Response to the Comments of Referee 2 on the paper "Mid-Holocene monsoons in South and Southeast Asia: dynamically downscaled simulations and the influence of the Green Sahara" by Yiling Huo, W. R. Peltier and Deepak Chandan

We would like to thank the referee for his valuable comments on the content of our manuscript and his suggestions for improving the document. Following the reviewer's suggestions and comments, we carefully revised our manuscript. We believe that the revised version satisfactorily addresses the referee's questions and concerns. In this reply, we seek to clarify the issues, raised by the referee, point by point. Please find the detailed response (red) to the referee's comments (black).

I feel this is research is generally well-described and worthy of publication in Climate of the Past. The manuscript is predominantly descriptive, though I have no issue with that.

I do, naturally, have some suggestions that I feel would improve the paper which should be considered before publication. I do not feel that they will change any of the conclusions, but will help convince the reader of the validity of those conclusions.

1. The region SA and SEA are only shown on Fig. 3 and never formerly defined. I find it strange that SA is a square in the rotated grid of the RCM, meaning that it cross various latitudes over northern India. Given that you are using regions and acronyms close to those used by the IPCC, at a minimum you should also show those. In fact, I suggest that you deploy the AR6 regions from Iturbide et al. (https://doi.org/10.5194/essd-12-2959-2020) - codes are provided to calculate them by the authors. It is also import to state whether you are only looking over land, as in IPCC.

We have already stated in the manuscript "These two analysis regions are identical to the inner WRF domains in Huo and Peltier (2020, 2021) wherein two levels of downscaling were employed." In these previous studies, we have applied and validated the same dynamical downscaling pipeline over SA and SEA under modern conditions and thus we would like to keep the definition of regions consistent with previous studies. Also, it is not quite possible to use the AR6 regions from Iturbide et al. (2020) in this study as the Southeast Asia region in that study

covers Maritime Southeast Asia, which is outside of our WRF domain. Besides, the SA region in that study extend to the west to 60 ° E, which is very close to the west edge of the WRF domain. The WRF data there may suffer from relatively larger errors.

We now added at the beginning of section 3: "All spatially-averaged anomalies reported here are calculated over the land surface of the Indian subcontinent or mainland SEA south of the TP (the two black rectangles in Fig. 3)."

2. I suspect that if you replotted (some of) your figures as a raster rather than interpolated contours, the higher resolution of the RCM vs GCM will be much more obvious. (This could be done using imageshow rather contourf in python or CellFill in NCL).

Thank you for this suggestion. We have replotted figs. 4-9.

3. Might I suggest a different approach to the palaeo calendar issue. At present you do discuss this in the methods, but it suddenly is mentioned in the figure caption. Firstly, I suspect there little benefit to calendar adjusting for an average over JJAS - we found there was no need for MJJAS in Brierley et al (2020). However, the calendar effect would alter the seasonal cycle time series that you present. If instead you plotted this seasonal cycles from daily data instead, then the issue of defining month is irrelevant. You must have daily resolution data from the GCM (to drive the RCM). I think that plotting from daily data would be more useful to identify shifts in Fig 11.

Thank you for this suggestion. We have now replotted fig. 11 using daily data.

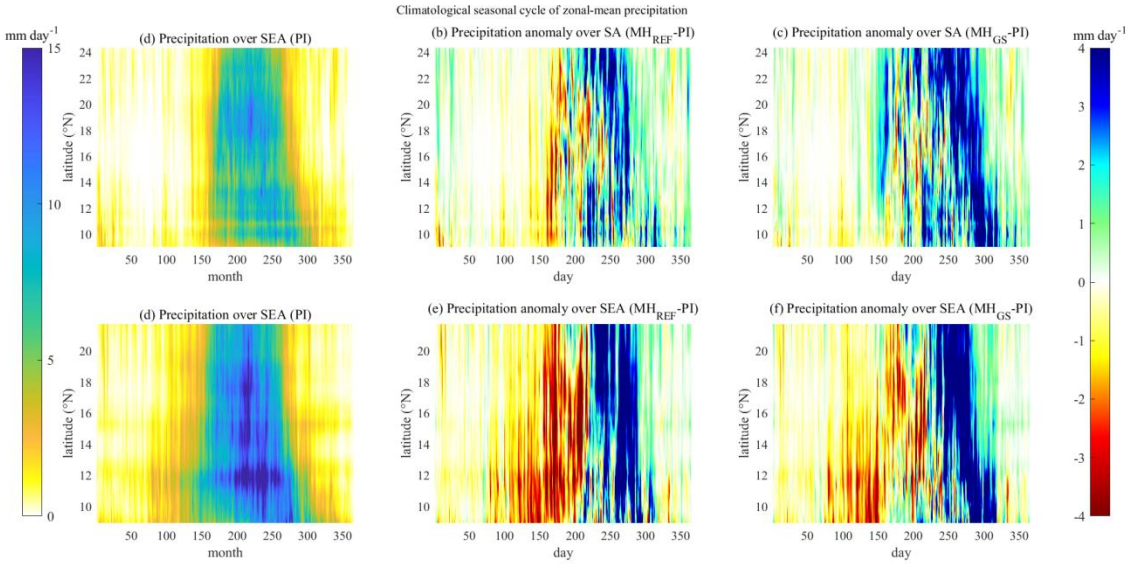

Figure 11: Climatological seasonal cycle of zonal-mean precipitation (mm day^{-1}) for the PI simulations of the first WRF-CROCO ensemble member over (a) SA and (d) SEA. Zonal-mean precipitation anomalies for (b, e) MH$_{REF}$ and (c, f) MH$_{GS}$ over (b, c) SA and (e, f) SEA.

4. Please can you be more explicit about the 15 years selected to drive the RCM simulations. Obviously ENSO would influence monsoon rainfall, can you reassure the reader that a different sampling on ENSO events is not responsible for the patterns described?

We agree with the reviewer that ENSO would influence monsoon rainfall and that could be a source of uncertainty. We have now stated in the conclusion that "Also note that the reported precipitation changes are subject to uncertainties associated with the parts of the global simulations that have been employed to force the downscaling ensemble. Since the precipitation results are strongly affected by large interannual variability associated with ENSO, further simulations with different initial conditions will be needed to characterize the internal variability and confirm the robustness of this result."

5. Some more information about improved representation of ocean upwelling the regional model would be useful to place the SST changes in context.

These results for the SST from the regional ocean model are supported by the higher resolution ocean dynamics

captured in the regional ocean model but a detailed discussion must be left for the ongoing work as the first referee has already requested that the number of figures in the paper be reduced. Huo and Peltier (2021) also had some discussion on the improved representation of ocean upwelling in the regional model under modern condition (Fig. 10 in Huo and Peltier, 2021).

Huo, Y., Peltier, W. R.: The Southeast Asian Monsoon: Dynamically Downscaled Climate Change Projections and High Resolution Regional Ocean Modelling on the Effects of the Tibetan Plateau, Clim. Dyn., in press, https://doi.org/10.1007/s00382-020-05604-9, 2021.

6. Can you please provide some context of the GCM and RCM resolutions with respect to the rest of the PMIP4 ensemble.

We have added "Note here most PMIP4 models have a resolution of approximately 1° (Otto-Bliesner et al., 2017), which is close to that of our GCM UofT-CCSM4 and is considerably coarser than the resolution of our regional model."

7. Table 1 is rather uninformative. Either scrap it or, preferably, include more synthesis of the different convection schemes.

This table has now been removed in the manuscript.