Point-by-point reply to comments by the editor and the reviewers

First, we would like to thank the editor and the reviewers for the thorough and constructive review of our manuscript. Our detailed response to all comments is provided below. For clarity, the reviewer's comments are shown in black and our response in blue.

Please note that our response to the reviewer's comment is basically the same as our published response. We mainly added references (e.g. line numbers) to the changes we made in the revised manuscript.

Editor: Public justification (visible to the public if the article is accepted and published):
The two reviewers are highly positive, and I agree that the manuscript presents a valuable contribution that is suitable for publication after revision. The responses address most of the reviewers' technical comments carefully. Before a final decision, however, I ask the authors to strengthen the broader scientific interpretation and to clarify more sharply what is genuinely new.

Clarify the novelty and central scientific result: The manuscript should state much more explicitly what is learned beyond the established conclusion that the termination of the African Humid Period was spatially heterogeneous and proxy-dependent. In my reading, one important new result is not simply that some changes were abrupt and others gradual. Rather, the analysis suggests that vegetation composition can undergo substantially more abrupt transitions than total vegetation cover, and that a gradual large-scale expansion of desert conditions can coexist with local or ecosystem-specific step-like reorganizations. Please formulate two or three explicit statements of novelty near the end of the Introduction and return to precisely these statements in the Abstract and Conclusions.

We fully agree with the editor that the novel aspects of our study should be highlighted more clearly, particularly in view of the reviewers' comments. **(Revisions see lines 9/10 and 21-24 in the Abstract, lines 53/54 and 61/62 in the introduction and, specifically, lines 677/678, 699-703 in the new conclusion.)**

Strengthen the proxy synthesis: Please provide a clearer conceptual synthesis of the environmental variable represented by each proxy type, its spatial source area, its typical temporal resolution, and the extent to which it can be compared with simulated bare-surface fraction or individual PFTs.

We have used the well-established concept of phytogeographical regions and groups as outlined in the classical paper by White (1983), for example, and, more relevant for our study, as discussed by Dinies et al. (2026). We have thus restricted ourselves to a brief summary in Section 2.2. The temporal resolution of proxies is given in the Table A1 and discussed in **the new Discussion Section 4.4: Challenges in comparing model and data (lines 665 – 671)**. We have more clearly highlighted the difficulties in comparing the phytogeographical groups with simulated vegetation changes in **Section 2.3 (lines 164 – 183)** and in the **Discussion Section 4.4: (lines 650 – 654)** including cautious statements on the topic of vegetation cover, PFTs, pollen abundance, and source area of pollen record. The source area of dust found in the marine dust records is mainly the western Sahara, as discussed in detail by Egerer et al. (2016, 2018) who were the first to provide a link between the changes in marine dust flux and uptake of dust in the western Sahara. We have added sentences in relation to this topic **(see our response to Jed Kaplan, reviewer#2)**.

Provide a clearer analysis of the climatic drivers: The climatic pathway from orbital forcing to the detected vegetation transitions should be discussed more explicitly (including precipitation, seasonality, monsoon strength, and SST-related changes).

To a first approximation vegetation changes in the Sahara can be related to changes in precipitation. In **a new Section 3.4**, we have thus addressed the question of whether the abrupt changes in bare surface and PFTs found in our simulations could be related to abrupt changes in precipitation. The simple answer is: no, they cannot. We went a step further to explore the question of whether the abrupt changes in bare surface and PFTs are associated with thresholds in precipitation as discussed in the study by Liu et al. (2006) who proposed the idea of unstable vs stable collapses.
The idea to relate vegetation changes in the Sahara to precipitation changes is a simplified assumption, of course. Neither in JSBACH nor in natural ecosystems is vegetation determined by precipitation alone. We have thus **added a Section 4.3** in the newly structured Discussion to critically assess the question of climatic drivers of abrupt vegetation changes in the Holocene Sahara. We sketch the causal pathway of climatic drivers from orbital forcing through changes in the seasonal climate, sea-surface temperatures and atmospheric circulation to the meteorological variables and ecosystem processes that directly regulate plant growth. We argue that targeted sensitivity experiments would be needed for a clear attribution analysis. This could be an interesting topic for a subsequent study.

Separate forced trends, internal variability, and threshold crossing: Incorporate the ensemble comparison into the manuscript and discuss whether internal variability primarily affects the timing of transitions, while orbital forcing controls the broader spatial pattern.

We appreciate this important comment very much which was also raised by reviewer #1. We have therefore **added a Section 4.2 in the Discussion**.

Develop a careful “lesson from the past”: Explain what can, and what cannot, be transferred from the Holocene Green Sahara to present-day climate change and ecosystem responses to gradual forcing.

We agree that this is an important aspect in paleoclimate science, because most projects claim to learn from the past to better assess future climate changes. But can this really be done? We first argue that the Holocene transition is not a proper analog for present-day and future climate change because of the differences between past and present climate forcing. But there is a fairly general “lesson from the past”: As in the past, future vegetation changes in the Sahara and Sahel are likely associated with threshold dynamics even in the ongoing rapidly evolving anthropogenic forcing. And we might expect more rapid changes in the regional vegetation composition than in the large-scale vegetation cover. See our **Conclusion (lines 704 – 713 in the new Section 5).**

Recast the final conclusion: End with a short hierarchy of conclusions (what the quantitative framework adds, what the combined model-proxy analysis establishes, what remains unresolved, and what general insights emerge regarding ecosystem responses).

We have to admit that our previous conglomerate of discussion and conclusion lacks clearity and conciseness. We hence appreciate this comment. We have separated Discussion (Section 4) and Conclusion (Section 5). The Discussion focusses on an assessment of Defining and quantifying abruptness (4.1), Robustness of results (4.2), Climatic drivers of vegetation transitions (4.3), Challenges in comparing model and data (4.4). The Conclusions address the aspect as suggested by the editor.

The manuscript is of high quality, and a thorough revision along these lines has the potential to make it a particularly strong contribution to the discussion of abrupt environmental change during the African Humid Period.

We are pleased with the appreciation of our work and we thank the editor for his comments and suggestions to improve our text.

References:

Dinies, M., Hoelzmann, P., Lebamda, J., Razanatsoa, E., Wotzka, H.-P., and Darius, F.: Ecological shifts from the Sahara to the Tropical rain forest: The past 6,000 years, in: Entangled Africa, 189–219, https://doi.org/doi.org/10.34780/42s5am91, 2026.

Egerer, S., Claussen, M., Reick, C., and Stanelle, T.: The link between marine sediment records and changes in Holocene Saharan landscape: simulating the dust cycle, Clim. Past, 12, 1009–1027, https://doi.org/10.5194/cp-12-1009-2016, 2016.

Egerer, S., Claussen, M., and Reick, C.: Rapid increase in simulated North Atlantic dust deposition due to fast change of northwest African landscape during the Holocene, Clim. Past, 14, 1051–1066, https://doi.org/10.5194/cp-14-1051-2018, 2018.

Liu, Z., Wang, Y., Gallimore, R., Notaro, M., and Prentice, I. C.: On the cause of abrupt vegetation collapse in North Africa during the Holocene: Climate variability vs. vegetation feedback, Geophys. Res. Lett., 33, 2006GL028062, https://doi.org/10.1029/2006GL028062, 2006.

White, F.: The vegetation of Africa: a descriptive memoir to accompany the Unesco/AETFAT/UNSO vegetation map of Africa, Unesco, Paris, 356 pp., 1983.

Reply to Review #1

We would like to thank the reviewer for the thorough and constructive review of our manuscript. The well-considered comments and suggestions will help us clarify some points that might otherwise have been misunderstood or that simply benefit from further elaboration. Our detailed response to the reviewer's comments is provided below. For clarity, the reviewer's comments are shown in black and our response in blue.

This manuscript provides a highly rigorous quantitative framework to evaluate the termination of the African Holocene Humid Period (AHHP). The authors successfully move the long-standing debate from a qualitative discussion of "collapse vs. gradual decline" to a standardized, metric-driven assessment. The integration of transient simulations from the MPI-ESM with carefully aggregated pollen groups and marine dust flux records is solid. By applying a changepoint detection method to linear regressions, the authors effectively demonstrate that the end of the Green Sahara was characterized by a complex spatial and temporal mosaic: a gradual expansion of desert areas on a macro scale, accompanied by abrupt, step-like transitions within specific PFTs and regional ecosystems. The discussion of the limitations of the single model study (such as the challenge of representing savanna biomes in JSBACH), and the uneven distribution of proxy data availability provides a clear path forward. The manuscript is a pleasure to read, and my comments below are offered as minor (and mostly technical) suggestions to further polish an already outstanding paper.

We are very pleased with this statement, and we appreciate very much the concise questions and comments which we respond to below.

1. As the authors noted, the complete omission of the Sahelian group from the model comparison—due to JSBACH's known inability to accurately simulate savanna ecosystems—is a significant limitation. Given that the Sahelian transition is central to the aridification of the Sahara, the authors should explicitly outline how the lack of a savanna biome representation might skew the modeled rapidity of the bare-surface expansion. If grass and trees cannot properly co-exist as savanna in the model, does the transition to bare soil appear artificially abrupt?

Our statement (line 184) that "the land-surface model JSBACH is known to not properly describe any type of savanna (Groner et al., 2018) " is misleading in this context. First we apologize for the erroneous citation: not Groner et al. (2018), but Dallmeyer et al. (2021) showed that the simulation we analyzed in our study captures the present-day global patterns of the savanna biome with the poorest skill compared to all other biomes in terms of best neighbor scores (see Dallmeyer et al., 2021; Table 2). Dallmeyer et al. (2021) mention that mapping savannas in terms of biomes is limited in JSBACH, because savannas are affected more by perturbations like fire and windbreak and to a lesser extent by climate. In addition, the various types of savanna mainly differ by their proportions of tree.

However, JSBACH does describe a savanna; the grid boxes in the SW of the Lake Yoa (Fig. 4, see also our answer in response to comment 3) can be taken as an example. At the beginning of the simulation around 8 ka, the MPI-ESM produces a lush green savanna dominated by grass in this region. Around 5 ka, the grass fraction considerably declines while the tree fraction is almost constant. After around 3 ka, the tree PFT starts to vanish, and the bare surface fraction in the MPI-ESM increases well above 0.8 after 2 ka. Thus, the rapid increase in simulated bare surface fraction is not caused by the absence of a savanna, but by a change in the type of savanna, from a lush grass savanna to a meager tree savanna, and finally, to a desert.

What we actually wanted to say – and say in the **revised manuscript (lines 206 – 212)** – is the following. Sudanian and Guinean taxa are clearly dominated by the abundance of tree species and, hence, can be more or less directly compared with the fraction of simulated tropical tree PFT. The Sahelian

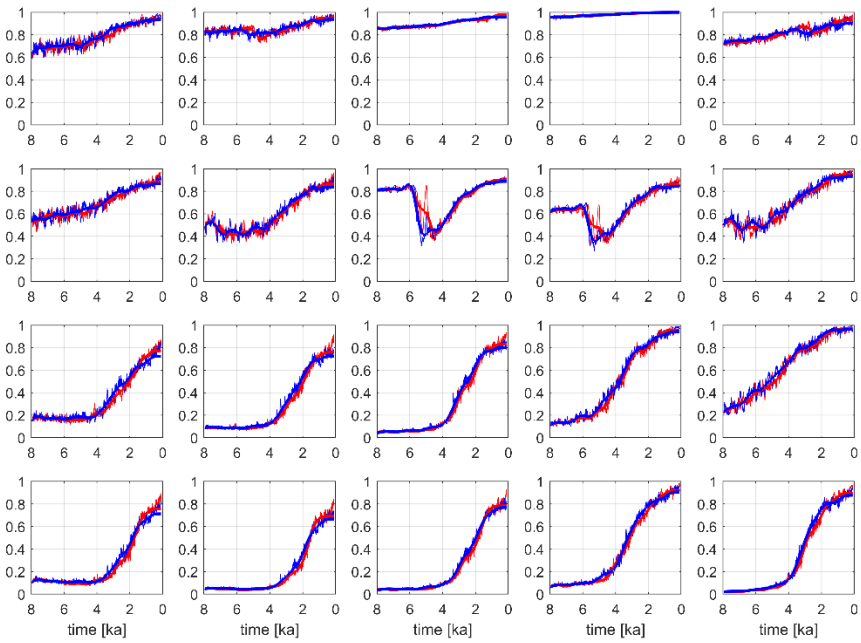
group is defined by a mixture of grass, shrub and tree species, depending on the topography, the occurrence of bush fire and climate. This group can thus not realistically be compared with the limited numbers of PFT simulated in JSBACH (Dallmeyer et al., 2021).

As a side remark in response to this comment only: We have tried to compare the Sahelian group with some flexible proportions of simulated PFTs or by using some non-linear function of the proportions of simulated woody types. But we were not successful so far.

2. Orbital forcing vs. internal variability. The definition of abruptness relies on changes occurring faster than the linear trend in orbital forcing. While this is a solid baseline, it would be beneficial to elaborate on the potential role of unforced, centennial-scale internal climate variability. Does the MPI-ESM transient simulation show periods where internal variability can coincidentally push the vegetation over a bioclimatic threshold or beyond the defined abruptness threshold, accelerating the transition independently of the orbital decline?

In the MPI-ESM, the coherent patterns of largest abruptness in northern Africa are found in the region of the largest vegetation changes in a strip around 20°N in the west and 15°N in the east. This pattern of large vegetation changes seems to be mainly dominated by the orbital forcing rather than by internal climate variability. We can show this by comparing two different simulations from the same ensemble of our transient Holocene simulations, but with different initial conditions and with a different volcanic forcing.

The figure below depicts the bare land fraction in the western Sahara region (see Fig. 3 in our manuscript) for the simulation used in our study (blue line) and for a simulation with a presumably unrealistically strong Holocene volcanic forcing (red line). Please note that we switched to the more conventional notation 'ka' = 1000 years before present as suggested by Reviewer#2. Both simulations differ with respect to their internal climate variability. But the vegetation changes seem to differ only on very short time scales, but not on much longer time scales and not on the long-term trend. Only the "cumbersome grid boxes" (second row, 3rd and 4th column) discussed in our Appendix B as model artefacts show a somewhat larger deviation between simulations. Such grid boxes are rare exceptions from the rule when considering all grid boxes of our study area.

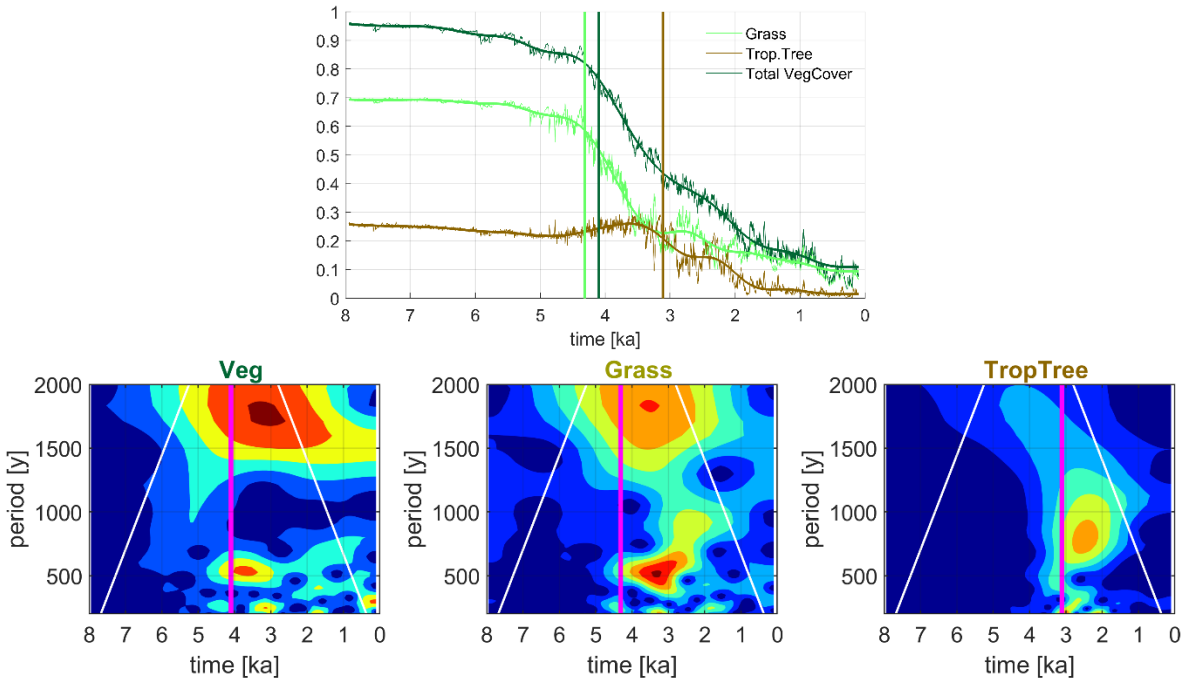


Thus, we suppose that the coherent pattern of largest vegetation changes, and hence largest abruptness, is a robust feature, mainly triggered by changes in orbital forcing. We agree with the reviewer that a brief statement on this issue is sensible to add which we have done in the **new Discussion Section 4.2. Robustness of results (lines 591 – 616).**

3. To restrict the changepoint detection a priori to a single transition in linear regression is a logical choice to isolate the primary step-like termination of the AHHP. However, some of the highly variable proxy records (and perhaps some regional grid boxes in the transient simulation) might naturally exhibit multi-stage transitions or "flickering" before settling into a desert state. The authors might consider adding brief discussion on whether releasing this single-changepoint constraint in future studies might reveal secondary and smaller transitional phases.

We have excluded a discussion of multi-stage transition and flickering from our study, because we wanted to focus on the abruptness of step-like transitions which is a “logical choice” as stated by the reviewer. Many simulated time series and proxy records considered in our analysis of the last 8000 years are characterized by a big transition either as a step change or as a change in the trend. But there are grid boxes and proxy records that reveal small long-term trends with large fluctuations. If we would consider multiple changepoints to analyze the grid boxes characterized by large variability, then the quantitative definition of abruptness would become a challenge. Would the number of changepoints be a measure of abruptness or the strongest abruptness of the various changepoints in a grid box? Or something else? We mention this point in the **newly structured Discussion (Section 4.1 Defining and quantifying abruptness, lines 580 - 590)**. For other records and simulations, an analysis of multiple changepoints makes perfectly sense, of course, as for example for the long Chew Bahir record (Trauth et al., 2018).

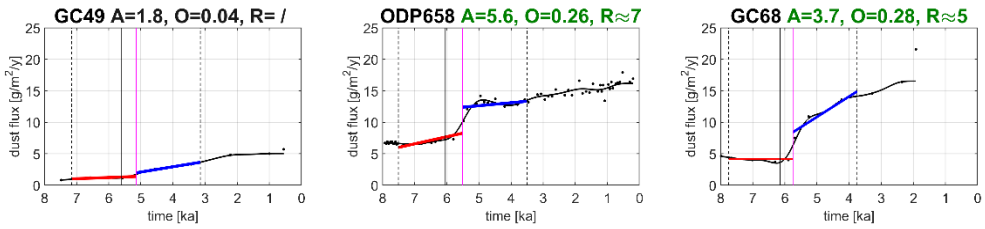
As for the “flickering”, we have begun to look into this topic. Flickering does occur in our simulation. The figure below shows the time series and wavelet analysis of time series of grass PFT, tropical tree PFT and total vegetation cover in a grid box in the southwestern part of the Lake Yoa region.



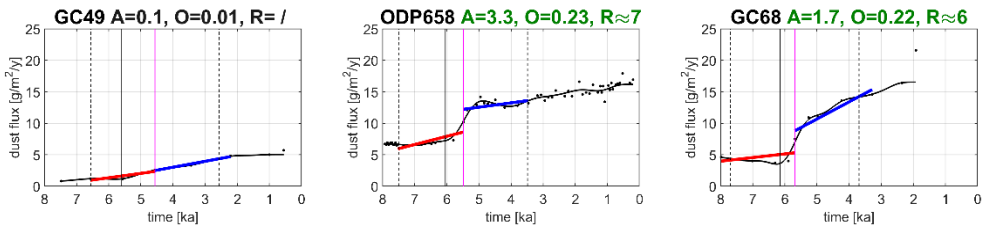
It is interesting to see that flickering starts prior to the respective changepoints (vertical straight lines). Please note that the times series were detrended by using high-degree polynomial before applying the spectral analysis. With some imagination, even a shift towards larger frequencies / longer periods can be found in the grass and the tropical tree PFTs, but after the changepoint, which could point at an early warning signal as the PFTs approach a bifurcation point. This, however, is work in progress, not mature enough to be discussed in our present manuscript.

4. To ensure consistency between the high-resolution simulation output and the proxy data, the authors interpolate pollen and dust records to annual values before applying a 500-year Butterworth filter. Because the temporal resolution of the proxy records varies considerably (e.g., from roughly 50 years to nearly 400 years for pollen, and up to 990 years for the GC49 marine core), the authors might briefly clarify if the interpolation process introduces any minor spectral artifacts or aliasing, particularly for the coarsest records. A minor note assuring the reader that the changepoint detection is insensitive to this specific interpolation step would be a helpful technical addition.

This is an interesting point which we briefly mention in the revised version of our manuscript **(lines 261 – 267)**. We have considered using the raw proxy data without any interpolation. It appears that the use of raw data does not affect the detection of changepoints, except for "marginal changepoints" as shown in the case of the marine record GC37 depicted in the figures below. However, the values of abruptness differ between the analysis of interpolated and non-interpolated time series. With apparently similar offsets and similar regression lines, we can attribute the reduction in the A values mainly to an increase in standard deviation of data from the linear regression lines in the non-interpolated time series. Hence, we find it sensible to interpolate the proxy data to the same resolution as the simulated data, thereby putting the abruptness statistics of data and model on the same footing.



(analysis with interpolation of data)



(analysis without interpolation of data)

In a revised version of our manuscript, we will briefly mention this effect of interpolation on the detection of changepoints and evaluation of A without showing the figures.

Typos:

Thanks a lot for spotting the typos which we will eliminate in a revised version of our manuscript

References

Dallmeyer, A., Claussen, M., Lorenz, S. J., Sigl, M., Toohey, M., and Herzschuh, U.: Holocene vegetation transitions and their climatic drivers in MPI-ESM1.2, Clim. Past, 17, 2481–2513, https://doi.org/10.5194/cp-17-2481-2021, 2021.

Groner, V. P., Raddatz, T., Reick, C. H., and Claussen, M.: Plant functional diversity affects climate–vegetation interaction, Biogeosciences, 15, 1947–1968, https://doi.org/10.5194/bg-15-1947-2018, 2018.

Trauth, M. H., Foerster, V., Junginger, A., Asrat, A., Lamb, H. F., and Schaebitz, F.: Abrupt or gradual? Change point analysis of the late Pleistocene–Holocene climate record from Chew Bahir, southern Ethiopia, Quat. Res., 90, 321–330, https://doi.org/10.1017/qua.2018.30, 2018.

Reply to Review #2 by Jed Kaplan

We would like to thank Jed Kaplan for his thorough and constructive review of our manuscript. The well-considered comments and suggestions will help us clarify some points and make our manuscript easier to read. Our detailed response to the Jed Kaplan's comments is provided below. For clarity, his comments are shown in black and our response in blue.

This manuscript describes a study to investigate the end of the "Green Sahara" period in the middle Holocene. First noticed by explorers of the Sahara nearly a century ago, research has demonstrated that much of the area of northern Africa that is currently occupied by arid and hyper-arid deserts was occupied by grasslands, shrublands, and savannas, and lakes and wetlands during the mid-Holocene. This transition from vegetated and watered surfaces to extreme desert represents one of the greatest land cover changes over the preindustrial Holocene and in terms of areal extent would be second only to the waxing and waning of the ice sheets on Quaternary timescales. While it is well understood that the ultimate driver of land cover change in the Sahara is orbital forcing, mainly the precessional cycles, that slow and relatively linear effect does not correspond well with paleoenvironmental reconstructions that suggested land cover change in the Sahara may have been abrupt. An abrupt end to the Green Sahara period was initially posited by analyses of the dust record in Atlantic Ocean marine cores off the coast of West Africa. In the more than 25 years since these records were published, a wide range of studies have attempted to understand the timing and extent of the end of the Green Sahara, not least in part because it is one of the best examples we have of a rapid change in land cover in response to slow forcing, which could contain lessons for the future about the stability of terrestrial ecosystems in the context of the buildup of anthropogenic greenhouse gases in the atmosphere.

In this manuscript, Claussen et al. describe the most comprehensive and quantitative study to-date to understand the spatial and temporal signature of the end of the Green Sahara period based on paleoenvironmental archives and, using an earth system model, investigate the mechanisms that caused that dramatic land cover change. Overall, this is a fascinating study and a detailed, and well-presented manuscript that will certainly be of interest and value to the community. There are a few things that could be done to make this manuscript easier to read and therefore more impactful, but overall it should be suitable for publication with minor revisions.

We are very pleased with this statement, and we appreciate very much the concise comments and suggestions which we respond to below.

General comments

While I really appreciated this manuscript, I found it hard to read in many places and was distracted by the extensive use of symbology, dense text especially in the methods and results sections, and some figures that were initially confusing. These issues can be easily addressed with minor edits. Anything that could be done to streamline the text in sections 2 and 3, and avoid the use of symbols and acronyms, would make the manuscript text easier to read. See my specific comments below for further details.

One of the reasons why this manuscript is dense and difficult to get through is because of the use of many symbols. Maybe it would be more intuitive and easier to read quickly if these variables were given descriptive names. For example, the authors could simply use the label "vegetation change" or "land cover change" in the main text instead of the symbol ΔX_f. Alternatively, or as well, all the symbols along with their names, units, and a short description could be included in a small table instead of buried in the manuscript text, perhaps as part of section 2.5.

We use symbols to indicate what we precisely mean when using a term like “tendency” or “vegetation change”. We agree with the reviewer that using symbols without explanation makes the text harder to read in some parts. We therefore reduce the use of symbols and we use symbols only with a brief explanation like for example, “… the long-term tendency (ΔX_f) and the absolute offset (O) …”, or “… we exclude time series from the changepoint analysis with a small tendency (i.e., $\Delta X_f < 0.1$) and a small offset at the changepoint (i.e, $O < 0.05$).” And we have included a table **(Table 1) of symbols at the end of Section 2.5 and have modified the relevant expressions throughout the text.**

Furthermore, the A, O, and R values are critical to the presentation of the manuscript. Do these variables have units associated with them or can they be interpreted in terms of physical units? This should be noted.

The values A, O, and R are dimensionless: the offset O at a changepoint is evaluated relative to the maximum vegetation change or, in the case of the marine records, relative to the maximum dust fluxes found in these records. The abruptness A is measured by the offset O relative to the standard deviation of the vegetation fluctuation from the linear regression lines. And the rapidity R is computed as a slope normalized with the long-term tendency. We note this in the revised manuscript more explicitly **(lines 260 and 297).**

Specific comments

Line 80

Following radiocarbon convention of BP time being indexed to 1950 CE, 8000 BP is only approximately 6000 BCE and so the word “approximately” should be added here. 8ka is actually equivalent to 6051 BCE.

Thank you very much for the hint. We clarify this item in the revised manuscript **(lines 85/86)**. The proxy data are all given in calibrated kilo years before present, i.e., relative to 1950 CE. Our simulation starts at 6000 BCE and runs until 1850 CE, i.e. from 7.951 ky BP until 0.1 ky BP, or in the “ka” notation: from 7.951 ka until 0.1 ka, see next comment.

Furthermore, the use of negative values for time (-8 ky and later in the paper -4 ky) is unconventional and unnecessary. The abbreviation “ka” means “years ago” and so it would be easier for readers to immediately grasp your message if throughout the text that convention was followed, e.g., “8 ka” or “4 ka”.

We agree with the reviewer that negative time numbers are unconventional. We hence switch to the common “ka” notation and modify our graphics accordingly **(see changes throughout the text).** We also change the title lines in Fig. A4 from “-8 ky” to “mH” (and in the Caption: mH = mid Holocene), because the maps show multi-year averages, not vegetation patterns of individual years.

Line 96

Although it may not make a big difference for the current study, I find it curious that “a delayed development of bare surface is assumed with a time scale of 50 years”. In ecosystems dominated by herbaceous vegetation, bare surface can develop seasonally, and it certainly takes much less than 50 years for a surface to transition from a vegetated to unvegetated state. Given that the timescale for the end of the Green Sahara is on a few centuries (see discussion around lines 520-530), it would actually make sense that, around the time of the transition to drier conditions, land cover “flickered” between years with herbaceous vegetation cover and years that were mainly bare ground during the transition. The authors should acknowledge the limitations of including this unrealistic boundary

condition in the vegetation model and in the discussion on uncertainties comment on how this limitation could affect the interpretation of the model results and the timescale for the simulated abruptness to the end of the Green Sahara.

This is an important point which we hope to clarify in the revised manuscript. As correctly stated by the reviewer, desertification of a natural site dominated by herbaceous vegetation can occur very rapidly, even on a seasonal time scale. The time delay of desertification was introduced into our land-surface model for all types of landscapes which become unhospitable, including tropical and extra-tropical and herbaceous and woody regions. Implicit in the implementation of a time scale for the development of a bare surface is the assumption that desertification of a large area such as a grid box, which amounts some 40,000 km^2, does not occur all the sudden within a year. Some niches of vegetation might prevail which will fade eventually as the climate becomes more unfavorable for all plants. The time delay is also introduced to avoid annual flickering which would cause numerical problems, specifically regarding the simulated carbon fluxes. We state this in the **revised manuscript (lines 101 – 106).**

Line 187

Please include a scientific name for “Tiger bush”.

Thank you for spotting a somewhat sloppy description. Tiger bush, or brousse tigrée, refers to a patterned vegetation community consisting of trees, shrubs, or grass separated by bare ground or low herb cover, not just shrubs. We skip the reference to Tiger bush **(line 213)** and refer to Appendix B where we explain the dynamics of the shrub PFT in detail.

Figure 2

It would be easier to interpret, particularly if someone extracts this figure, e.g., to include in a presentation, if units were provided for each of the scale bars. There is a lot of information in this figure, and I didn’t immediately notice that the color bar in panel (a) is inverted compared to panels (b) and (c). Furthermore, it’s not immediately apparent what the 0-6 and 1-7 scales represent. I would almost suggest trying to break this figure into three separate figures, to get a little more space and put the common color bars at the bottom and make the key results easier to immediately appreciate.

In an earlier version of our manuscript, Fig. 2 was split into 3 separate figures. The splitting of figures, however, made it difficult to directly compare the patterns seen in these figures. We have therefore put the 9 subfigures into one figure.

We agree with the reviewer that the inverted color bars can be puzzling. **We thus put the color bar for Fig. 2a on the left side of Fig. 2a and all other color bars on the right side of Fig. 2c, f, i. Thereby we save space to put some more explanation onto the color bar, like “decrease … increase” for Fig. 2a, “more … less vegetated” for Fig. 2b,c, “more … less abrupt” for Fig. 2 d-f, and “more … less rapid” for Fig. 2 g-i.**

We have experimented with the scales of the color bars. It turns out that the scales (0-6 for Fig. 2 d-f and 1-7 for Fig. 2 g-I) best visualize the patterns. That is the only reason for this choice. We have mentioned this in the figure caption.

All figures are archived as png, pdf and pptx on an open-access server and can hence be easily extracted.

Line 328-330

It is not surprising that the change in simulated bare surface fraction is not as abrupt as the dust flux reconstructions because most of the dust comes, even at present, from playas that are not explicitly simulated by the model. This fact is noted in the discussion, but it could also be mentioned in the introduction and methods that the model does not include lake and wetland dynamics.

We mention the lack of lake and wetland dynamics also in the **method section (lines 226 – 228) and in the Discussion Section 4.4, lines 660 – 662).**

Lines 383-385

Again, it is not surprising that less change is observed in pollen records compared to dust flux records. This may of course be because non-vegetated surfaces produce no pollen at all and so unless one considered pollen accumulation rate as opposed to taxonomic fractions, one might not expect an increase in bare ground fraction to make a difference in the pollen record that is immediately visible in the pollen record.

We agree with the reviewer that a direct and quantitative comparison between changes in dust flux and pollen records is problematic. Pollen accumulation rates (PARs) can provide indirect evidence for vegetation openness, for example through changes in total pollen influx or tree pollen influx. However, in arid and semi-arid regions, PARs are strongly affected by uncertainties related to pollen preservation, sedimentation, basin-specific processes, and long-distance pollen transport, which may reflect vegetation changes in distant source areas rather than local vegetation dynamics (e.g. Runge et al., 2021, for the African pollen database and other studies). In addition, PARs are not available for all records used in this study. At least in the present study, PARs can therefore not be used to estimate bare-ground cover fractions quantitatively. We hence infer changes in vegetation openness solely from changes in the reconstructed cover fractions of phytogeographical groups. **We add a caveat regarding this interpretation in Section 2.3 (lines 167 - 183) and the Discussion Section 4.4 (lines 650 – 654).** We will also avoid a direct comparison between pollen records from the western Sahel and marine dust-flux records located several hundred kilometers farther north, because the latter mainly reflect dust-source changes in the western Sahara **(related lines skipped in Section 3.2).**

Line 450

It's also not surprising that dust fluxes increase before bare surface fraction since, as noted above, most of the dust is coming from playas, which are not included in the modeling experiments. We might expect the dynamics of shallow playas such as those in the Lake Chad-Bodélé Basin to have been much more sensitive to climate variability than the native grasses, shrubs, and trees of the Sahel and Sahara that are well adapted to seasonal and interannual drought.

Thank you very much for the hint. We also mention this point in the discussion **(lines 660 – 662).**

Figure 7

I found this figure very nice and easy to understand, but as noted above, I would prefer to see standard "ka" notation instead of using negative numbers for time.

We agree and we have changed the text to the standard "ka" notation.

Line 506-508

Finally, it is explained here that lake dynamics are undoubtably of importance for understanding the dust record of the Sahara. This information should be included in the introduction and signposted to the reader that these lake dynamics are not included in the model simulations.

We agree and we mention the lack of lake dynamics in the method section where we discuss the model structure, the dust fluxes and the comparison of data and model **(lines 213 – 215, 660 – 662)**.

Line 545-546

I understand that the red dots were used in the analysis and not the gray ones, but I had to re-read the figure caption several times because I thought these different colors represented terrestrial and marine records, respectively. Consider re-wording for clarity.

We agree that the sentence "The red and grey dots indicate the approximate location of marine records and pollen records, respectively in the model grid" is pretty misleading. **We have reformulated the caption.**

Figure A4

Again, it should be noted that the color scale is inverted for bare surface compared to the other panels. Consider using a different color scheme for these panels to make it more immediately obvious.

When using "Total Vegetation" instead of "Bare Surface", we can put all subfigures on the same color scale, thereby saving space for additional explanation of the color bar.

Appendix B

I really liked this deep-dive into the vegetation model behavior and results.

Thank you very much!

Referenz:

Runge, J., Gosling, W. D., Lézine, A.-M., and Scott, L.: Quaternary Vegetation Dynamics – The African Pollen Database: The African Pollen Database, 1st ed., CRC Press, London, https://doi.org/10.1201/9781003162766, 2021.