Response to Referee Comment 1 (RC1) on: *“Pattern scaling of simulated vegetation change in North Africa during glacial cycles”* by Duque-Villegas et al.

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We thank the referee very much for carefully reading our manuscript and for the constructive feedback. Below we respond to all comments needing clarification (in blue font; our response in black font).

[0] The paper from Mateo Duque-Villegas et al. uses MPI-ESM v1.2 for the past 134 ka to simulate northern African hydroclimate and vegetation to reconstruct past AHPs. It aims to construct a pattern scale model to estimate AHPs of the past 800 ka. The model uses forcing variables (insolation, ice volume, and GHG) and is tested against proxy variables such as d18O (vs. 2K T), dP (vs. L*), and vegetation (vs. isotopic depletion and dust). Then, past AHPs are defined as peaks in the pan-Saharan vegetation coverage (%) and analyzed spatially. Here, EOFs capture dominant patterns of vegetation change, which are used to fit a linear model between the forcing variables and the PC1. They state orbitals influence around 66% of the linear model outcome. Next, the pattern scaling is applied and compared against MPI-ESM output, showing a trustworthy approach and extending the temporal extent back to 800 ka. The comparison with the Saporpel record from the Mediterranean does show a good agreement when using some arbitrary thresholding. Last, the pattern scaling approach and their MPI-ESP are used for future SSP scenarios with moderate and intensive GHG emissions for the coming ten kyr. Differences are observed, mainly when the pattern scaling linear model is used for the intense GHG emissions, as this quasi-empirical model has no data example for that.

[1] The paper presents an insightful, well-written approach and uses well-curated figures to show its results. The paper structure is sometimes unusual with literature work and comparisons and discussions within the result section and, therefore, a short discussion section due to the linear progressing type of analysis. I like it, but others can see this differently. The length and the grade of detail are sufficient and allow fluent reading.

[2] The method and the presented results seem logical and reasonable; no significant flaws were detected by my side, even though I am not an expert in GCMs, especially not for this model. Therefore, from my modest perspective, I see the paper as acceptable, but there are some suggestions and comments from my side:

Major comments

[3] The temporal framework in which the model results are presented is based on glacial cycles and the marine isotope stages, whereas the research target is mainly tropical. There are arguments to see this as problematic, as it can be a north-western perspective that acts as a framework to understand tropical climate, leading to false reasoning about forcing factors. They may also lead to false conclusions when they are used to discussing tropical climates if NH climate implications are applied. In contrast, cycles of the Monsoon Index could simply be counted and labeled, or absolute ages could be used.

Indeed, the focus of our study are the tropics, but our simulations are global, and we discuss the climate and vegetation changes in northern Africa in a global context. We find the MIS terminology useful, because it characterizes the (background) state of the global climate system while the changes in the

Sahara occurred. Many of the proxy data publications of late Quaternary climate change in northern Africa also include such MIS referencing (e.g. Rossignol-Strick, 1983; Ehrmann et al., 2017; Tierney et al., 2017; Skonieczny et al., 2019; Grant et al., 2022). Alternative labelling such as AHP1, AHP2, etc., or AHP of Monsoon Index (MI) 1, AHP of MI2, etc. would not provide such a global context. We would be surprised, if the MIS terminology would lead to false reasoning about the forcing factors. We clearly specify the forcing factors in terms of the (tropical) Monsoon Index for scaling (although the numerical simulations are driven by the global changes in the meridional insolation), the radiative forcing changes linked to GHG changes, and the extent of Northern Hemisphere ice sheets (although the prescribed Antarctic ice sheet also changes slightly in the numerical simulations). Nevertheless, we agree that the absolute ages should be clearly stated. In a revised version of the manuscript, we will explain our use of MIS labels to identify the four AHPs, as well as make sure absolute ages accompany their first appearance in the text.

[4] The draft presents its results mainly as a cost-efficient estimation of past AHP patterns based on the simple linear fit. However, the fit itself is of interest, as it shows that orbitals are the main drivers of AHPs, not GHGs or ice sheets. Why is this not presented as an exciting result to be discussed?

We agree with the reviewer that our result regarding the dominance of orbital drivers is exciting. We published this result already in a previous paper (Duque-Villegas et al., 2022), but we are happy to re-iterate that also the more complex model exhibits this dominance in the forcing factors (previously we used an intermediate complexity model). In a revised version of the manuscript, we will explain in the discussion section how both studies are connected by this finding.

[5] Vegetation and vegetation feedback are non-linear and have multimodal distributions. However, EOF assumes unimodality and works better with a Gaussian distribution. How is this considered?

We discuss the linear limitation of our scaling approach in the discussion section (lines 392 ff.). Indeed, such non-linear climate-vegetation interactions appear in the simulations with the numerical climate model, as we show for the Sahara in Fig. 2. However, the feedback is not as strong as seen in our previous application with an intermediate complexity model, and we suspect part of the success of our EOF method could be attributed to this rather weak feedback. Moreover, the problem of spatial heterogeneity in the local vegetation responses clearly limits the applicability of our uni-modal EOF approach, yet we argue the method should be capturing a common general trend of change imposed by the forcing factors we consider, all other things being equal. At least at our working horizontal resolution (grid spacing of about 400 km) we find such an uni-modal response that explains much of the regional variability along glacial cycles. We will emphasize these issues in a revised version of the manuscript.

Minor comments

[6] L5: Capitalize African Humid Period (AHP)?

In a revised version we will capitalize the AHP.

[7] L25: „1.7 ka to 4.2 ka," Where are these definite numbers from? There is a reference needed. Also, a broadened perspective on when, where, and how the AHP ends would be helpful (e.g., abrupt/gradual, eastern vs. western Africa, or N-S transect, e.g., Shanahan et al., 2015)

The missing reference is Claussen et al. (2017), but we agree that with the broadened perspective we do not need such precision in the ages. In the revised version, we will briefly mention the spatial heterogeneity and abrupt and gradual changes making also a reference to Dallmeyer et al. (2020).

[8] L110: Marine sediment reflectance is rapidly introduced here and would need some more explanation for the non-marine audience

The revised version of the manuscript will include a sentence in the main text next to the citation of the Cariaco Basin record.

[9] L115: More wording could be helpful to explain that the output of the model is compared to proxy results and other model insights

The revised manuscript will include an opening paragraph to introduce in such way the results section.

[10] L145: Eastern Africa instead of East Africa, as East Africa refers to the colonial name, whereas eastern Africa is the geographical term

We thank the reviewer for the clarification. This will be corrected in the text.

[11] L165: If the last AHP, which has the best ground truth so far, is not correctly reconstructed, how do we assume the model works correctly?

The climate model we use has similar assumptions and biases as other state-of-the-art general circulation models (GCMs). It is periodically evaluated as part of model inter-comparison palaeo-experiments (Braconnot et al., 2012; Brierley et al., 2020). Recent evaluations of the simulated Holocene show the model aligns well with general trends in current proxies for northern Africa and globally (Dallmeyer et al., 2020, 2021). Combined with several deglaciation experiments since the Last Glacial Maximum until present, which were performed with the same model (e.g. Dallmeyer et al., 2022; Kleinen et al., 2023), it gives us confidence the model tends to work reasonably well, at least in broad agreement with current evidence, although with the known limitations of GCMs. It was those previous applications that alerted us that the Holocene AHP should not have appeared as weak as it did. The extent to which a weak Holocene AHP affects our analyses and the pattern-scaling is difficult to measure, but we do not expect that our results would be qualitatively different.

[12] L235: I think this is an exciting output of the study as it shows, with a simple linear model, the contributions of the forcing factors to Africa's climate heartbeat. For the last sentence, I would pronounce the weakening of the orbital forcing without necessarily increasing NH forcing on tropical African climate.

We agree with the reviewer on rather emphasizing the role of the weakening of the orbital forcing in the last sentence.

[13] L325: Indeed, pattern scaling is an empirical method, and there is no precursor to having the model trained on, so it is extrapolating; hence, reaction patterns to this GHG forcing are simply unknown.

We thank the reviewer for the clarification. In spite of the uncertainty in such extrapolation we find it interesting to discuss jointly the output of the dynamical numerical model and the pattern scaling in such future scenarios, as a way to understand the differences between changes driven mainly by Earth's orbit or GHGs.

[14] L375: An AHP SW-NE tilt exists in the Krapp et al. (2021) dataset, foremost in the MAP. It is weak and underestimated compared to the terrestrial observations, but it exists.

We thank the reviewer for pointing this out, since we had only looked at the biomes output. We will revise the comparison with this dataset.

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