Response to Referee Comment 1 (RC1) 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

10th May 2022

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

[0] This is a good paper that presents some interesting new simulations. I appreciate the work that's gone into these runs and their analysis and can readily see this manuscript being published in Climate of the Past. There are some aspects of it that need clarification before publication, and I think a little bit of further analysis would greatly enhance the reach of this manuscript. I especially appreciate the data and code placed in the online repository.

[1] The model description (Sect 2.1) mentions nothing about the land surface model. Given the importance of the vegetation fraction in this manuscript, you need to provide some information about how vegetation is simulated by the model (tree, grass etc) – and what, if any, feedbacks it has on the atmosphere.

We agree and in a revised version of the manuscript we will expand our description of the vegetation model component VECODE (see our reply to CC1).

[2] I feel the analysis about the rates of change (Sect 3.3) is out of place in this manuscript. It seems to invoke a fundamentally different conception of an AHP to the other work. The rest of the work talks about thresholds (implying transitions between bistable states). Yet this section discusses the speed of the changes as being related to the speed of forcing changes irrespective of their location w.r.t. the thresholds. Personally, I feel this aspect of the research should be removed to focus more on the subject in the title.

We understand the title does not fully reflect the breadth of our study. This also is a prevalent comment in all reviews. Therefore, we will change the title after consultation with the Editor. Regarding Section 3.3, we partially agree. We still think that the section is interesting, since we find a threshold in the changes of vegetation, but no threshold in the speed of this change. But instead of discussing these results in a separate section, in a revised version of our manuscript we will shorten the discussion and put it as extra paragraphs into Section 3.2.

[3] You discuss the threshold as a function of the maximum orbital forcing. This may be appropriate for precipitation, but is this really the best way to think of vegetation threshold? Intuitively, I see a threshold as being lower than the maximum value with the intensity of the vegetation response driven by the time spent over that threshold.

Indeed, we focus on the threshold in the orbital forcing, following the classical paper by Rossignol-Strick (1983), who has found a threshold in monsoon forcing above which sapropels (proxy for AHPs) in marine cores from Eastern Mediterranean occur. We were excited to see that in our model, we capture the same threshold behavior as found in the proxy data. The threshold is a value (between 15–20 W m-2) lower than the maximum value of the orbital forcing (about 30 W m-2 in the last 200 ka), and the intensity of the vegetation response is driven by the time spent over the threshold and by how much the forcing exceeds the threshold.

[4] It is not clear precisely what is plotted in the trajectories of simulated data. Are these the data for a single grid box? If so, which one? Is the vegetation fraction presented a proportion of this grid box, with the rest of it being bare soil?

Indeed, in the coarse-scale model CLIMBER-2, the Sahara is represented as one grid box (see previous CLIMBER-2 publications). We will point out this more clearly in a revised version of our manuscript.

[5] Why have you selected only the past 190 kyr (Sect 2.2)? I presumed this was motivated by the 2 references cited on L36 – although you should make this explicit. It seems though that Ehrmann & Schmiedl review back to 200ka and Blanchet et al seems to go back to 160ka from their Fig 3. I don't expect you to redo any simulations – your start date is fine for the science. But it needs a solid motivation written in the paper.

Yes, we chose this time window based on the data sets in Ehrmann et al. (2017), Ehrmann et al. (2021). We will motivate our choice more clearly when revising our manuscript.

[6] There is no discussion in the paper of internal variability in the simulations. My own work (Brierley et al, 2018, https://www.nature.com/articles/s41467-018-06321-y) building of Zhengyu Liu's model relies quite heavily on the fact that the AHP transitions involved some stochasticity. I suspect this will be case for CLIMBER-2 as well, and that this would explain the difference in precipitation at MIS5e between EI7 and E0 in Fig6b. Again, I don't think any additional analysis is needed – just some discussion of its implication for your analyses.

CLIMBER-2 is a statistical–dynamical model that has by design no short-term weather variability, but only climate variability at time scale of decades and longer. The effect of weather on the climatic circulation (mainly the meridional heat and momentum transport) is parameterized (Petoukhov et al., 2000). Therefore, we use century-scale averages of the model output. The differences between EI7 and E0 at MIS5e are actually due to the combination of prescribed forcing factors, which all happen closely around 125 ka, but not exactly at the same time as in E0. To see this, one has to closely look at Fig. 1a and b (GHGs and ice sheets) and notice the small differences between the black solid and orange dashed lines around MIS5e. In a revised version of our manuscript we will discuss the internal variability of the statistical–dynamical model CLIMBER-2.

[7] You could go further with your simulations and combine the results from the future simulations with that of EI2, EI4 and EI6 to perform an analysis similar to that in Fig. 3 to quantify the impact of GHG forcing on the orbital threshold. As currently written this feels like a missed opportunity to really demonstrate the statement in the title.

We welcome this suggestion and we will study the output of such analysis.

Other comments

[8] 'Synergical' feels very awkward – try 'synergistic'

This will be fixed in a revised version.

[9] I agree that with Dr Liu that a slight rebranding of the Monsoon Index would be helpful

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

[10] You should explain how the lagged peaks in Fig 2a reflect the intensity during the sapropel. You make no comment about the split event at 5c in SL77. Why are these better measures of intensity than something like the co-eval Ba/Al ratios measured by Zeigler et al (2010)?

The interruption in sapropel S4 is a known feature of sapropels from the Eastern Mediterranean, and is probably related to postdepositional "burn down" events via redox reactions in the sediments (Emeis et al., 2003; Grant et al., 2016). We do not argue one proxy data to be better, however, the data from Ehrmann et al. (2021) is associated with weathering (rainfall) and accumulation of minerals in water bodies across (most likely) North Africa, while Ba/Al is a measure of seabed primary productivity susceptible to multiple oceanic processes also discussed by Ziegler et al. (2010). Though our study is a purely modelling effort, we do agree a bit more context about the data could be helpful. In a revised version of our manuscript we will add extra remarks about proxy data in Section 3.1.

[11] "reckon" on L169 sounds informal. Please replace.

This will be fixed during revision.

[12] You are too precise stating that the change point at 20Wm-2. Surely all you can tell is that its between 15-20 Wm-2.

The precision comes from the monsoon index value at MIS 1 (Holocene) in Table 2, which is 20.0 W m-2 (i.e., the method does not compute new numbers). The changepoint method is only selecting the value in Table 2 (column 5) where there is a jump in data. In a revised version we will add a parenthetical note about this "20 W m-2" being the Holocene value.

[13] How do you justify LOWESS smoothing all the forcing in Fig. 4, but not the simulated vegetation fraction? [I recommended cutting this section above]

The smoothing is only applied to the GHGs and ice sheets series to ease the visual inspection of trends. We will remove smoothing of forcings in a revised version of the manuscript.

[14] I strongly suspect that the analysis in Fig 5 would have also show the rates of initiation and termination of the AHP events is strongly correlated to the peak monsoon index. How can be sure that your style of analysis is more appropriate. [I recommended cutting this section above]

We thank the reviewer for this remark. Please see our response here to comment [2].

[15] 6. I like this figure, but can you please check that it works for color-blind individuals.

This was checked with https://www.color-blindness.com/coblis-color-blindness-simulator/ and the figure works for some but not all types of colour blindness. We will test for better colour sets and update the colours in Fig. 6 during the revision.

[16] This sentence seems odd. If you really feel that it is only the weak orbit that matters, then please rephrase to avoid the conflation with 'glacial times' – as that phrasing intuitively suggest that GHG and ice-sheets play a role. You might want to try: "This analysis demonstrates that it is the relatively low maximums in orbital forcing that result in the absence of AHP conditions at 6b, 4 and 3a – rather than the low GHG forcing or large ice sheets."

Assuming this statement refers to Line 274, we welcome this suggestion. We will re-phrase the sentence in Line 274 in a revised version of our manuscript.

[17] It would be instructive to take the work about future AHP conditions a little further. Can you find a way to quantify the impact of GHG forcing on the orbital threshold. I feel that there should be enough data here.

We appreciate this suggestion and we will assess possible ways to achieve this.

[18] I also wonder if you could provide some additional context for the future simulations for those of us not fully versed with the future carbon cycle pulses. As well as the GHG forcing, it might be helpful to plot global mean temperatures and atmospheric CO2 levels. In effect, I am wondering how the future AHP at M1 relates to proposed warming levels and safe operating spaces.

In a revised version we will expand details about future climate change scenarios in Section 2.3, and include global average temperature time series in Fig. 7.

References

Ehrmann, W., Schmiedl, G., Beuscher, S., & Krüger, S. (2017). Intensity of African humid periods estimated from Saharan dust fluxes. *PloS One*, *12*(1).

Ehrmann, W., & Schmiedl, G. (2021). Nature and dynamics of North African humid and dry periods during the last 200,000 years documented in the clay fraction of eastern mediterranean deep-sea sediments. *Quaternary Science Reviews*, *260*, 106925.

Emeis, K.-C., Schulz, H., Struck, U., Rossignol-Strick, M., Erlenkeuser, H., Howell, M. W., Kroon, D., Mackensen, A., Ishizuka, S., Oba, T., Sakamoto, T., & Koizumi, I. (2003). Eastern mediterranean surface water temperatures and δ18o composition during deposition of sapropels in the late quaternary. *Paleoceanography*, *18*(1).

Grant, K. M., Grimm, R., Mikolajewicz, U., Marino, G., Ziegler, M., & Rohling, E. J. (2016). The timing of mediterranean sapropel deposition relative to insolation, sea-level and african monsoon changes. *Quaternary Science Reviews*, *140*, 125–141.

Petoukhov, V., Ganopolski, A., Brovkin, V., Claussen, M., Eliseev, A., Kubatzki, C., & Rahmstorf, S. (2000). CLIMBER-2: A climate system model of intermediate complexity. Part I: Model description and performance for present climate. *Climate Dynamics*, *16*(1), 1–17.

Rossignol-Strick, M. (1983). African monsoons, an immediate climate response to orbital insolation. *Nature*, *304*(5921), 46–49.

Ziegler, M., Tuenter, E., & Lourens, L. J. (2010). The precession phase of the boreal summer monsoon as viewed from the eastern Mediterranean (ODP site 968). *Quaternary Science Reviews*, *29*(11-12), 1481–1490.