Response to Referee Comment 2 (RC2) on: *"Threshold in orbital forcing for onset of African Humid Periods decreases with increasing greenhouse gases"* by Mateo Duque-Villegas et al.

Mateo Duque-Villegas (on behalf of all co-authors)
mateo.duque@mpimet.mpg.de

1st June 2022

We thank the anonymous referee very much for carefully reading our manuscript and for providing constructive remarks. Below we respond to every comment (blue font, our response in black font).

[0] This study presents modeling results of the last 190,000 years of African rainfall and vegetation history, classifying certain thresholds as "African humid periods" and commenting on the strength, duration, and rates of change of these differing AHPs. The authors find that orbital forcing is the primary driver for changes in rainfall and vegetation extent during past AHPs, but that the sensitivity threshold of AHPs to orbital forcing is modulated by GHG concentrations. Future modeling experiments are also conducted that show future AHPs are more likely to occur with higher concentrations of GHGs, as future orbital insolation thresholds are too low to induce AHPs without GHG increases.

[1] This study is well motivated and provides novel findings with respect to previously unknown factors contributing to the strength, duration, and rates of change of past AHP. This paper is exceptionally well-written and clearly presents its results and conclusions. In addition to a few minor comments, I believe one area for improvement can come from some added discussion on the uncertainties present within the very coarse model resolution of CLIMBER-2. It is important to show that the authors have considered all of the uncertainties involved in using this specific model and conclude that these uncertainties do not impact the conclusions of this paper – i.e., this model is the perfect fit for use with this specific research question. I recommend this paper be accepted with some very minor revisions. I list each comment for the revised manuscript below.

We agree it is important to consider possible sources of uncertainty in our study. CLIMBER-2 model uncertainty has been previously found to be comparable to that of CMIP5 models on the global scale (e.g., Ganopolski et al., 2016). And the data we use to prescribe the forcing in the model is based on widely recognised and discussed data sets. Moreover, our results are obtained with an ensemble totalling 21 experiments. Therefore, we believe uncertainties in model and forcing data should not compromise our findings. In a revised version of the manuscript we will refer in Section 2 (model description) to the uncertainties in the model and in the forcing data.

Comments

[2] It will strengthen the manuscript to elaborate upon the scale of the research question with regard to these simulations (for example: this study examines shifts in state of climate, such as desert vs. >50% vegetation cover, present within the single North Africa grid cell and does not require finer details with regard to the simulated climate) and how examination at this scale minimizes the large uncertainties present with using CLIMBER-2 to simulate paleoclimate. Bringing in discussion of multiple climate equilibria (green vs. desert) in northern Africa may help to strengthen this argument.

We welcome this suggestion and we will include in a revised version of Section 4 (discussion) a paragraph about the scale of our research question and about how the uncertainties in model and forcing data should not invalidate our discussion. At our scale of interest the possibility of multiple climate equilibria in North Africa has been discussed extensively.

[3] In Table 1, it would be more clear to list “Monsoon index via orbital parameters” (or something like this) so to not confuse readers over what is being prescribed in the model. The authors prescribe orbital parameters, which in turn dictate the monsoon index, rather than directly prescribing “monsoon index” as a specific boundary condition. Slight added nuance to reflect this would preclude confusion for future readers.

This will be fixed in a revised version as explained in our response to Dr. Liu (CC1).

[4] In Table 1, what does GHG radiative forcing = 0.0 W/m2 correspond to? The base value is listed for monsoon index (line 428), so it would be helpful to include the same for GHG radiative forcing. Or if this value is more difficult to assess, at least define more clearly that deltaRF is a change from the modern day... which is what time period? 1950 CE?

Indeed we were wrongly saying “radiative forcing” when it should be “radiative forcing change” with respect to a preindustrial base concentration for CO_2 of 280 ppm (C_0). This is explained in Appendix A, but not in Table 1 or the main text. This will be fixed in a revised version of the manuscript.

[5] On line 158, there are several studies with updated simulations using sophisticated models that could be cited here, in addition to Harrison et al. (2015). I would suggest adding at least a few of the following citations: Pausata et al. (2016), 10.1016/j.epsl.2015.11.049 Thompson et al. (2019), 10.1029/2018GL081225 Hopcroft et al. (2021), 10.1073/pnas.2108783118 Chandan & Peltier (2020), 10.1029/2020GL088728 Dallmeyer et al. (2020), 10.5194/cp-16-117-2020

Some of these were already mentioned in the introduction in Line 35. In the revision we will add “e.g.” to this citation and include some of the others the reviewer suggests.

[6] Both the interglacial and glacial factor separation analyses are important results of this paper, yet only one is presented in the main text. I would suggest the authors bring the glacial factor separation analysis into the main text as an additional figure. Or the authors could at least describe why they believe the interglacial case is more important than the glacial case and use this explanation to justify why the interglacial case is included in the main text while the glacial case is not.

We agree the glacial factor separation is a relevant complement to our method, and that is why it is included in our study. However, we think it does not add anything significantly new to our discussion and that is why it is in Appendix B. We will justify this decision in the main text around Line 271 in a revised version of the manuscript.

References

Ganopolski, A., Winkelmann, R., & Schellnhuber, H. J. (2016). Critical insolation–CO_2 relation for diagnosing past and future glacial inception. *Nature*, *529*(7585), 200–203.