mode over the region? If the hydroclimate variability and changes along the time (past-present-future) of a region where the EASM affects is the goal of this paper, it needs to be into account as the other modes commented. The authors should use 30 years as usual for climatological studies.
3. To resolve these issues, it is necessary to extend the study period as much as possible, compare a longer period, and do statistical analysis.

Overall, a more critical discussion of the limitations and uncertainties of the study seems necessary.

Without these questions (methodological and analytical issues) resolved, this reviewer cannot consider this manuscript for publication at this stage.

Other aspects need more attention by the authors:

4. The input data for running FLEXPART model have different characteristics in terms of vertical and horizontal resolution. Is the number of particles modeled the same in the different experiments? If they are the same, the model preconditions are different, and this fact could affect the results and the interpretation of the field comparison. This needs to be checked and explained in the manuscript.
5. The authors comment that the thresholds in WaterSip were changed, and the selected RH% , for instance, was determined for NorESM dataset. Why not other data or reanalysis? Could this affect the outputs of the model when used for the future? The diagnostic for the imposed thresholds was done only for one year! Which year? Why this one? If the year selected was another, could the threshold change? ... How this affect the results?
6. Only one emission scenario, RCP6.0, was used in the study. Why? RCP6.0 represents an intermediate scenario, and it is not very used in climate change studies. What about RCP8.5?
7. Many of the comparisons in the manuscript, such as those derived from figure 3, etc..., were in absolute values. 1.0 mm/day seems a small quantity, but it is not small if the variables to compare have a range from 4 to 8mm/day. Better in %.
8. There are many typo errors in the manuscript (table, figures, captions, ...). Please check the manuscript carefully when the revision is done.

This review has further comments on the results and their interpretation, but these are not necessary at this stage of the review until the above is resolved.