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. Therefore, we will reduce the use of symbols and we will 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 will include a table of symbols at the end of Section 2.5.

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 will note this in the revised manuscript more explicitly.

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 will clarify this item in the revised manuscript. 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. Hence, we will switch to the common "ka" notation and modify our graphics accordingly. We will 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 will 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 will state this in the revised manuscript.

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 will skip the reference to Tiger bush and will 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. Therefore, we put the 9 subfigures into one figure.

We agree with the reviewer that the inverted color bars can be puzzling. We will 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 will mention this in the figure caption.

All figures are archived as png 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 will mention the lack of lake and wetland dynamics also in the method section.

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). In addition, PARs are not available for all records used in this study. Therefore, at least in the present study, PARs cannot be used to estimate bare-ground cover fractions quantitatively. We therefore infer changes in vegetation openness solely from changes in the reconstructed cover fractions of phytogeographical groups. We will add a caveat regarding this interpretation to the discussion section. 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

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 will also mention this point in the discussion.

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 will change 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 will 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.

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 will reformulate 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.