Response to the Comments of Referee 1 on the paper "Mid-Holocene climate of the Tibetan Plateau and hydroclimate in three major river basins based on high-resolution regional climate simulations" by Yiling Huo, W. R. Peltier and Deepak Chandan

We thank the referee for his/her valuable comments on the content of our manuscript and his/her suggestions for improving the document. Following the reviewer's suggestions and comments, we have carefully revised our manuscript. We believe that the revised version satisfactorily addresses the referee's questions and concerns. In this reply, we respond to the issues, raised by the referee point by point. Our responses to the individual comments are shown in red text following the comments in black. For convenience, the modifications made to the text will also be shown in red.

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.

Many thanks to the reviewer for this overall positive feedback, which we appreciate. We agree that the readers will likely find our paper new and interesting. We have also taken care to strengthen the validation against historical observation and the demonstration of MH climate.

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.

Yue S, Yang K, Lu H, et al. Representation of Stony Surfaceâ€• Atmosphere Interactions in WRF Reduces Cold and Wet Biases for the Southern Tibetan Plateau. Journal of Geophysical Research: Atmospheres, 2021, 126(21): e2021JD035291.

De Lannoy G J M, Koster R D, Reichle R H, et al. An updated treatment of soil texture and associated hydraulic properties in a global land modeling system. Journal of Advances in Modeling Earth Systems, 2014, 6(4): 957-979.

Chen F, Zhang J, Liu J, et al. Climate change, vegetation history, and landscape responses on the Tibetan Plateau during the Holocene: a comprehensive review. Quaternary Science Reviews, 2020, 243: 106444.

Li Q, Wu H, Yu Y, et al. Large-scale vegetation history in China and its response to climate change since the Last Glacial Maximum. Quaternary International, 2019, 500: 108-119.

We agree with the referee that the land surface has significant impact on climate and hydrology, but the purpose of our paper is to isolate the impact of the Green Sahara on the hydroclimate impacts on the TP. Thus, adding the regional impacts of surface vegetation changes in China on MH hydroclimate changes on the plateau would detract from the main focus of this study. Furthermore, the suggested data set for the mid-Holocene vegetation of China (Li et al., 2019) seems not to be publically available online yet. However, we still added references to Chen et al. (2020) and Li et al. (2019) in the last paragraphy, where our original manuscript already addressed the influences of Eurasian forests during the MH and proposed possible future work.

Minor comments:

1. 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.

It is true that definition of the TP and our WRF inner domain are not the same, and we have already stated in lines 135 that “while the inner domain encompasses the TP, as well as parts of the surrounding territory”. We have now added “, which covers the TP, as well as some surrounding regions” at the beginning of our results section in line 179 to make it clearer.

2. 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.

Zhang C, Zhao C, Yu S Y, et al. Seasonal imprint of Holocene temperature reconstruction on the Tibetan Plateau. Earth-Science Reviews, 2022: 103927.

Kaufman D, McKay N, Routson C, et al. A global database of Holocene paleotemperature records. Scientific data, 2020, 7(1): 1-34.

Thank you for suggesting we compare our simulated temperature with proxy data. We've added comparison to temperature records from Kaufman et al. (2020) and Zhang et al. (2022) in Fig. 4 and lines 223-235:

"Moreover, compared to paleoclimatic reconstructions based on lake sediment in Fig. 4 (Zhang et al., 2022; Kaufman et al., 2020), both MH experiments generally capture the summer warming trend over the TP. In south-eastern TP, there is a good agreement between the model simulations and the reconstructions. The simulated temperature anomalies in MH_{GS} fit the reconstructed paleoclimatic records in the north-eastern and south-central TP better but overestimate the warming signal over the central-eastern part of the TP (Fig. 4d). Meanwhile, the two proxy data points located in the central-western TP west of the 85° W indicate strong cooling ($< –4$ ◦C) during the MH, which disagrees with all model simulations. Note here these two records are from frozen lakes, where the reconstructed temperature records reveal air temperature changes during the ice-free season (May - September), not just JJAS. Also note here all proxy records are subject to uncertainties that arise from the dating processes (Wang et al., 2021) and site-specific factors like the change in vegetation cover and the retreat of glaciers (Chen et al., 2020a). Averaged over all the points except these two, both UofT-CCSM4 and the WRF ensemble average have a bias of –1.2 ◦C in MH_{REF}. Inclusion of a GS greatly reduces this cold bias to –0.1 ◦C in WRF, which is also smaller than that of the global model in MH_{GS} (–0.4 ◦C)."

Chen, X., Wu, D., Huang, X., Lv, F., Brenner, M., Jin, H., Chen, F.:Vegetation response in subtropical southwest China to rapid climate change during the Younger Dryas, Earth Sci. Rev., 201, p. 103080, https://doi.org/10.1016/j.earscirev.2020.103080, 2020a.

Wang, M., Hou, J., Duan, Y., Chen, J., Li, X., He, Y., Lee, S. Chen, F.: Internal feedbacks forced Middle Holocene cooling on the Qinghai-Tibetan Plateau, Boreas, 50, 1116-1130, https://doi.org/10.1111/bor.12531, 2021.

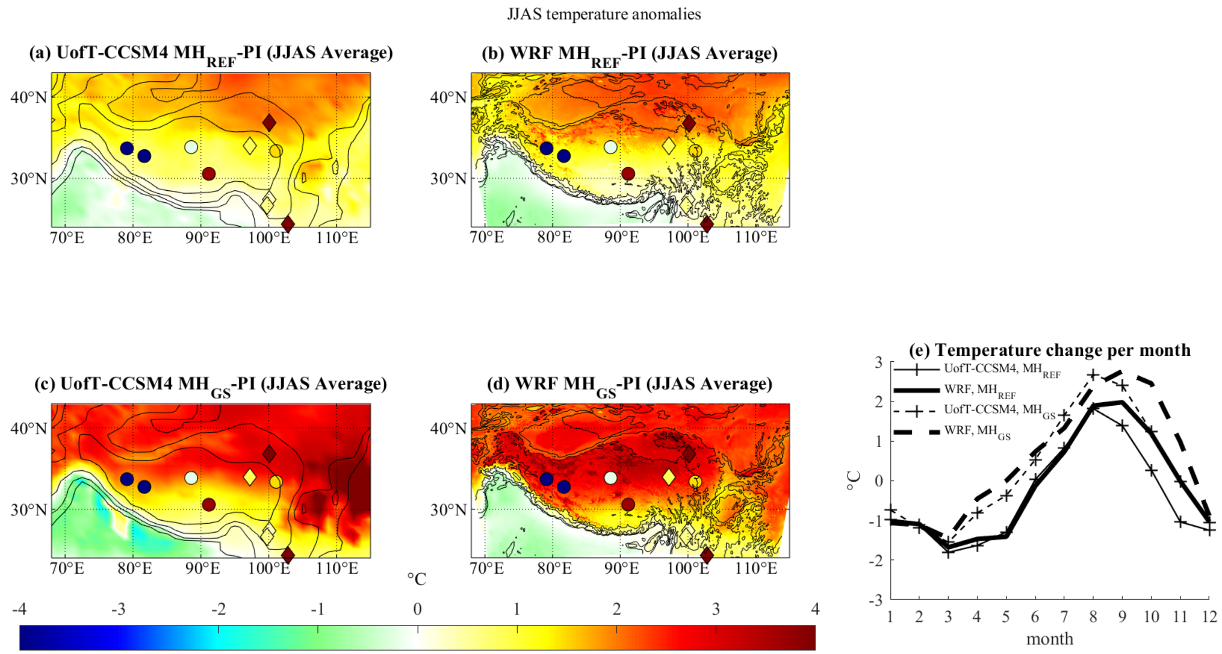

Figure 4: JJAS surface air temperature anomalies (◦C) for (a, b) $\mathrm{MH_{REF}}$ and (c, d) $\mathrm{MH_{GS}}$ in (a, c) UofT-CCSM4 and the (b, d) WRF ensemble mean. (e) Monthly continental air temperature anomalies for $\mathrm{MH_{REF}}$ (solid) and $\mathrm{MH_{GS}}$ (dashed). Reconstructed temperature differences between the MH and present day from Zhang et al. (2022) and Kaufman et al. (2020) are plotted as circles and diamonds, respectively. The topography contours (black) of 500, 1000, 2000, and 4000 m are shown in (a-d).

3. 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.

We have now added some discussion regarding the influence of temperature and precipitation biases in winter. In lines 185-186, 190-191 and 202-205, we now state:

“Most of the UYEB and UYAB are affected by this warm bias, which will likely reduce summer streamflow due to increased evapotranspiration.”

“Among the three river basins, the UBB is most strongly affected by the cold bias in winter, which may decrease streamflow in the cold season due to late snowmelt.”

“The overestimation in precipitation over the western and south-eastern TP in WRF is also accompanied by lower surface temperature, which is likely related to the greater cloud cover reflecting more shortwave flux at high levels. Both models show a similar wet bias in winter (Fig. 3d) and such excessive snow in winter possibly contributes to the lower DJF temperature (Fig. 2) through snow–albedo feedback.”

4. 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.

We have reduced section 5 by around 15%. We have also restructed section 4 and shortened it by around 1/3.

5. 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.

Thank you for this suggestion. We now instead use APHRODITE’s (Asian Precipitation - Highly-Resolved Observational Data Integration Towards Evaluation) gridded precipitation dataset version 1101 (Yatagai et al, 2012), which covers Monsoon Asia (APHRO_MA_V1101) at 0.5◦×0.5◦ horizontal resolution. It is a set of long-term (1951 onward) continental-scale daily products based on a dense network of rain-gauge data for Asia including the Himalayas, South and Southeast Asia. However, using this new dataset didn’t significant change the results in our original manuscript (Fig. 3). Comparison of the model results and CRU precipitation in JJAS during the historical period has now been moved to Fig. A1.

Yatagai, A., Kamiguchi, K., Arakawa, O., Hamada, A., Yasutomi, N., and Kitoh A. APHRODITE: Constructing a Long-term Daily Gridded Precipitation Dataset for Asia based on a Dense Network of Rain Gauges, Bulletin of American Meteorological Society, https://doi.org/10.1175/BAMS-D-11-00122.1, 2012.

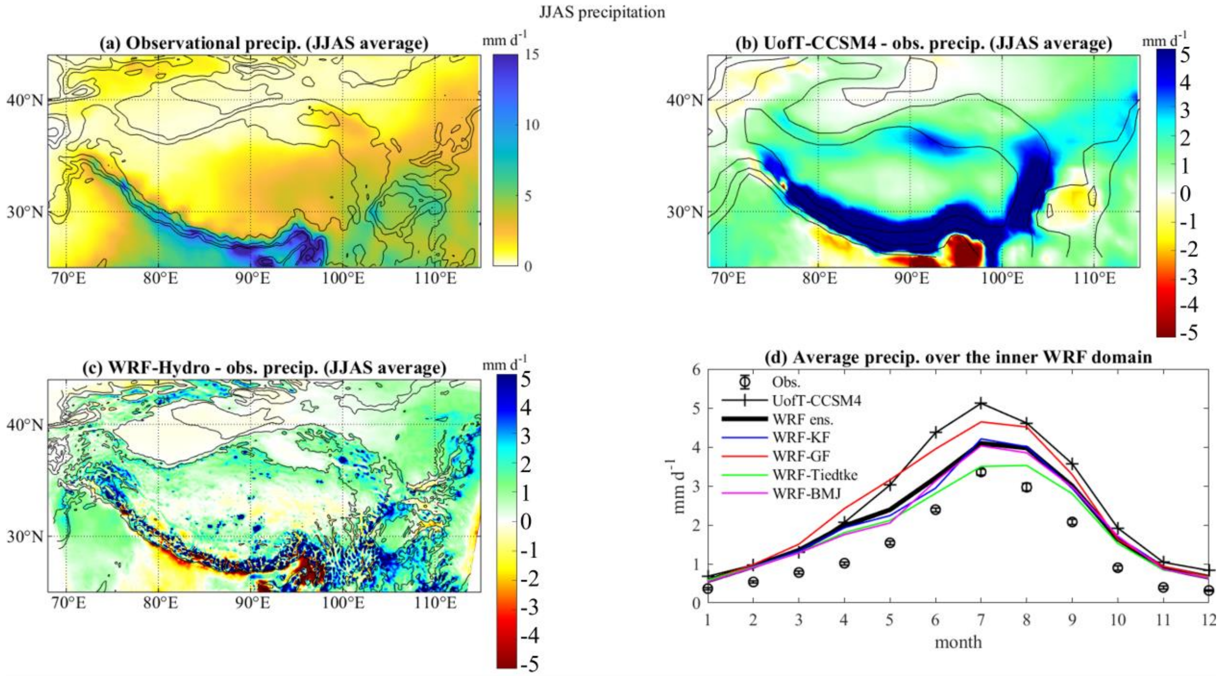

Figure 3: JJAS precipitation bias with respect to (a) the APHRODITE observational dataset for (b) the UofT-CCSM4 and (c) the WRF ensemble. (c) Monthly precipitation over the inner WRF domain in the observation, UofT-CCSM4, WRF ensemble average and four individual physics ensemble members. The topography contours of 500, 1000, 2000, and 4000 m are also shown in black in (a, b, c).

6. Lines 99 and 221: Please confirm the expression "and. Since" and "half. Reconstruction".

We apologize for the error in line 99, and we have corrected the text by removing "and". There was no error in line 221, but we have still adjusted the text from "further reducing the bias with respect to the reconstruction by Bartlein et al. (2011) by half." to "further reducing the bias with respect to the pollen-based reconstructions (Bartlein et al., 2011) by half" to be clearer.

7. Rewrite the last sentence of Abstract.

We have now rewritten the last sentence in the abstract:

"The simulation results were first validated over the upper basins of the three rivers before the hydrological responses to the MH forcing for the three basins were quantified. Both the upper Yellow and Yangtze rivers exhibit a decline in streamflow during the MH, especially in summer, which is a combined effect of less snowmelt and stronger evapotranspiration. The GS forcing caused a rise in temperature during the MH, as well as larger rainfall but less snowfall and greater evaporative water losses. The Brahmaputra River runoff is simulated to increase in the MH, due to greater net precipitation."

8. Lines106 and 212: Please show the full name before using the abbreviation.

We apologize for these errors, and we have corrected the text by stating "upper Yellow, Yangtze and Brahmaputra River basins (UYEB, UYAB and UBB, respectively)" in line 106 and using the full name "greenhouse gas" instead of "GHG" in line 212.

9. 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.

Thank you for this suggestion. We have now changed the text as suggested.

10. Gula J, Peltier W R. Dynamical downscaling over the Great Lakes basin of North America using the WRF regional climate model: The impact of the Great Lakes system on regional greenhouse warming. Journal of Climate, 2012, 25(21): 7723-7742.

We are not quite sure what the reviewer meant by this comment. This paper is cited and included in the reference list of the original manuscript.

11. 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.

In accordance with the referees' wishes, we have now adjusted the colorbar of Figs. 2 (below) and 3 (see under comment #5).

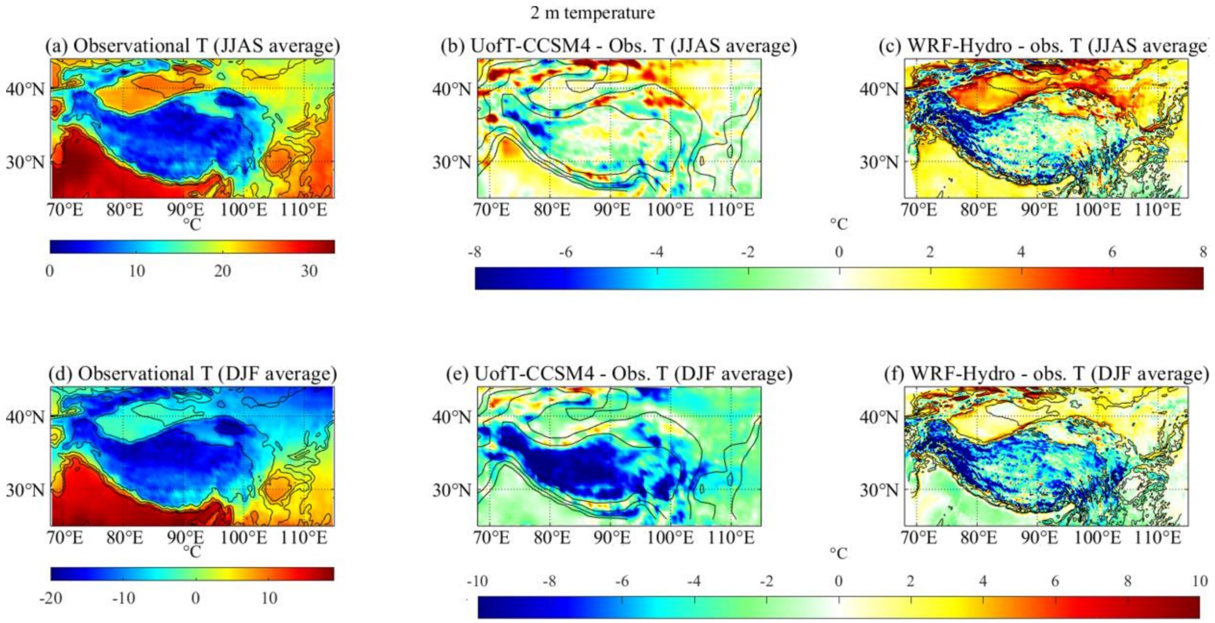

Figure 2: Absolute 2-m air temperature bias with respect to (a, d) the CRU observational dataset for (b, e) the UofT-CCSM4 and (c, f) the WRF ensemble in (a, b, c) JJAS and (d, e, f) DJF. The topography contours of 500, 1000, 2000, and 4000 m are also shown in black.

12. The legend of “WRF1/2/3/4” in the figures might be replaced by WRF and the abbreviation of cumulus parameterization or specific name.

In accordance with the referees' wishes, we have now changed the legend of “WRF1/2/3/4” in the Figs. 3, 6 and 8-10 to “WRF-KF, WRF-GF, WRF-Tiedtke, WRF-BMJ”.

13. 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.

We changed the axis scale to fit the axes box more tightly around the data in Figs. 8-10. In other words, we tried to zoom in as much as possible so that the readers can view the different lines more clearly. However, the lines of “WRF ensemble” are still sometimes overlayed by the lines of single experiments and this simply means their results are very similar in magnitude. In Figs. 11 and 12, we now use error bands to show the distribution range for all ensemble members instead of separate lines for each ensemble member.

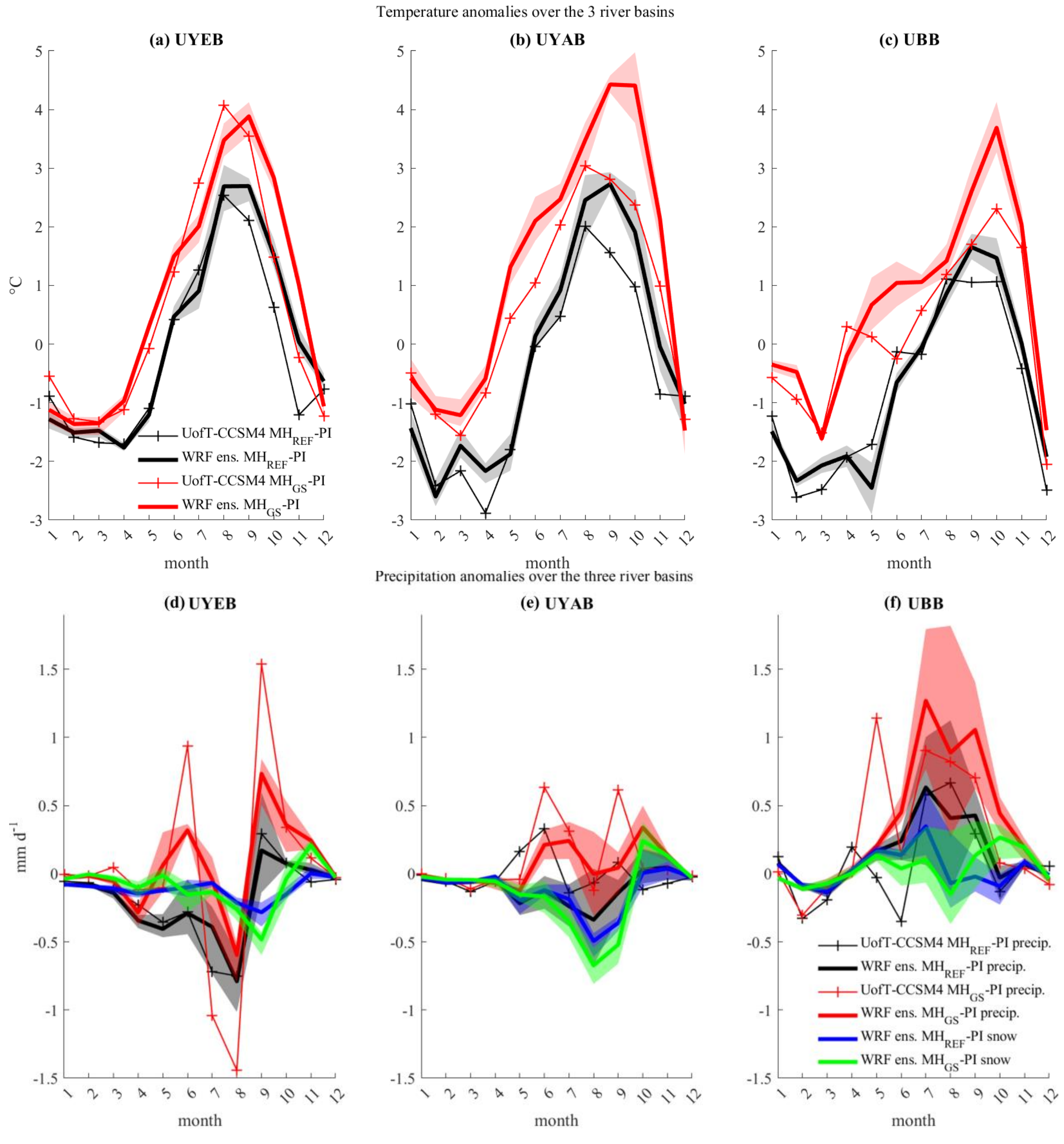

Figure 11: Monthly anomalies of (a-c) temperature and (d-f) total and solid precipitation over the three basins.

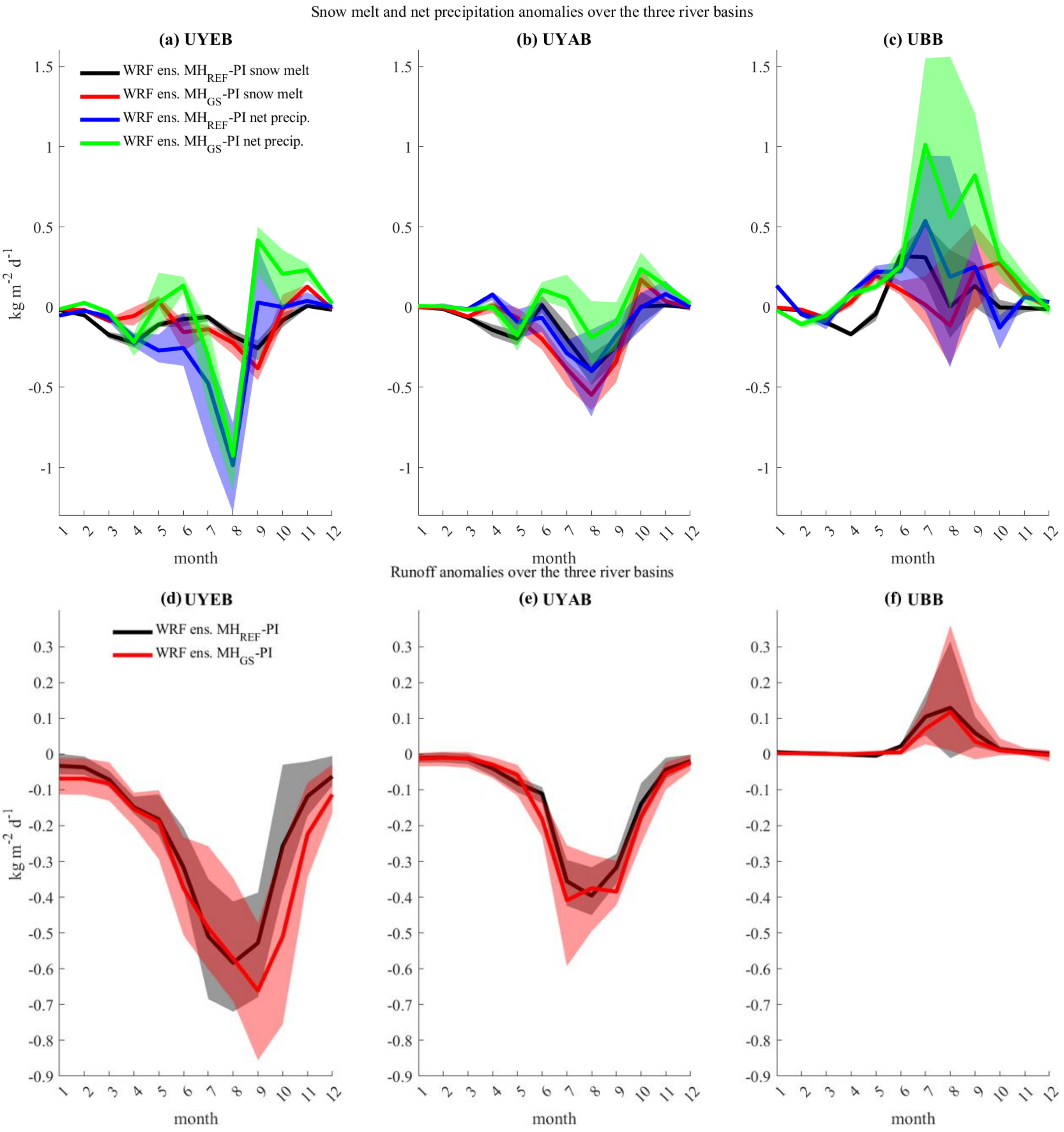

Figure 12: Monthly anomalies of (a-c) net precipitation and snowmelt and (d-f) total runoff from WRF over the three basins.