Response to the Editor's comments

Firstly, we thank Prof. Claussen for considering our manuscript fitting the scope of the journal and advancing it for peer-review stage. We take here the opportunity to address his insightful comments as follows:

The term "fully coupled model" may indeed give the impression of a more extensive coupling than is present in our simulations. The model we used is an atmosphere-ocean coupled model without dynamic interaction with vegetation, lakes, soil and carbon cycle. We have revised the terminology in the manuscript to reflect this more accurately, now referring to it as an "atmosphere-ocean coupled climate model".

With respect to the sensitivity of the EC-Earth model to dust effect, we first highlight that, although including dust reduction does increase simulated rainfall by about 30%, most of the monsoon strengthening observed in our simulations is attributed to the effects of Sahara greening (see Pausata et al. 2016). Furthermore, a strong impact from dust reduction in the strengthening of the African monsoon during the MH has been reported by Thompson et al. (2019), showing a contribution of about 15%–20% to the total increase in rainfall. Hopcroft and Valdes (2019) discussed the variability in dust-precipitation feedbacks dependent on dust optical properties and particle size with precipitation changes ranging from -20% to +50%. Similarly, Sagoo and Storelvmo (2017) have shown significant rainfall changes simulated for idealized low and high dust concentrations. These studies support the robustness of our results against the model sensitivity to dust effect. We add a sentence in Section 2 to address this aspect:

"The inclusion of dust reduction in the experimental setup significantly influences the simulation of the monsoonal dynamics, leading to an increase of around 30% of cumulated precipitation compared to simulations with prescribing vegetation only (see Pausata et al., 2016). Similar impact has been observed in other modelling studies by Thompson et al. (2019), Hopcroft and Valdes (2019) and Sagoo and Storelvmo (2017), confirming the relevance and comparability of the model's response to dust effects".

Pausata, F. S. R., Messori, G., and Zhang, Q.: Impacts of dust reduction on the northward expansion of the African monsoon during the Green Sahara period, Earth Planet Sci Lett, 434, 298–307, https://doi.org/10.1016/j.epsl.2015.11.049, 2016.

Thompson, A. J., Skinner, C. B., Poulsen, C. J., & Zhu, J. (2019). Modulation of mid-Holocene African rainfall by dust aerosol direct and indirect effects. Geophysical Research Letters, 46, 3917–3926. https://doi.org/10.1029/2018GL081225

Hopcroft, P. O., & Valdes, P. J. (2019). On the role of dust-climate feedbacks during the mid-Holocene. Geophysical Research Letters, 46, 1612–1621. https://doi.org/10.1029/2018GL080483

Sagoo, N., and T. Storelvmo (2017), Testing the sensitivity of past climates to the indirect effects of dust, Geophys. Res. Lett., 44, 5807–5817, doi:10.1002/2017GL072584.

Response to R1

The authors analyse mid Holocene climate model simulations with and without taking into account the effects of prescribed Saharan greening (SG). The model experiments using EC-Earth have been described elsewhere, and the new aspect of this paper is the focus on mid-latitude atmospheric circulation patterns and changes in weather regimes (WR). The authors discriminate between changes induced by the orbital and GHG forcing as in the standard PMIP3 simulation, and additional modifications of climate features by including prescribed SG.

The topic of the manuscript addresses a relevant scientific question in the discussion of causes for past climate changes on the regional and continental level, where a lot of discrepancy remains between proxy reconstructions and model simulations.

Extending the analyses to the mid-latitudes is therefore timely and useful and provides important insights.

The study underlines the importance of including changes in vegetation and the related dust fields not only for the monsoon systems, but also for mid-latitude circulation and circulation patterns. The authors find significant effects coming from SG for temperature and precipitation fields. Regarding variability patterns, the effect of SG is merely an enhancement of effects that are already seen in the standard PMIP experiments, although they are not making an attempt to explain why the SG effects would do that.

Another important finding is that one has to include variability on the daily timescale to refine the regional effects by means of WRs, which sometimes give different patterns than circulation patterns based on monthly data. The authors also discuss if and how much the proxy-data gap can be narrowed down in the SG simulation, but with rather limited success.

Overall, the manuscript is written in a clear and concise way and the findings are well supported by the prescribed analyses and figures.

Save for a few inconsistencies, I find the manuscript quite mature and I would recommend that the paper can be published after (very) minor revisions.

We thank the reviewer for the time s/he spent in revising our work and her/his positive reception, as well as for the comments which will be useful to improve the manuscript.

Minor issues:

Abstract, ln 21ff: Don't you say in the paper that the NAO+/- shift comes about in the run without SG and that SG just enhances this effect (c.f., Fig. 6)?

Thanks for highlighting this oversimplifying phrasing. From figure 6 we see that monthly NAO shits from prevailing positive in PI to prevailing negative in both the sensitivity experiments in summer, while it shifts to negative in the SG experiment and neutral in the no-SG experiment. We rephrase to clarify:

"Furthermore, the Saharan greening modifies the atmospheric synoptic circulation over the North Atlantic, enhancing the effect of the orbital forcing on the transition of the North Atlantic Oscillation phase from prevailingly positive to negative in winter and summer".

Figure 1: blue dots, in particular over southern Europe are hard to distinguish from the gray ones, maybe make them slightly bigger?

The figure is now improved, by making the grey dots lighter, to make blue and red dots more visible.

Ln 99: how uncertain is this estimate?

"This vegetation cover leads to an average decrease in surface albedo from 0.30 during the PI period to 0.15".

There's no uncertainty in the surface albedo value, because this is part of the vegetation prescription in the simulation, that is changing the desert albedo (0.30) to shrub albedo (0.15). We modify the sentence for clarification:

"This modification in land surface type leads to a change of surface albedo from 0.30 (desert) to 0.15 (shrub)".

Ln 115: a few more words about the Cohen and how it is applied over the selected regions should be included so readers don't have to look it up.

Thanks for this comment. A short description of the computation of the Cohen index is now added:

"The Cohen's Kappa index quantifies agreement between climate variables form climate simulations and proxy reconstructions through the probability that, at the proxy sites, the two data sets agree on the category of anomaly (e.g., positive/negative/no change in both simulation and proxy), but not by chance alone. The index is calculated by constructing a data matrix with the number of sites where the two data sets agree, partially disagree (one indicates a positive or a negative anomaly, while the other indicates no change) and completely disagree (one indicates a positive anomaly while the other indicates a negative anomaly, and vice versa). This data matrix is then multiplied by a weight matrix, to penalise complete disagreement more than partial disagreement. The resultant values for the index range from 0 to 1, where 0 indicates no agreement, 0.5 indicates partial agreement, and 1 indicates perfect agreement”.

Ln 295: maybe include also a discussion how that relates to the suggestions by Mauri et al. (Clim Past, 2014)

Many thanks for suggesting this interesting reference, that we missed. The comparison with Mauri et al. 2014 is now discussed in Section 5:

“The circulation anomalies associated with SB in winter and NAO+ in summer are consistent with those suggested by Mauri et al. (2014) to explain the MH thermal and precipitation anomalies in Northern Europe, namely stronger westerly and southerly flows towards Scandinavia in winter and summer”.

Ln 381: “impacts” of what?

The impact of simulating with and without SG. Now rephrased:

“The MH experiments show significant changes in both surface temperature and precipitation in the mid-to-high latitudes with respect to the PI control simulation”.

Ln 394: what gives you the idea of looking into changing ocean circulation, any references for that?

We did not look into the ocean circulation indeed; this was beyond the intended scope of this study. Our considerations come from the distinct temperature response noted in our simulations, specifically the more pronounced warming in the MHGS simulation compared to MHPMIP (see Fig. 2), particularly in the North Atlantic (see Fig. 1), which suggests that the AMOC might respond to changes induced by the Sahara greening, potentially influencing the atmospheric circulation and temperature. We rephrase to clarify:

“Moreover, the fact that the warm anomalies are more pronounced in MHGS than in MHPMIP simulation points to other drivers, beside the insolation, that could have amplified the warming. In particular, the prominent warming in the North Atlantic simulated when vegetation is prescribed in the Sahara raises the possibility of modifications in the ocean circulation (see e.g. Zhang et al. 2021, who found a strengthening of the Atlantic Meridional Overturning Circulation in response to the simulation of a green Sahara). Such changes could feasibly feedback on the atmosphere. Consequently, further studies focused on investigating potential changes in ocean circulation associated with the Saharan greening would be valuable for a better understanding of the widespread warming seen at mid-to-high latitudes”.