Response to the Comments on the paper "Mid-Holocene monsoons in South and Southeast Asia: dynamically downscaled simulations and the influence of the Green Sahara" by Huo et al.

We thank the referee for his/her valuable comments on our revised manuscript and his/her suggestions for improving the document. Following the reviewer's suggestions and comments, we have carefully revised our manuscript again. We believe that the new version satisfactorily addresses the referee's questions and concerns. In this reply, we respnd 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.

The authors have addressed all comments and put a lot of effort into revising the manuscript. They carefully respond to all major issues raised by the two Referees. They shorten the Introduction, reduced the number of figures and concentrate on the main question of the effect of a Green Sahara on the precipitation in South and South East Asia. Results are more explained than in the first version and are quantitatively evaluated against reconstructions. The sensitivity experiments with different convection schemes are shifted to the Appendix.

The manuscript is very much improved and reads much better than the first version. I agree with the publication of this revised manuscript in Climate of the Past, but still have some minor/technical suggestions:

General minor comments:

a) The introduction reads much better now, but is still very long. Please carefully look again through the paragraphs and try to further shorten it. It would also help to delete some sentences… For instance, in L 41 you state that the MH insolation was different from present-day. In the sentence afterwards you further describe this. This sentence (starting with During the MH…) would be enough to understand the main background. The paragraph starting at L 45 with the reconstructions is very long and it does not really help to understand your paper. You could simply say, that the insolation changes intensified the NH summer monsoons (orbital monsoon hypothesis) and that palaeo-reconstructions generally confirm this view (different references…) and than go on with the "detailed knowledge is still..."

I have the same feeling with other paragraphs, that there is just too much information given that is not necessarily relevant.

We have removed the part starting at line 45 with the reconstructions and now stated that "Significant changes in the strength of the in Asian monsoon during the MH have been revealed by various paleoclimatic reconstructions, such as those based on palaeoceanographic evidence (Hutson and Prell, 1980; Prell, 1984a, b; Cullen and Prell, 1984; Prell and Van Campo, 1986), Tibetan ice cores (Thompson et al., 2000), Chinese Loess Plateau deposits (An, 2000; Porter, 2001) and stalagmites (Wang et al., 2001; Dykoski et al., 2005)."

b) I have to admit that I was not precise enough in my comment on the comparison with other studies. My apologies. The authors do indeed draw references to other studies. I'm just wondering if there aren't already other studies with regional models to compare the results with. For India, I remember a study with HIRHAM (Polanski et. al. 2012), dealing also with the mid-Holocene climate.

Reference: Polanski, S., Rinke, A., Dethloff, K., Lorenz, S. J., Wang, Y., & Herzschuh, U. (2012). Simulation and comparison between mid-Holocene and preindustrial Indian summer monsoon circulation using a regional climate model. The Open Atmospheric Science Journal, 6, 42-48. doi:10.2174/1874282301206010042.

Thank you for pointing us to this study with regional models and we have added reference to it in lines 213 and 234.

c) Unfortunately, there are very few reconstructions for South Asia. However, one could for example compare the model results with the semi-quantitative moisture reconstructions of Wang et al. 2010. I don't know if that dataset is available, though.

Does the referee mean this study: Wang, Y., et al: Asynchronous evolution of the Indian and East Asian Summer Monsoon indicated by Holocene moisture patterns in monsoonal central Asia, Earth-Science Reviews 103, 135-153, https://doi.org/10.1016/j.earscirev.2010.09.004, 2010.? If so, their data are not publicly available online and most of their data points lie in China and only four data points are in northwestern India close to the Himalayas.

However, we still added some comparison to their moisture reconstructions based on the figures in their paper in lines 219-220, 231 and 234-236.

d) Regarding the quantitative comparison with reconstructions: It would be helpful to include a Table, showing all MRE values for the MHref and MHGS simulations (regional and global model).

We thank the reviewer for this suggestion and we have added a table listing all MRE values for the MH_{REF} and MH_{GS} simulations.

Specific comments:

L 23: “SA” is not defined before

We have now added the definition of “SA” in line 10.

L 31: “monsoon” instead of “monsoons”

We apologize for this error, and we have corrected the text as suggested.

L 36: Do you really mean ‘Additionally’ or should it be ‘Therefore’

We have changed to “Therefore” here.

L43: ‘altered’ instead of “enhanced”, during winter NH insolation is reduced during 6ka

We have changed this to “altered” now.

L44: the $20W/m^2$, is it a mean over JJAS?

Yes. The average JJAS insolation increase is approximately 20 $W\ m^{-2}$ in our UofT-CCSM4 model.

L76: A nice overview of the AHP is given in: Claussen, M., Dallmeyer, A. & Bader, J. (2017). Theory and modeling of the African humid period and the green Sahara. In Oxford Research Encyclopedia of Climate Science Oxford University Press. doi:10.1093/acrefore/9780190228620.013.532

Thank you for this suggestion and we have added this reference in line 72.

L198: Do you mean Fig. 4c?

L215: Do you mean Fig. 5b?

We apologize for the above two errors, and we have corrected the text as suggested.

L220-222: Since there are no reconstructions you can not state which model is correct. Maybe the reduction in precipitation seen in the global model is correct, maybe the increase in the regional model, but who knows?

We have added here “, while wetter conditions were indicated by the semi-quantitative moisture reconstructions of Wang et al. (2010)” to make our statement clearer.

L250-L256: Please include at least a warning on the cave records. I still think that they do not recorder local precipitation (see. e.g. Lui et al, 2014, or Maher, 2008)

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Maher BA (2008) Holocene variability of the East Asian summer monsoon from Chinese cave records: a re-assessment. Holocene 18(6):861–866

We have added a sentence here: “Note here Chinese cave records have been interpreted by some studies to reflect not local MH rainfall changes but upstream monsoon rainfall or rainfall source changes (Liu et al, 2014; Maher, 2008).”.

L260: In South China, about 30% of the rainfall occurs in the month before the monsoon sets in. This is the problem in most GCMs, they overestimate spring precipitation and also the decrease in spring precipitation due to less insolation during spring at mid-Holocene…. The decrease in spring precip exceeds the increase in summer precip and thus, the South China is drier during mid- Holocene than today (in the GCMs)

We agree with the referee, and have added here “, except over south China, where a decrease is simulated in annual mean while JJAS rainfall is simulated to increase (Figs. 5b and 6b)”.

L271: During 6ka, perihelion occurs in September, so probably the overall insolation forcing was strongest during September, which may explain the strongest signal in precipitation simulated for September…

The referee is right to point out that perihelion occurs in September during the MH, but the local insolation forcing over SA and SEA is in fact the strongest in August (Fig. 1). The insolation change in UofT-CCSM4 is approximately 24 W m^{-2} in August and 16 W m^{-2} in September.

L278: It would be helpful to explain, why WRF-CROCO is more sensitive to the insolation forcing.

We added two sentences here to explain why the spatially-averaged JJAS precipitation increases produced by the WRF-CROCO ensemble mean are larger than those simulated by the UofT-CCSM4:

“Such rainfall intensification is probably related to a better representation of topography as the major wet anomaly centers in the downscaled simulation lie in the local mountain ranges over SA and SEA, including the Western Ghats, the Satpura Range in northern SA and the Garo-Khasi-Jaintia range in northwestern SEA. Moreover, the warmer SSTs over the Arabian Sea also lead to more evaporation and thus contribute to the enhanced wet anomalies, especially over SA.”

L345: Do you mean Fig 11e instead of 11k?

We apologize for the this error, and we have corrected the text as suggested.

L411-412: ‚including a GS'….'influence of a vegetated Sahara' → is the same, you can delete one of it

We have now deleted ", when the influence of a vegetated Sahara is taken into account".

L430 Appendix: It would be helpful if you include 1-2 sentences on the differences in the ensemble members and why you are performing ensemble simulations (It is in the method part, but I think it is helpful to repeat it here)

Thank you for this suggestion. We have now added a sentence here:

"Four different cumulus parameterization schemes (Tiedtke, GF, BMJ, and KF) are employed in the WRF model to form a mini-physics ensemble, which enables us to study the sensitivity of model performance to different cumulus parameterizations and thereby to estimate the uncertainty associated with these parameterizations on the simulated MH climate."

Fig.11: I think, the headings of the sub-figures are not correct. The Figures are mixed up. Fig. b) and c) rather look like 850hPa winds, and e+f like 250hPa wind fields. Please check!

The headings of the sub-figures are correct. The 850 hPa wind maps use a smaller reference value (the red arrow in the right bottom corner of each sub-figure), so the 850 hPa wind arrows in Figs. 11e and 11f appear to be longer than the 250 hPa wind arrows in Figs. 11b and c.