

Clim. Past Discuss., referee comment RC1  
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## **Review by Chris Brierley (UCL) of cp-2022-26**

Chris Brierley (Referee)

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Referee comment on "Effects of orbital forcing, greenhouse gases and ice sheets on Saharan greening in past and future multi-millennia" by Mateo Duque-Villegas et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2022-26-RC1>, 2022

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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.

- 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.
- 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.
- 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.
- 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?
- 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.
- 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 Fig 6b. Again, I don't think any additional analysis is needed – just some discussion of its implication for your analyses.

- 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.

Other comments:

- 'Synergical' feels very awkward – try 'synergistic'
- I agree that with Dr Liu that a slight rebranding of the Monsoon Index would be helpful
- 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)?
- "reckon" on L169 sounds informal. Please replace.
- 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.
- How do you justify LOWESS smoothing all the forcing in Fig. 4, but not the simulated vegetation fraction? [I recommended cutting this section above]
- 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]
- 6. I like this figure, but can you please check that it works for color-blind individuals.
- 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."
- 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.
- 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.