Point-by-point reply to comments by the editor and reviewer#2

We would like to thank the editor and the reviewer for their efforts in reviewing our revised manuscript. Our detailed response to all comments is provided below. For clarity, the editor's and the reviewer's comments are shown in black and our response in blue.

Editor's comment:

The manuscript will be accepted for publication in Climate of the Past once the minor revisions have been satisfactorily addressed. There will be no further round of external peer review. I will review your point-by-point response and make the final decision. In preparing the final version, please address the referee's #2 specific corrections to wording, geographical names, and notation. Some comments are food-for-thought, e.g., the authors could comment on the controversial role of land use in contributing to the end of the Green Sahara period.

We thank the editor for his effort. In the revised manuscript, we follow the reviewer's suggestions on wording, geographical names and notation. We respond to the reviewer's comments in detail and we explain how we consider the comments in the revised manuscript.

Reviewer's comments:

One point that I thought about while reading the revision that I neglected to comment on in the first version of the manuscript – I apologize for this – is that it would have been nice if the authors could comment on the controversial role of land use in contributing to the end of the Green Sahara period (Wright, 2017; Brierley et al., 2018; Phelps et al., 2020). Do the simulations presented in this study leave unexplained variance or temporal patterns in vegetation cover change that could be a result of human activities? The quantitative analyses presented in this manuscript highlight how the paucity of well-dated, high-resolution, and geographically representative paleodata make it very difficult to unequivocally invoke the need for or against human actions as being necessary drivers. Nevertheless, if the MPI-ESM simulations can explain most of the AHHP land cover changes without any human action, the results may contribute to this debate. Any comment from the authors would be welcome.

We thank the reviewer for bringing up the issue of land use which still seems to be one of the unsolved "Green Sahara riddles". Wright (2017) argues that herding with domesticates in combination with climatic change could have accelerated the end of the Green Sahara. We have found unambiguous indicators of cultivation such as increased frequencies of date or doum palm pollen only in the Lake Yoa record, as mentioned in our manuscript. These indicators appear, however, during the last some 2000 years, i.e. significantly later than the termination of the AHHP in this region. Brierley et al. (2018) argue that herding may have increased the resilience of grasslands and thus postponed and slowed down the end of the Holocene Green Sahara. We agree with the reviewer that the paucity of well-dated, high-resolution, and geographically representative paleodata makes it, very difficult to unequivocally invoke the need for or against any effect of land use on the termination of the AHHP and the end of the Green Sahara.

In our simulation, land use is prescribed following the LUH2 land-use forcing by Hurtt et al. (2020). This forcing complies with the CMIP6 protocol, it is read annually, but is calculated daily by linear interpolation. The forcing begins 850 CE with a quasi-linear transition period (1000 years) starting 150 BCE to slowly build up the land use. For Northern Africa, prescribed land use only occurs in a very few grid cells outside the region of the Sahara – Sahel transition. Our simulation and earlier simulations (e.g., Claussen et al., 1999; Hopcroft and Valdes, 2022) thus reveal abrupt changes without any early land use in the Sahara. Following Occam's razor, we tentatively conclude that land use might have played only a minor role in the large-scale dynamics of the Green Sahara. We add

two statements, the first one to the Method section 2.1, lines 95 – 98, on the land-use forcing prescribed in our simulation, and the second one to Discussion Section 4.4 (Challenges in comparing model and data), line 679 – 690, on the topic of land use as mentioned above.

Specific comments
Line 238
For clarity of wording, change “exemplary” to “example”

We have skipped the word “exemplary”, and we have modified the caption in response to the comment below.

Line 241-242
For completeness, provide a short definition of the abruptness factor “A” as it is shown in the figure title line, as is done for “O” and “R”.

done

Line 312
The new Table 1 is very helpful, thank you. Please add a header row for clarity.

We are not sure, what is meant. Table 1 has a title line: “List of symbols”, and such lists of symbols generally do not need an extra header row, because it is self-explaining that the first column lists the symbols and the second, the meaning of the symbols.

In the table, the definition of “O” should have a qualifier such as “normalized” added to it, because as it is explained in the text and Figure 1 caption, “O” is a unitless factor but the way it is described in the table it could actually be a value with units.

done

Line 348-349
To follow general geographic naming conventions in English, change “Levante” to “Levant” and “Ethiopian Highland” to “Ethiopian Highlands”.

done

Line 535-536 and Figure 8
Could the lack of a relationship between vegetation cover and precipitation in the earth system model be indicative that, at least in the study region, MPI-ESM just has a weak land-atmosphere coupling? Studies over the years have shown that different climate models display varying degrees of the land-atmosphere coupling strength, although the southern Sahara/Sahel was identified as a region where several models had stronger than average coupling (Koster et al., 2004). I don’t think you need to do a lot here, maybe just a brief comment on the expected coupling strength in the version of MPI-ESM used in these simulations.

We have unfortunately not available an analysis of the “coupling strength” (as proposed by Koster et al. 2004) for the version of the MPI-ESM used in our study. Bali and Collins (2015) used the Koster et al metric to show that the predecessor version to the MPI-ESM, the ECHAM5/JSBACH, reveals significant coupling strength in the Sahel region and the South Asia monsoon region with respect to the coupling of soil moisture, phenology and climate variability for present-day climate. Likewise, we

see a significant effect of changing the surface albedo scheme on simulated Holocene precipitation in Northern Africa (Vamborg et al., 2011, 2013) and a significant effect of the selection of PFTs on the Holocene climate dynamics in Northern Africa (Groner et al., 2018). Hence we suppose that there is a significant coupling of vegetation and climate in the Sahara/Sahel region in our MPI model systems

The term "weakly positive feedback" in the sense of (Liu et al., 2006) means that there is a significant feedback between vegetation and precipitation, but the feedback is just not strong enough to trigger an abrupt change in the time series of precipitation. "Coupling strength" and "feedback strength" are different metrices. Since we cannot quantify them for the purposes of a nuanced discussion, we would prefer to refrain from adding a comment on "coupling strength" in the revised manuscript. This might only lead to some confusion, rather than clarification.

Line 565
Because JSBACH is a deterministic vegetation model, in some way the biogeographic response of vegetation cover to climate change should be predictable in advance. Nevertheless, this relationship is complex and determined by a range of competing factors as discussed in section 4.3 of the manuscript. So, I am ok with treating the model behavior as an emergent property here and appreciate the further discussion later in the paper.

ok

Line 637
Is it a reasonable assumption that "vegetation-induced changes in albedo and evapotranspiration [do not really] feed back on regional climate" in MPI-ESM, as noted in Figure 8? See comment above. Not really sure you need to do much here, except maybe refer to a comment on coupling strength in MPI-ESM.

As mentioned above, there is a significant feedback between vegetation and near-surface climate, albeit it might be not strong enough to trigger abrupt change in vegetation and climate. We therefore prefer to keep this sentence as it is.

Line 640-641
I agree it would be great to do sensitivity analyses on the climatic controls on vegetation distribution!

Thanks a lot!

Line 648-649
In addition to using different types of vegetation models, it would be interesting to try using forward proxy system models that directly simulate, e.g., pollen production, transport and deposition, to evaluate model-data agreement. Such models do not really exist for pollen at the moment but are widely employed with geochemical paleo-proxies and could in the future.

We fully agree. Perhaps one could use a similar approach as for ECHAM-HAM model which includes a model (HAM) for the emission, transport and deposition of a spectrum of aerosols (Egerer et al., 2016). We add a sentence to the first paragraph of Section 4.4, lines 653 – 656.

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