Response to Referee Comment 2 (RC2) on: *"Pattern scaling of simulated vegetation change in North Africa during glacial cycles"* by Duque-Villegas et al.

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16th January 2025

We thank Dr. Tiwari very much for carefully reading our manuscript and the constructive feedback. Below we respond to every comment (blue font; our response in black font).

General comments

[0] In my view, this study is very interesting and relevant, and quite novel. Barring some grammatical problems, the manuscript is well-written in terms of coherence and flow and the figures are well-made. In my view, it is certainly a study which should be published and would be of interest to the paleoclimate community (especially AHP workers). However, it requires some revisions before publication. There are two major issues that the authors should address:

[1] 1. The under-estimation of the Holocene AHP is critical. This is not just because the model may be under-representing AHPs in general, because that would hopefully apply to all simulated AHPs and hence not greatly affect the linear scaling. The issue is that the proposed mechanism (unexpected MOC variability) for this under-estimation appears to apply only to the Holocene AHP. It appears to me that similar MOC variability also occurred before the AHP in MIS5e. Does it lead to a similar under-representation of that AHP? In any case, either one or both of the interglacial AHPs are under-estimated but the other two are not. Can a linear scaling model really be applied in this case?

To clarify: my issue is not with the scaling of the orbital forcing, which is evident in Fig. 1a. Intuitively, this would likely lead to a scaling of the AHP representation. My issue is with the ability of your simulations to show this scaling given the imbalanced representation of the different AHPs.

We agree with Dr. Tiwari that the under-estimation of the Holocene AHP is important, however, we do not expect that it alone can upset greatly the relationship we find in concert with the other AHPs. Since orbital changes appear to be the dominant forcing of AHPs in the past, the limitation that other processes/forcing factors operate in some AHPs (e.g. AMOC variations), but not all, is tolerable for applying a regression model. It certainly has an impact on how well the individual AHPs are represented by the regression model. It is important that the influence of the orbital boundary conditions is clearly visible in all AHPs and is not masked by another process. Nonetheless, we are confident that only the Holocene AHP was affected by some odd MOC variability that appeared after the strong Meltwater Pulse 1a (MWP1a) near 14.4 ka. The MOC variability near the AHP of MIS 5e is not as great (away from 0 Sv in Fig. 1e) and is in line with the discharges of the ice-sheet reconstruction. Compared to our previous other (shorter) simulations, only the Holocene AHP appears disturbed, while the AHPs of MIS 5 appear consistently as in previous simulations. Therefore, even when the model is under-representing AHPs in general (dry bias, line 390 ff.) we believe the linear relationship we establish should remain valid. In a revised version of the manuscript we will mention in the main text parts of Appendix B that refer to the previous experiments that make it clear only the Holocene AHP was unexpectedly weakened by AMOC variations.

[2] 2. To me, a study of the future where ice-sheets are kept unchanged is meaningless, especially when ice volume is one of three predictors. I'm afraid the point of Section 3.6 is quite lost on me. From my viewpoint, it would be a perfectly good and "complete" study without attempting to simulate the future climate (imperfectly) as well.

We agree with Dr. Tiwari that the assumption of unchanged ice sheets over the next 10 kyr is a bold one. Indeed, ice volume as a predictor in our method is mainly important for past glaciation cycles, while for the two future scenarios it remains neutral. However, we do not think that it is meaningless. Specifically, we refer to the study by Ganopolski et al. (2016), where they suggest that only in the case of decreasing CO_2 concentration we can expect some increase in the ice sheet extent. In other cases, their simulations suggest a nearly constant or smaller ice extent. In a revised version of our manuscript, we will emphasize that our assumption of a constant ice sheet extent over the next 10 kyr is only a very first approximation. We suppose that this assumption will only marginally affect our conclusion that the possible future patterns of Saharan greening do not look like the patterns of the last 130 kyr.

[3] Another relatively minor issue is that I would have liked to see more evidence in favor of their scaling approach for ancient AHPs through more extensive comparison with other proxy records or modelling studies. This is discussed briefly, but to me, elaborating this would be most beneficial in reinforcing the applicability of this technique to ancient AHPs.

We agree that it is key to evaluate our simulations and pattern scaling method with available sources. We compare our findings with some marine proxies and a few other modelling results, but we keep it brief because we do not think that a "more extensive comparison" is possible yet. Only for the Holocene AHP more extensive comparisons have been made and we refer to some of them which are linked to our climate model (e.g. Dallmeyer et al., 2020), but for the AHPs of MIS 5 the proxy records are too few and fragmented, and therefore evaluation or inter-comparison of climate models is still work in progress (e.g. Otto-Bliesner et al., 2021).

Specific comments

[4] L18: Perhaps deMenocal et al. (2000) is not the best reference here, because you mention water bodies and expansion of vegetation cover only.

In a revised version of the manuscript we will mention that such covers modulated dust output to the Atlantic Ocean.

[5] L22: I don't think our understanding of the extent and timing of the AHP changes depends on climate simulations (and their discrepancies with records), but instead, it is more to do with the proxy data limitations is well taken. You could highlight the point about the discrepancies in a separate sentence.

We agree with the reviewer that the sentence lacks clarity.

[6] L25: The references Blanchet et al., 2021 and Ehrmann et al., 2017 are not really wrong, but perhaps misplaced here.

We agree they can be omitted here.

[7] L32: You should either provide a reference for the second part of the sentence which focuses on climate models, or break this sentence into two and present the second part as a general point.

We will refer to Tierney et al. (2020) where they mention computational expenses in paleoclimatology.

[8] L54: Both the references are related to the Holocene AHP only, hence misplaced in a paragraph that focuses on ancient AHPs. You could cite the same papers but to say that this is a problem with the Holocene AHP simulations which may extend to the ancient AHPs too (depends on the point you want to make, of course).

We agree it is important to make this distinction in a revised version.

[9] L109: Keeping ice sheets unchanged at present day levels is a critical limitation, in my opinion.

As discussed in a previous point, we will refer to the study by Ganopolski et al. (2016).

[10] L138: This is confusing. What is the simulation offset from: the proxy record or the orbital monsoon index? It looks like both to me (at different times). In Fig. 1f, the model precipitation shows more variability and the proxy record is smoother. I see from the figure caption that the proxy record is smoothened, but it would be helpful to mention that here too.

We thank the reviewer and we will clarify this in a revised manuscript.

[11] L157: Is this lag not related to the time taken for vegetation to develop and fully expand over the region?

This is also likely a cause for a lag, but only of a few centuries. In a revised version of the manuscript we will explain it is *most of the* lag what is related to the orbital configuration.

[12] L164: Wouldn't the effect of the MOC variability apply to MIS 5e too?

Yes, but the difference is that the MOC variability during MIS 5e is expected because it is part of the meltwater discharges related to the ice-sheets reconstruction, while in the case of the Holocene it is unexpected because the meltwater discharges are not so large that they should cause such AMOC variations (see AMOC in Kleinen et al. (2023)).

[13] L303: I think it is key to quantify how many of the simulated AHPs do agree with sediment core signals, since that would be the most robust evidence in favour of applying this linear scaling to ancient AHPs.

We will add this number in a revised version of the manuscript.

[14] L327: I think this is a crucial limitation of applying your linear scaling model to the future. Simulating the future with invariant ice-sheets is both unrealistic and meaningless. It is additionally problematic if your linear scaling model has an ice volume parameter. For me, the Section 3.6 does not hold any meaningful results and I do not see the point of including it in the manuscript. (Perhaps the significance has eluded me, and the authors could explain it better.)

We addressed this point in a previous comment. We think it is meaningful to contrast Sahara greening patterns according to different dominant drivers (orbit vs. GHGs), at least as a very first approximation.

[15] L335: Independently of the previous comment, I would argue that it is not correct to say that the simulated pattern "does not resemble any of its AHP predecessors". The absence of the northward vegetation expansion along the west coast is strange, but hardly the only criterion to be employed. (I see a decent zonally-extended expansion and also a meridional pattern in eastern Africa, for example.)

We thank the reviewer and we agree this should be fixed in a revised version of the manuscript.

[16] L336: I don't think either of the papers cited is appropriate for a comparison (or for showing agreement) here. Neither discusses the time after 9 kyr. Pausata et al. (2020) discuss the future in terms of geoengineering projects only. If you would like to keep this statement, please clarify how those studies have results similar to the differences you're talking about.

We thank the reviewer for pointing this out. Indeed Pausata et al. (2020) is misplaced here, and it should have been Gaetani et al. (2017). The relevance of Gaetani et al. (2017) and D'Agostino et al. (2019) is because both studies look at the response of the West African Monsoon under high GHGs scenarios. Although the orbital configuration at 9 kyr AP is different in our experiments (and explains part of the Saharan greening), we argue that the effects of the high GHGs scenarios is comparable across these studies and ours.

[17] L337: I see that this is the same point I was trying to convey earlier. However, I don't think it really qualifies to be a "result" of this study, but is instead a foreseeable limitation.

In a revised version we will rephrase this to clarify that this is an expected limitation of our scaling approach because the fitting data did not include such ranges of GHGs because they did not happen during the late Quaternary.

[18] L369-370: This line is unclear. How are you comparing vegetation changes with precipitation changes from a model-data comparison? This is the first mention of any comparison with Scussolini et al. (2019). The results from the comparison should be mentioned somewhere in the Results section before.

In a revised version we will clarify the qualitative nature of this comparison and its limitations.

Technical corrections

We appreciate all the suggestions and will address them in a revised version of the manuscript.

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