Response to Referee Comment 3 (RC3) 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

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We thank the anonymous reviewer 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 paper is a valuable contribution to the theory underlying African Humid Periods and their variable forcings. The authors present a carefully considered set of intermediate complexity model simulations that allow for factor separation analysis. They have clearly produced a lot of data/results, and I appreciate the efforts they have made to condense the work to the most important points. I think the paper is close to being ready for publication. Here, I touch on some previous points by other reviewers that I agree with, and I add a couple of additional, minor points.

[1] The most substantial point that I wish to emphasize comes from Dr. Liu about how the main text is somewhat disconnected from the title. This is a substantial point only in that I think the paper could benefit from re-structuring the arguments, but I don't see this as necessary for publication. Specifically, I suggest re-framing the paper more explicitly as a comparative analysis of past and future AHPs. This would involve discussing the future simulations more prominently and, as other reviewers mentioned, diving into more detailed hypotheses as to how/why GHGs lower the orbital threshold. The question of whether emissions can compensate for low future eccentricity to cause future AHPs is thought-provoking, and the results—casting emissions scenarios as the primary determinant of the frequency and amplitude of future AHPs—could motivate much further research into GHG and orbital "synergies".

Please see our responses to Dr. Liu (CC1) and Dr. Brierley (RC1) on this topic. In a revised version we will re-structure parts of the text, expand on the model description and on the possible mechanisms for GHGs lowering the orbital threshold. We will also, in consultation with the Editor, consider updating the title if possible.

[2] A couple of smaller points that I agree with from other reviewers. I like Dr. Liu's suggestion to call "Monsoon Index" the "Monsoon Forcing Index". I also agree with Dr. Brierly that more background on the land surface model is needed, especially with respect to the threshold behavior, relevant feedbacks (including fire), and whether there is hysteresis. I also agree that the rate of change analysis could be removed. It's not currently grounded by anything in the discussion, and I agree that it is difficult to square with the threshold behavior.

Please see our response to Dr. Brierley (RC1) on these issues. In a revised version of the manuscript the monsoon index will be "re-branded", the model description extended and the rates-of-change part will be shortened and moved.

[3] My two suggestions are (1) to cite/discuss some more proxy work; and (2) be more explicit about any assumptions associated with factor separation analysis:

[3.1] The paper focuses on two records from the Mediterranean for comparison. However, there are other records that span the same time interval, and it is worth mentioning how they compare (amplitude, duration, etc) to the new model results. Two datasets that are particularly relevant are

Miller et al. 2016 (JQS) and Skonieczny et al. 2019 (Sci. Adv). The Miller paper is useful for more directly comparing the vegetation results to a vegetation reconstruction; and the Skonieczny paper presents another dust flux record off West Africa.

We thank the reviewer for the literature suggestions and we will briefly discuss these additional proxy records in an updated version of the manuscript.

[3.2] One concern I have has to do with any assumptions inherent to the FSA (I don't have expertise in FSA, so please bear with me). It seems like one implicit assumption is that any non-linearities (when multiple forcings yield a different result than the sum of individual forcings) can only arise due to "synergies" or interactions between the forcings. That is, the response to any forcing is assumed linear so the responses can be summed together (and deviations from the sum are synergies). However, a non-linear response to a forcing (such as threshold behavior in vegetation %) could lead to "apparent synergies" between forcings that are actually projections of a non-linear response (rather than a non-linear interaction between forcings). For example, if GHGs and orbital forcing alone both cause a stepwise increase from desert-to-grassland, then FSA would expect the GHG + orbital simulation to produce this stepwise transition twice (without synergies) and any deviation from this would be attributable to synergies. The authors briefly touch on this specific case in line 265, but they relate the issue to the fact that vegetation % is bounded between zero and 100. However, maybe it's the case that the bounding only makes the issue clearly diagnosable. I'm curious if the issue is broader, applying to any non-linear response where the response is not necessarily the sum of its parts (even without "synergies"). I expect that a basic discussion of the assumptions of FSA would suffice here. If my comment about non-linear responses is entirely off-base, then maybe adding a sentence about why this intuition is wrong could be helpful for readers like me.

The factor separation analysis does not diagnose whether the response–factor relationship is linear or not. Instead, it diagnoses whether contributions to the response from multiple factors can be added linearly or not. It is possible that individual forcing factors cause non-linear responses, but they should always do it in a similar way, and therefore their joint effects could be added linearly to predict a full response. When they cannot be added linearly, then we need to include the synergies. In other words, synergies are "non-linear terms" in the sense that they show that individual effects from multiple factors cannot simply be added to predict the full response. The challenging part is assigning a physical meaning to the synergies that the separation method obtains. In a revised version we will expand on the description of the method to explain the synergies in this way.