Paris, 16/12/2022

Dear Editor,

We thank you for your last review (minor revision) of our article:xxx. We have done our best to address the reviewers questions and requests and updated the manuscript accordingly. We have deposited the processed datasets at the following DOI https://doi.org/10.5281/zenodo.7003853. The raw data are available either in databases or in the literature. A Supplementary file is now included which illustrates our methodology for the 2 categories of data used. We believe that our approach, based on our expertise of more than 40 years regarding hydroclimate and paleoecology in Africa, is honest and realistic. It offers the possibility for readers unfamiliar with paleodata to have a direct and simplified reading of the environmental change of the last millennium. We also want to mention that such an approach was cross-validated by instrumental data and independant published datasets such as the semi-quantitative index developed by Nicholson, which also relies on indirect data (i.e. narratives or archaeological findings) that are discontinuous and widely dispersed geographically and chronologically. We hope that the manuscript is now acceptable for publication.

Yours, Sincerely

Anne-Marie Lézine

Reviewer 1

The abstract presents the second result using unnecessarily strong words: irreversible? Nothing says these changes cannot be undone. Dramatic? At most, the records drop by 2 units of the qualitative scale, 2 out of 6. And they are never reaching 0. These words do not add anything to the study, and it reads as if the authors were trying to oversell their results. This is not necessary and detracting.

The abstract has been modified accordingly.

However, the reviewer can observe that the zero value (bare soil) is reached at Yoa. This value cannot be reached for the hydrological record since the lake or marine series never show complete drying of lakes, wetlands and rivers. Conversely, the maximum value (6) is reached in the tropical forest. The intermediate values are 1: steppe/grassland; 2 wooded grassland; 3: woodland/degraded forest; 4: secondary forest; 5: montane forest (panel F to I). For hydrology and climate, the index shows the evolution from dry (1) to wet (4) with intermediate values showing the gradation between these two extremes (panel A to E)
The legend of the figure 4 has been changed accordingly.

Being not an expert on the regional climate, I would appreciate seeing the convincing explanation about the role of ENSO provided in the responses in the manuscript. Without that background knowledge, the sentence is still floating and not connected to the study. Or is the sentence about the AMV's spatial pattern in the Pacific supposed to make the link? I trust the

authors that everything here is relevant, but I am unsure how. Please make this clearer for uneducated readers.

As discussed in the introduction, it is well known and admited that the West African Monsoon and Sahel rainfall variability during the 20th century was mainly driven by contrasting patterns of sea-surface temperature (SST) anomalies related to both the Atlantic Multidecal Variability and the El Niño Southern Oscillation global teleconnections (Folland et al. 1986; Mohino et al. 2011; Rodríguez-Fonseca et al. 2015), amplified by land surface processes (Giannini et al. 2003; Kucharski et al. 2013). We refer the reviewer to the cited articles in the introduction for more details. It is, in that sense, relevant to discuss the general skills of our climate model in representing these main features to assess the validity of the protocol and confidence we can expect in the robustness of our results.

I am still trying to be convinced by the 6-point scale devised by the authors, but this 'handmade' transformation is bugging me. I would recommend doing this using a mathematical transformation to ensure the correct assignment. Otherwise, I do not see how a proper classification consistency could be reached. Alternatively, I would appreciate seeing an appendix with the summary curve on top of the actual data, similar to the figure provided in the responses, to enable readers to also get a feel for the data supporting these qualitative indices.

The paleodata available are of various types. There are (1) original data (e.g. raw pollen counts) that are available either in the literature or in databases and (2) published data but whose authors do not provide the original counts and whose curves have been re-drawn from published figures. There is no reason to question these published data which have gone through the review process (3) and finally, there are complex published data such as vegetation succession, so that a single curve is not sufficient to encompass the entire local evolution of hydrology and vegetation. This is why it is not possible to use a mathematical transformation as suggested by the reviewer, while the processing and analyses of these 3 groups of datasets collectively by experts can provide a valuable picture of environmental changes in the region thoughout the last millenium. All the indices are avalable at the following link : https://doi.org/10.5281/zenodo.7003853. As requested by the reviewers we also provide the supplementary figure illustrating the method used to deduce the indices from the 3 types of data.

We also want to stress that our methodology follows the procedure developed by Nicholson (Nicholson, S. 1978 Climatic variations in the Sahel and other African regions during the past five centuries. Journal of Arid Environments 1, 1, 3-24). Such semi-quantitative historical data was assessed and cross-validated by Villamayor et al, 2018, relying on the independant reconstruction from Gallego et al, 2015 and with instrumental observations (see figure below from Villamayor, J., Mohino, E., Khodri, M., Mignot, J., & Janicot, S. (2018). Atlantic control of the late nineteenth-century Sahel humid period. *Journal of Climate*, *31*(20), 8225-8240).

Figure from Villamayor et al, *Journal of Climate*, 2018

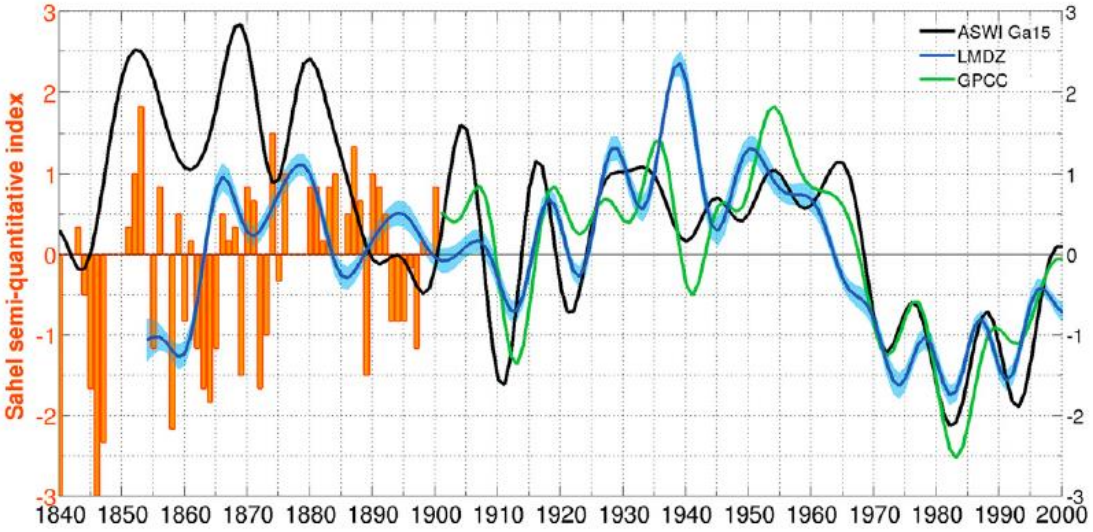

FIG. 4. The bars show the semiquantitative index of Sahel precipitation of the reconstruction of NI12. The lines represent the seasonal ASWI in JAS of GA15 (black) and the observed index of JAS seasonal precipitation in the Sahel (averaged in 17.5°W–10°E, 10°–17.5°N) in observations (green) and in the ensemble-mean simulation (blue). The last three indices have been low-pass filtered with an 8-yr cutoff period and standardized with respect to the observed period (1901–2000). The blue shading is the standard deviation among the 19 members simulated.

We have done our best to answer the reviewer in our previous response. We cannot do better at this stage given the heterogeneity of the data used.

Reviewer 2

Please, when possible, include the references leading to the data used in the example presented in the supplement

Done