Response to the Comments of Referee 2 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.

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

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.

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.

Thank you for this suggestion. Lines 120-124 now state: "UofT-CCSM4 is based on the standard CCSM4 (Gent et al., 2011), but specific modifications have been made to the diapycnal diffusivity of the ocean component to make it more appropriate for paleoclimate simulations (Peltier and Vettoretti, 2014; Chandan and Peltier, 2017). These changes have been taken into account in both the MH and PI simulations to avoid introducing ambiguity related to different diapycnal mixing schemes when these two simulations are compared."

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?

Yes. We now stated in lines 129-130 that "Compared to the PI, the MH_{REF} experiments are forced by precessionally enhanced boreal summer insolation and slightly lower greenhouse-gas concentrations (Otto-Bliesner et al., 2017)."

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.

Yes. The presence of GS conditions in northern Africa shifts the Walker Circulation westward through changes in equatorial Atlantic SSTs and warms the Indian Ocean, which enhances the summer precipitation over Asia. Although a detailed discussion of the difference in SST forcing was presented in Huo et al. (2021) for the MH monsoons in South and Southeast Asia, we have chosen to highlight some of its conclusions here in accordance with the referees' wishes by adding one more figure and the following discussion illustrating the changes in SST and atmospheric circulation in the global model in lines 275-283:

"The enhanced precipitation in MH experiment with Saharan vegetation is probably owing to ocean-atmosphere teleconnections as suggested in previous studies (Huo et al. 2021; Pausata et al., 2017). An albedo-induced warming develops over the vegetated Sahara, leading to a strong intensification and northward expansion of the West African Monsoon and a significant tropical North Atlantic SST warming (Fig. 7b; Pausata et al., 2016), which in turn changes atmospheric circulation and induces a notable intensification and westward extension of the Walker Circulation over the Pacific Ocean in the MH_{GS} (Fig. 7d) compared to the MH_{REF} experiment (Fig. 7c). The changes in the Walker Circulation weakens the low-level easterly winds over the eastern Pacific, but enhances easterly anomalies over the northern Indo-Pacific Ocean (Fig. 7b), which suppresses ENSO activity and enhances the Asian monsoon (Pausata et al., 2017)."

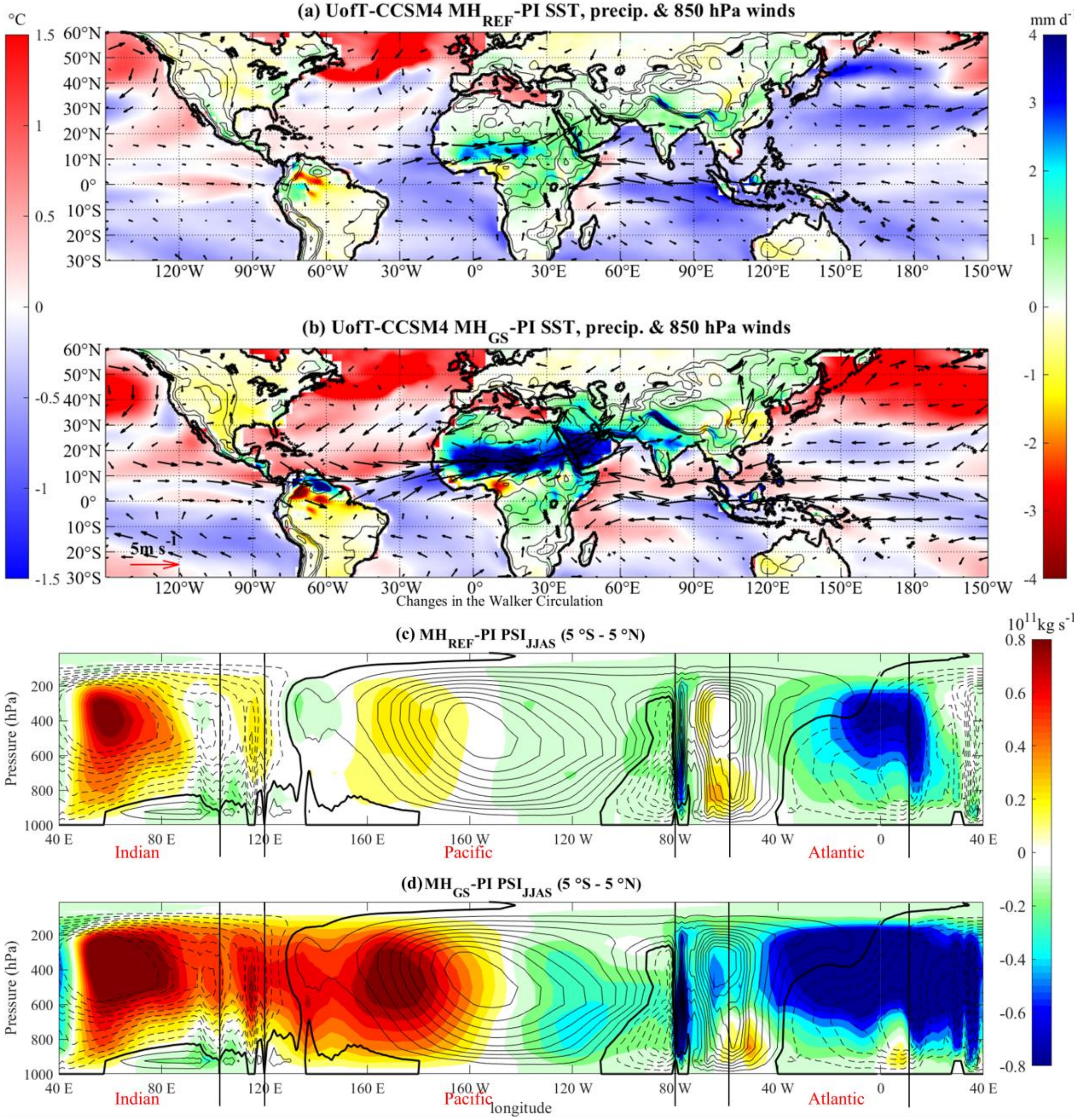

Figure 7: SST (shaded, ° C), precipitation (shaded, mm d^{-1}) and 850 hPa winds (vector, m s^{-1}) anomalies during JJAS from the UofT-CCSM4 for (a) MH$_{REF}$ and (b) MH$_{GS}$. The topography contours of 500 m, 1000 m, 2000 m and 4000 m are also shown. PI climatological zonal stream function of the Walker circulation (contours: 0.2 × 10^{11} kg s^{-1} interval from −2 to 2 × 10^{11} kg s^{-1}; 0 line in bold) and associated changes (shaded) in (c) MH$_{REF}$ and (d) MH$_{GS}$ relative to the PI.

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.

At the end of the abstract we now state:

"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."

We have also restructed section 4 and shortened it by around 1/3.

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.

The model resolution may have a great impact on the simulation of the regional climate over the TP. In our manuscript, significant such discussion will be found regarding how our regional model with higher resolution outperforms the coarse-resolution global model in terms of temperature and precipitation simulation when compared to observations during the historical period and proxy data during the MH. Has the referee missed these? Although we agree with the referee that some researchers argued that the South Asian summer monsoon circulation is unaffected by removal of the plateau, provided that the narrow orography of the Himalayas and adjacent mountain ranges is preserved, we are unaware of any studies that state both the TP and Himalayas are unimportant. Moreover, the focus of our study is the regional climate over the TP not South Asia. If there are specific references regarding the role of the Himalayas and TP on the TP regional climate that the referee believes we have missed, it would have been more helpful to have listed them.

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

In the conclusion, we rewrote the quantifications over the river basins in lines 433-441 and shortened the other parts:

"Both UYEB and UYAB hydrological regimes exhibit changes in the MH as manifested by a decline in streamflow, especially in summer. Such flow decreases are a combined effect of changes in snowmelt and evapotranspiration. A significant amount of solid precipitation shifts to liquid precipitation and the JJAS net precipitation is simulated to shrink in both MH experiments. The GS forcing caused a rise in temperature during the MH, as well as larger rainfall but smaller snowmelt and larger evaporative water losses compared to the MH_{REF}. In the UBB, the simulated annual total precipitation increase in the MH is the largest among three basins, and, unlike the other two basins, the most significant MH hydroclimatic anomaly over the UBB may be an increase in runoff in both MH experiments, particularly in mid-summer, due to greater net precipitation. The greening of the Sahara led to higher temperature and enhanced snowmelt in spring and eliminated the drop in runoff in April in MH_{REF}."

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.

We have added a figure and some more discussion regarding the forcing due to Saharan vegetation (see under comment #3). In the conclusion part, we now added in lines 428-430:

"Saharan vegetation plays a crucial role in intensifying the West African Monsoon and modulating the atmospheric circulation, which alters the Walker circulation and increases Asian monsoon precipitation, through changes in equatorial Atlantic SSTs (Fig. 7)."