Response to the Comments of Referee 1 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 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 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. For convenience, the modifications made to the text will also be shown in red.

The authors use dynamically downscaled simulations to assess the impact of a vegetated Sahara on the South Asian and Southeast Asian monsoon region under mid-Holocene (MH) greenhouse gas (GHG) and orbital conditions. They couple the regional climate model WRF with the regional ocean model CROCO and drive this coupled model by output of the global model UofT-CCSM4 GCM. An ensemble of experiments is conducted using different convection schemes in WRF and different PI and MH boundary conditions.

Due to a better representation of the complex orography in South and Southeast Asia, the regional precipitation and temperature distributions are resolved in more detailed in the regional model. The MH forcing leads to an enhancement of the monsoon systems and an increase in precipitation in northern South and Southeast Asia. On the Indo-Chinese Peninsula and on the Tibetan Plateau, precipitation is rather decreased. The MH forcing furthermore leads to shifts in the monsoon season. In both areas, the monsoon onset is delayed and the withdrawal is postponed. Anomalies due to the MH forcing are generally more pronounced in the regional model than in the global model and show a better agreement to pollen-based reconstructions. However, both models are not able to capture the reconstructed mid-Holocene precipitation pattern in South China and the dry central Asian regions.

The incorporation of a vegetated Sahara enhance the precipitation response to the mid-Holocene orbital and GHG forcing and generally leads to a positive precipitation anomaly in South India, along the northern flank of the Tibetan Plateau and in North China beeing more in line with the pollen-based reconstructions. In addition, the simulations with Green Sahara only show a sligth shift in the monsoon season compared to pre-industrial times.

The authors have taken great effort to compare the regional and global model simulations. They visualise their results with many, easy-to-read illustrations, which are described in detail and comprehensibly in the text. Unfortunately, however, the analysis rarely goes beyond these descriptions. Results are not quantified and not analysed in detail. Also, with the large number of illustrations and descriptions, the main guiding question about the influence of the green Sahara on the monsoons in the regional model is lost. I also miss a comparison with results of model studies (regional and global models) that have already been carried out on the Asian monsoon during the mid-Holocene. Since the core question is very interesting and the study could make a major contribution to better understanding and quantifying the interactions between the West African and Asian monsoons, I still recommend considering publishing the manuscript. However, major revisions are needed.

Many thanks to the reviewer for this positive feedback, which we appreciate. We agree that the readers will likely find our paper new and interesting.

The referee also suggests that the importance of the main guiding question concerning the influence of the green Sahara on the monsoons in the regional model may have been somewhat obscured by the large number of illustrations and descriptions. In the revision, we have reduced the number of figures from 12 to 11 and, for the ones we keep, we provided a more quantitative presentation of our results**.** We have also moved description of the sensitivities of our results to the various cumulus parameterization schemes employed in our physics mini-ensemble into the appendix. In the main text, only ensemble means (both temperature and precipitation) from the simulations are discussed.

The referee seems to be incorrect in suggesting that we have not adequately compared our simulations to previous results of model studies on the Asian monsoon during the mid-Holocene. In our discussion significant such discussion will be found (lines 196, 213-214, 223-224, 300-301, 324-325). Has the referee missed these? If there are specific references that the referee believes we have missed, it would have been more helpful to have listed them.

Main comments to the authors:

a) I understand that the monsoon precipitation distributions may strongly be influenced by the convection scheme in the model, since most rainfall stems from convective cloud cluster. However, in the context of this study, a comparison of the simulations with the different convection schemes seems to me to be too extensive. It is more a , disruption ‘ than a significant contribution to underline the core message. Perhaps one could simply discuss an ensemble mean from the simulations in the main text and, for example, include the uncertainties in plots about the precipitation mean over the two regions. A comparison of the different simulations could then be presented in the supplement or appendix. Omitting the comparison would also help the paper to focus more on the main question.

We thank the reviewer for this suggestion and we have moved description of the sensitivities of our results to the various cumulus parameterization schemes employed in our physics mini-ensemble into the appendix. In the main text, only ensemble means (both temperature and precipitation) from the simulations are shown and discussed but that didn’t change any of the major conclusions.

b) It is useful to compare the results with palaeo-reconstructions. Since the pollen-based reconstructions cover a larger spatial variability, it makes sense to concentrate on these reconstructions and not to discuss the cave records. On the one hand, it is still not entirely clear what the cave records recorder at all (whether changes in wind direction or changes in precipitation), and on the other hand, they are located very unfavourably, precisely on the border between positive and negative anomalies in the model. In the meantime, there is also a new pollen-based data set by Herzschuh et al. (2019) that mainly covers China. It would be interesting to see whether the deviations from the models to the reconstructions also show up in a comparison with these new reconstructions. Please use a metric to quantify your findings. Just per eye it can hardly be seen that e.g. the regional model fits better to the reconstructions than the global model for Mhref.

(reference: Herzschuh, U. et al.: Nature Communications, 10: 2376, 2019 doi:10.1038/s41467-019-09866-8).

Thank you for suggesting we integrate the new palynological data into our paper. We have compared the results with data set by Herzschuh et al. (2019) in the revision. We also used Mean Relative Error (MRE) to quantify the goodness of fit between model results and reconstructions:

"To quantify the fit to the reconstructions, Mean Relative Error (MRE) is calculated using the following Eq(1):

$$\mathrm{MRE} = \frac{1}{n}\sum_{i=1}^{n}\frac{|p_i-d_i|}{|d_i|}, \tag{1}$$

where n is the number of data points, di stands for the proxy data, pi for the model prediction. Considering all the points that have proxy data (Bartlein et al., 2011) in the WRF domain, dynamical downscaling reduced MRE of the global model by 12%. For the points north of the TP, both the GCM and the downscaled simulation fail to simulate the same sign of precipitation anomalies as indicated by the proxy data. Considering only the points south of 40° N, MRE of downscaled simulation is 35 % smaller than that of the GCM. The pollen-based data set by Herzschuh et al. (2019), however, suggests much larger precipitation enhancement during the MH especially over East China and is therefore less consistent with both global and regional model results."

Regarding the usefulness of the cave data, previous modelling results (Pausata et al., 2011; Lewis et al., 2010) and analyses of modern-day instrumental records of $\delta^{18}O$ and precipitation over Asia (Dayem et al., 2010) suggest that the large-amplitude precessional scale variability in the $\delta^{18}O$ in speleothems is owing to precessionally forced changes in the strength of the Indian monsoon and monsoon rainfall in south of China that are in phase and oxygen-isotope records in speleothems are often interpreted as an index for the Asian monsoon intensity. Thus, we choose not to remove the discussion on the cave records in the revision.

Dayem, K. E., Molnar, P., Battisti, D. S. & Roe, G. H. Lessons learned from oxygen isotopes in modern precipitation applied to interpretation of speleothem records of paleoclimate from eastern Asia. Earth Planet. Sci. Lett. 295, 219 – 230 (2010).

Lewis, S. C., LeGrande, A. N., Kelley, M. & Schmidt, G. A. Water vapour source impacts on oxygen isotope variability in tropical precipitation. Clim. Past 6, 325 – 343 (2010).

Pausata, F., Battisti, D., Nisancioglu, K. et al. Chinese stalagmite δ 18O controlled by changes in the Indian monsoon during a simulated Heinrich event. Nature Geosci 4, 474 – 480 (2011). https://doi-org.myaccess.library.utoronto.ca/10.1038/ngeo1169

c) I think you could reduce the number of figures. For instance, you could show the SST and continental surface temperatures in one plot (Fig. 4 + 5). You could show the topography of both models together with the names of the geographical regions. Please think about which plot is really necessary and which do not help to underline what you want to say in your paper.

In accordance with the referees' wishes, we have now combined Figs. 4 and 5 to Fig. 4 in the revision and reduced the number of figures from 12 to 11.

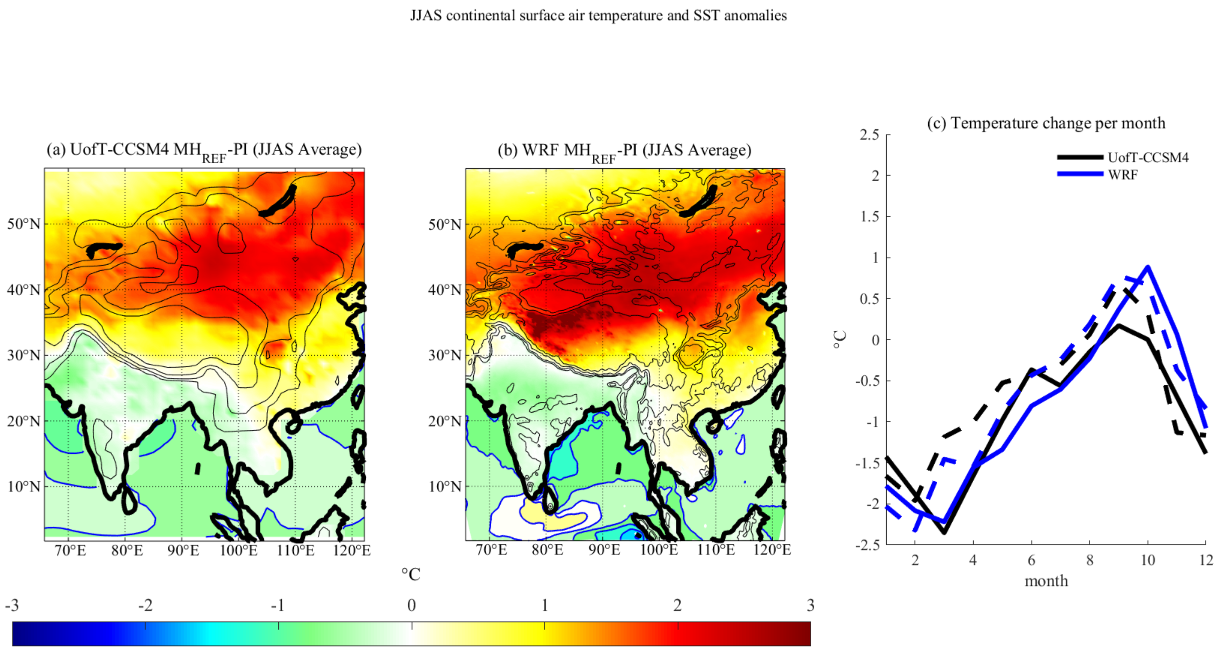

Figure 4: JJAS SST (contour interval 0.3° C) and continental surface air temperature anomalies (in ° C) for MH_{REF} in (a) UofT-CCSM4 and (b) WRF. (c) shows monthly continental air temperature anomalies over SA (solid) and SEA (dashed). Shifts in calendar are not accounted for, i.e. the model calendar is used for the calculation of all anomalies. The topography contours (black) of 500 m, 1000 m, 2000 m and 4000 m are also shown in (a, b).

d) The paper would benefit on a detailed discussion which processes are connecting the Green Sahara and South and Southeast Asia. Please already summarize in the Introduction, why the land-surface in North Africa may affect the Asian monsoon, how this teleconnection work and which dynamical circulation systems may be involved. To me it is e.g. not clear, why a greener land-surface outbalances the monsoon season shifts seen in the MHref simulation. It would e.g. be helpful to show and discuss the precipitation pattern and the atmospheric circulation in the global model for the entire region, North Africa + South/Southeast Asia.

We added a sentence in the introduction to discuss how the land-surface in North Africa may affect the Asian monsoon:

“Pausata et al. (2017) and Sun et al. (2019) pointed out that the presence of GS conditions in northern Africa shifts the Walker Circulation westward through changes in equatorial Atlantic SSTs and warmed the Indian Ocean, which enhances the SAM and SEAM.”

We also show and discusse the precipitation pattern and the atmospheric circulation in the global model for Northern Africa and SA and SEA now:

“In the global model, the GS enhances the northward expansion of the North African monsoon (Fig. 9c). Under the GS boundary conditions, the low-level westerlies over the northern Indo-Pacific Ocean becomes weaker than in MH_{REF} (Fig. 9f), which decreases the upwelling and hence increases the SSTs of that region(Fig. S1), favouring more evaporation. Anomalous easterlies induced by GS over the North Pacific carry more moisture into SA and SEA, intensifying the monsoon precipitation there.”

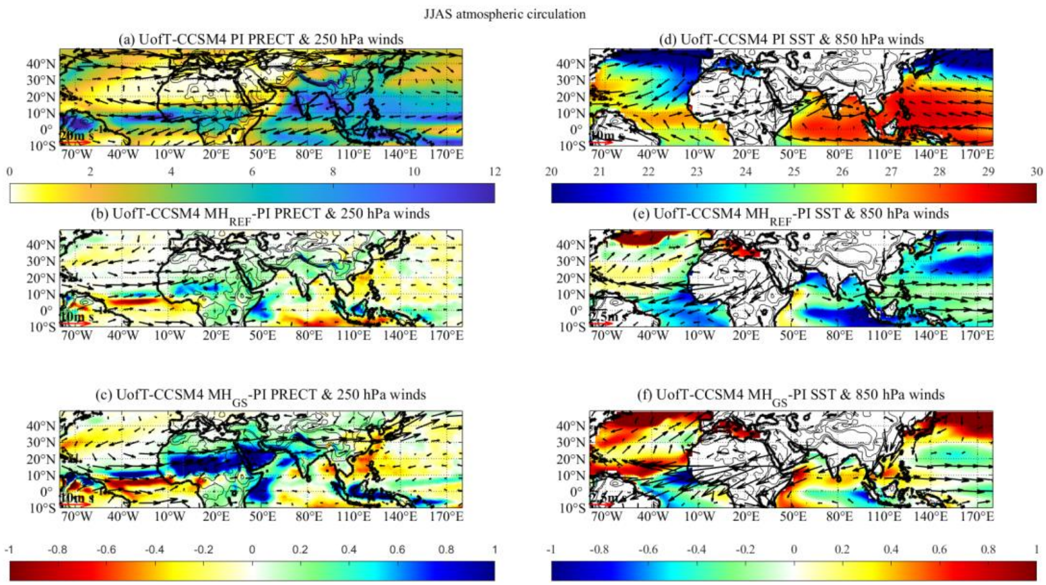

Figure 11: 250 hPa winds (vector, m s^{-1}) and precipitation (shaded,mm day^{-1}) of the WRF-CROCO ensemble mean for (a) the PI simulations and anomalies in (b) MH$_{REF}$ and (c) MH$_{GS}$. 850 hPa winds (vector, m s^{-1}) and SST (shaded, ° C) of the WRF-CROCO ensemble mean for (d) the PI simulations and anomalies in (e) MH$_{REF}$ and (f) MH$_{GS}$. The topography contours of 500 m, 1000 m, 2000 m and 4000 m are also shown.

e) The Introduction is very detailed, but you present a lot of information that is not really necessary to understand your paper (at least one has the feeling that it does not help to understand the paper). I recommend to re-structure the Introduction and pushing the individual parts more towards the main topic. For instance, in the first part (ll. 30 to 42) you stress the importance of the Tibetan Plateau on the Asian monsoon. Afterwards you talk about the population. I think, it would be more target-oriented to connect the importance of the Tibetan Plateau with the need to use a high spatial resolution in climate models to better represent the effect of the Tibetan Plateau on the monsoon. In global models, the Plateau is usually very flat, so why should global models capture the effect of the Plateau on the regional circulation? And this is one reason why it is so important to downscale the simulation.

Try to shorten the Introduction by beeing more precise and always keep your main topic in mind. You want to „ convince “ everybody that it is necessary to use regional models to analyse and understand the effect of a Green Sahara on the South and Southeast Asian monsoon. It is also important to highlight the advantages of the regional model for analysing the effect of the Green Sahara on the South and Southeast Asian monsoon.

We have shortened the Introduction by about 20%.

We have moved lines 35-42 in the original manuscript to the third paragraph, where we connected the importance of the Tibetan Plateau with the need of high resolution climate simulations in lines 74-86:

"In addition, the strengths of SAM and SEAM circulations and their onset, maintenance and withdrawal are to a significant extent influenced by the contiguous Tibetan Plateau (TP) that serves an elevated heat source for the atmosphere in summer that intensifies the thermal contrast between the continent and ocean in the region influenced by the Asian monsoons (Wu et al., 2007). However, due to the coarse horizontal resolutions of the global models, the TP, as well as the local mountains over SA and SEA, is poorly represented in GCMs. As a result, GCMs are incapable of realistically capturing local-scale atmospheric circulation and precipitation processes, which are strongly influenced by the orography. In order to fill the need for high quality climate information on regional scales while maintaining the computational tractability of the problem, this study employs the same dynamical downscaling pipeline described in Huo and Peltier (2021), to dynamically downscale MH global simulations."

f) Some sentences are really long. Please try to keep sentences short (e.g. ll 13.-17)

Thank you for this suggestion. We have shortened the long sentence in lines 13-17 to two separate sentences: "In order to more accurately capture important regional features of the monsoon system in these regions, we have completed a series of regional climate simulations using a coupled modeling system to dynamically downscale MH global simulations. This regional coupled modeling system consists of the University of Toronto version of the Community Climate System Model version 4 (UofT-CCSM4), the Weather Research and Forecasting (WRF) regional climate model and the 3D Coastal and Regional Ocean Community model (CROCO)."

g) It is often annoying when too many methods are not explained, but instead reference is made to other articles. Please think about explaining the main methods and giving essential informations on the models directly in this paper.

In the absence of specific details it is difficult to respond to this.

Section 2 and Fig. 2 give details of the primary method (dynamical downscaling) employed in this paper. Three component of the coupled model system (WRF, CROCO and UofT-CCSM4) are introduced in section 2. The major features of the MH simulations (MH_{REF} and MH_{GS}) are also provided in this section.

However, in the revision, we have added a sentence regarding the global model UofT-CCSM4 in lines 148-149:

"It is based on the standard CCSM4 model (Gent et al., 2011) with modifications of the ocean component for paleoclimate simulations."

Minor comments:

L 22: Decreased surface temperatures during mid-Holocene monsoon seasons may to a large part also result from the evaporative cooling of the surface due to enhanced precipitation.

Since our paper mainly focuses on the simulation of MH monsoon precipitation over SA and SEA and the reasons for temperature changes were barely investigated, this sentence has been removed from the abstract.

LL81-92: This method part could be shifted to the end of the Introduction. It disturbs the story here.

We have now moved this sentence to section 2.

L.93: The Green Sahara is not only a , climate difference ‘.

In accordance with the referees' wishes, we have now changed this sentence to "During the MH, northern Africa was considerably wetter than today and was covered to a great extent by a mixture of shrubland, grassland, trees, and wetlands, —namely the existence of a Green Sahara (GS; Pausata et al., 2020; Holmes and Hoelzmann, 2017; Chandan and Peltier, 2020)."

L.155: It is not clear if you name the regional simulations or global simulations or both with MHRef.

We added a sentence at the end of section 2 to make this clear: "In the following analysis, the set of MH experiments (both global and regional) including no specific land surface changes over north Africa is referred to as the reference MH simulation and denoted as MH_{REF}, while the other set of MH simulations which incorporates GS boundary conditions is referred to as MH_{GS}."

L. 159: I somehow miss a description of the land-surface conditions in Asia. Are they also prescribed according to mid-Holocene climate conditions? Does the global model includes dynamic vegetation? East Asia is also greener during mid-Holocene and this also affects the Asian monsoon circulation.

Vegetation cover over Asia are prescribed in the model and pre-industrial land cover was used. We have added "a vegetation prescribed to PI values" in line 155 to make it clear.

The suggested experiment using interactive vegetation or prescribed MH vegetation is interesting and would provide further improvements in reconstructions of the MH Asian monsoon systems, and we will look into that in our future studies.

L. 186-189: You could check if SST records are available for the region and if they indicate the same pattern

We have added comparisons with proxy records in lines 186-188 and 192-194:

"Regional Mg/Ca (Banakar et al., 2010; Govil and Naidu, 2010) and alkenone (Böll et al., 2015) indicate 0 to 1° C cooling in the northern and eastern Arabian Sea during the MH, which is consistent with both global and regional model results."

“Proxy records also suggest slight warming off the coast of south India (Saraswat et al., 2013; Gaye et al., 2018), which is more consistent with the results of CROCO.”

L.203: Please explain!

The negative temperature anomalies during spring and winter are a direct consequence of the reduced solar insolation. The cooling during early summer is likely related to the increased reflectance of shortwave flux at high levels from the greater cloud cover and increased surface evaporation due to enhanced precipitation. However, we chose not to extensively explain the reasons of the MH temperature anomalies here in the manuscript since our main focus is the SAM and SEAM precipitation.

L.211-213: , most of SA experiences wetter climate... ‘ In the plot most regions are yellow which means reduced precipitation during MH.””

We are referring to the WRF results (Fig. 5b) not the GCM results (Fig. 5a) here. We have now added “(Fig. 5b)” to the text to make it clearer.

L.221: , substantial differences between global and regional model...attest to the importance of high resolution modeling···. ‘ Both models more or less agree to the reconstructions, but it is not clearly visible which model performs better.

We now use Mean Relative Error (MRE) to quantify the goodness of fit between model results and reconstructions in the revised manuscript (see under general comment b).

LL.224-228:Why do increased temperatures downstream of the monsoon circulation result in more precipitation, please explain!

We have removed this sentence here and the cause of precipitation increase is explained under general comment d.

L.239: Please discuss the change in East Asian monsoon circulation and its effect on the precipitation in East China.

The suggested analysis is interesting and would provide additional information concerning the East Asian Monsoon, but we feel that it falls outside the scope of this study since our study focuses upon SAM and SEAM.

L.244-245: it's , Fig.6a and 6b ‘

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

L.245: Speleothems do not always recorder total precipitation.

This comment has already been addressed under general comment (b).

L.260-272: I would delete this part or move it to the Appendix.

This comment has already been addressed under general comment (a).

L.273: It's Figs. 6c and 7f.

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

L.308: Please explain the consequences of a reduced cooling over the northeastern Arabian Sea and southern BOB.

We have added the following sentence here to explain the consequences of SST increase here:

“The increase in SST favors more evaporation over the Arabian Sea and BOB, thus contributing to the SAM and SEAM precipitation increase.”

L. 356: Please also discuss the large-scale circulation, including Northern Africa.

This comment has already been addressed under general comment (d).

L. 436: The changes in precipitation as response to the Green Sahara forcing may also feed back to the South and Southeast Asian monsoon circulation. Please comment on this.

We now added the following discussion to the revised manuscript:

"The precipitation increase as response to the GS forcing is associated with a drop in surface temperature over SA and SEAM (Fig. 7), which reduces the sensible heat flux. On the other hand, the substantially increased SAM and SEAM precipitation leads to a release of latent heat, which warms the middle and upper troposphere and adds to the temperature difference between land and ocean (Fig. 7), thus driving stronger winds and moisture advection, which in term leads to enhanced precipitation."

Fig. 1: You do not really need this figure since you hardly explain it

We disagree concerning the role of Fig. 1 and have decided to keep it. This comment seems somewhat gratuitous. We have now added reference to Fig.1 whenever our analysis mentioned a new geographical location to help the reader to appreciate the geographical setting of our analyses.

Fig. 3: The black contours are difficult to see. The monsoon circulation is also determined by the cross-equatorial temperature gradient and the SSTs in the Southern Indian Ocean. Please explain, if this fact affects your results infered by the regional model that does not include these areas.

We have increased the weight of the black contours in the revision:

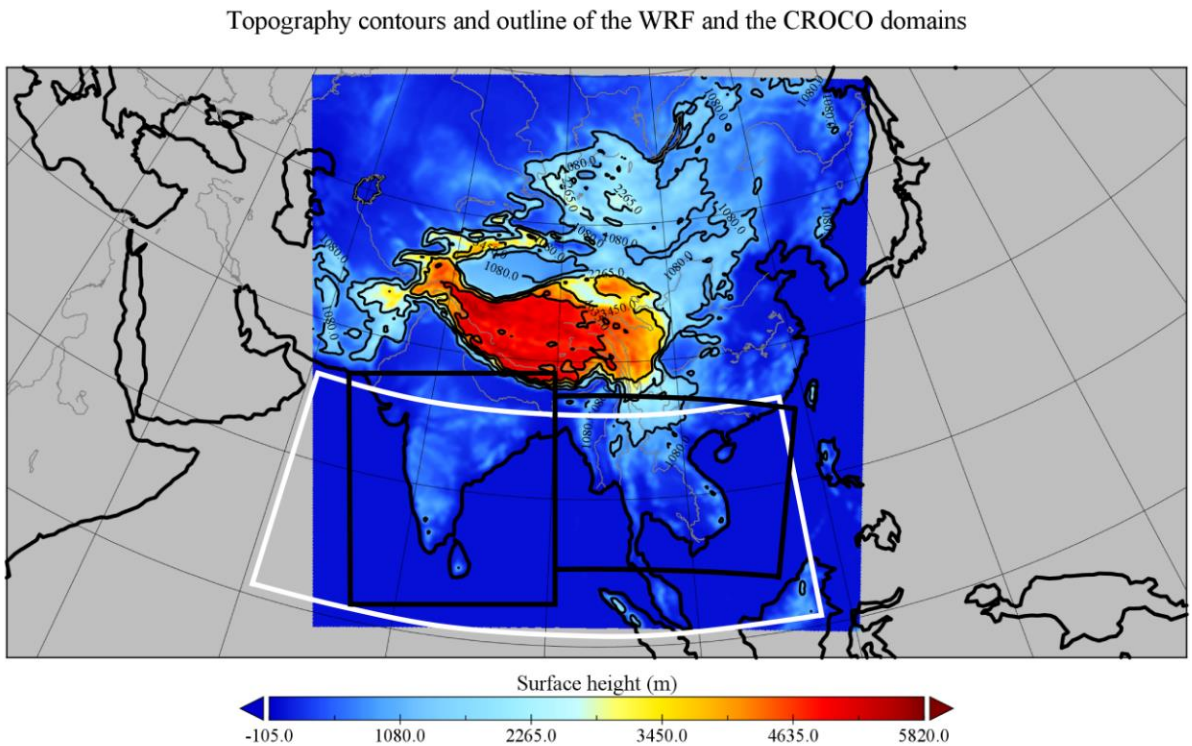

Figure 3: Shaded topography along with the outlines of the (shaded region) WRF and (white rectangle) CROCO domains. The two black rectangles denote the regions used to calculate spatial averages over SA and SEA. Major rivers and lakes are shown in grey contours and selected topographic heights in thin black contours.

We agree with the referee that SSTs in the Southern Indian Ocean can also affect the Asian monsoon circulation, but extending the domain of the regional model southward to include the entire Southern Indian Ocean can make the integration computationally more expensive. Besides, the Southern Indian Ocean has less complex coastlines than the northern part and thus the added value of higher resolution is expected to be relatively smaller.

Fig. 8c) It seems that in both regions the MHGS simulations reveal higher temperatures year round (or at least during most of the year) compared to the MHREF. Please explain why and how this affects the precipitation distribution.

We added a sentence to explain the reason for the temperature increase:

"Such warming is directly related to the increase in the SSTs since regions with the most pronounced warming (west coast of SA and south and west SEA) also have the largest increase in SST."

As to the link to precipitation distribution, we noticed that regional precipitation and temperature anomaly distribution patterns seem to be conversely related: regions with relatively smaller precipitation increase (he west coast of SA and south and west SEA) also have larger temperature increase and vice versa. Such correlation is likely due to the fact that increased precipitation is associated with increased cloud fraction and albedo, which lead to cooler temperature. Given that the magnitude of this temperature change over MH_{REF} is quite small (~ 0.2° C in JJAS), it is difficult to draw definitive conclusions on the effect of this warming on precipitation here.

Fig. 9c) Why is the precipitation increased in MHGS in the post-monsoon season. It would be helpful to show an anomaly plot MHGS-MHREF.

We have now added two dotted lines in this figure to show the MH_{GS}-MH_{REF} anomalies over SA and SEA.

JJAS precipitation anomalies

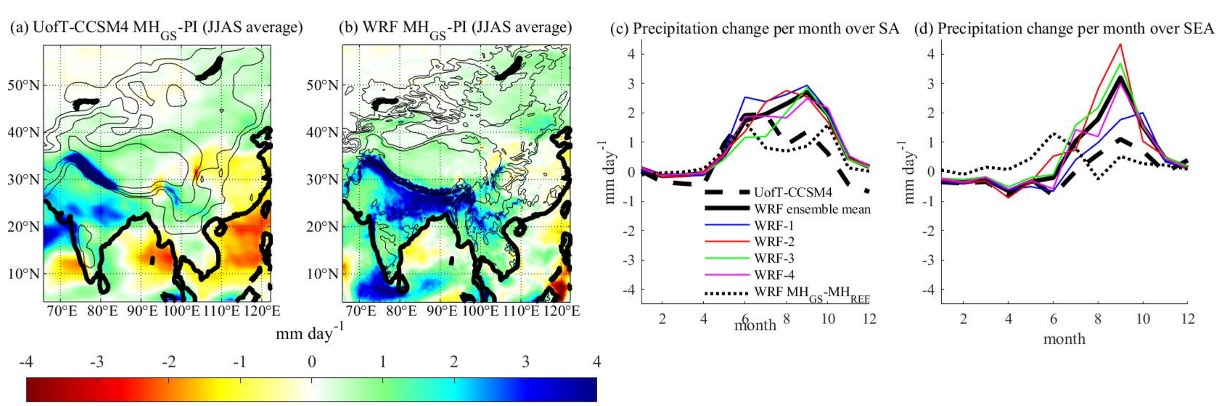

Figure 9: JJAS average precipitation anomalies (mm day-1) for MH_{GS} in (a) UofT-CCSM4 and (b) WRF ensemble mean. Monthly precipitation anomalies (mm day-1) for MH_{GS} in UofT-CCSM4 and four physics ensemble members over (c) SA and (d) SEA. Shifts in calendar are not accounted for, i.e. the model calendar is used for the calculation of all anomalies. The topography contours of 500 m, 1000 m, 2000 m and 4000 m are also shown in (a, b).

Fig. 11E: heading: it is SEA instead of SA

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

Fig.12: Please also show the global model results and the circulation changes over Northern Africa.

This comment has already been addressed under general comment (d).