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Climate of the Past Discussions

Interactive comment on "A late Holocene pollen and climate record from Lake Yoa, northern Chad" *by* A.-M. Lézine et al.

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Anne-Marie Lezine and co-authors present a thorough analysis and interpretation of a pollen and climate record from Lake Yoa, Northern Chad, covering the last 6000 years.They define three major pollen zones (6000 – 4750 cal BP, 4750 – 2700 cal BP, 2700cal BP – present), and discuss differences in environmental and climate conditions between these zones by comparing palaeobotanic evidence with results from three time-slice climate simulations. Simulations are done with a global atmospheric general circulation model using a stretched grid with highest spatial resolution of roughly 100 to 150 km over North Africa. This paper is a valuable contribution to the discussion of Holocene climate change in the Sahara. It is well written and should definitely be published in Climate of the Past. I have, however, got two major questions – maybe

just a matter of wording - and some minor comments which should be considered.

1) On page 2415, line 26/27 (and in the abstract) the authors write that "The discovery of a groundwater fed lake . . . yields a unique, continuous sedimentary sequence of late Holocene age for the entire Saharan desert". Certainly, the Lake Yoa records provide a unique sedimentary sequence, and it is a unique sequence *in* the Sahara. But is it also a unique sequence *for* the entire Sahara? Perhaps I misinterpret the statement. In that case the authors should state more clearly that the Lake Yoa record is a unique record – unique in the sense that one has not found any records of similar rich information elsewhere in the Sahara. If, however, the authors want to claim that the Lake Yoa record is representative of the dynamics of the entire Sahara region, then they should substantiate their assertion. They could do so in fact by providing the spatial view and interpretation of the simulation results. This point was / is a matter of debate, when Stefan Kröpelin and co-authors (Kröpelin et al., 2008) interpreted the Lake Yoa records as a support of a weak biogeophysical climate-vegetation feedback in the (entire) Sahara. In a reply (Brovkin and Claussen, 2008) we argued that this data would not invalidate earlier hypotheses and modelling results on strong land atmosphere coupling in the Western Sahara for which the Lake Yoa record might be far less representative.

You are absolutely right, this is a translation error. "For" changed by "In". We do not intend to prove here that Lake Yoa is representative of a large region. Our goal is really to provide new information from 1 particular site and to use the climate simulations to show that the variations in pollen assemblages recorded at this site are consistent with large scale climate evolution. We also do not quantify the link between pollen and climate. A throughout analyses of the site and of the hydrological basin is needed for this, because the hydrological basin is complex and a quantification of the different thresholds in water availability from the surface and the ground water is required to do a proper job. In addition the simulations we are using do not have the resolution needed to fully discuss the impact of the surrounding orography (Tibesti) on runoff.

2) The authors conclude (see abstract and conclusion) that “changes in the seasonal distribution of precipitation were at (?) the origin of the retreat of tropical plant communities from the Lake Yoa”. I think this statement is plausible but the authors have not clearly demonstrated the linkage between changes in the seasonal cycle of precipitation and the composition of vegetation. This could be done by providing values of drought tolerance of the different species found in the Lake Yoa records and by comparing these limits with the simulated changes in precipitation. Likewise, the authors might show the results of the BIOME-4 model which was asynchronously coupled with the atmospheric model. Which plant functional types and which shift in types does the BIOME-4 model show for the region around Lake Yoa?

We are not able to provide data on the drought tolerance of plants described in this article, individually. Table 1 in the supplementary material shows the correspondence which can be established between each pollen type (which often includes several species or several types) and West African bioclimatic groups. The following groups have been determined: Sahara, Sahel, Sudan and so on. These groups have been widely described in the literature (e.g., White, 1983). They correspond to well known bioclimates. Referring to Trochain (1980) for example, we can roughly describe the bioclimates corresponding to the Yoa pollen data as follows: - Dry bioclimate with a dry season longer that the wet season : o Semi-arid : 500<P<800 mm ; 7<DS<9 months (Sudano-Sahel) o Arid : 150<P<500 mm ; 9<DS<11 months (Sahelo-Sahara) o Hyper-arid : P<150 mm ; DS > 11months (Sahara) - Humid bioclimats with a dry deason shorter than the wet season (unimodal) o Semi-humid : 800<P<1500 mm ; 5<DS<7 months (Soudanien) o Humid : P= 1200 mm ; 3<DS<5 months In addition, some herbs have life cycles adapted to specific rainfall conditions such as therophytes which have a very short life cycle and are adapted to scarce and irregular rains characteristic of the Saharan desert. Palynological observations at Yoa show a succession of floras whose composition reflects the different bioclimates described above since it distinguishes a rather semi-humid, semi-arid (both tropical) and hyper-arid group of plant types (Figure 8) as well as a Mediterranean one. These observations allow for the characterization

of three main periods corresponding to different conditions of rainfall and seasonality. These are supported by climate simulations that were performed in parallel. We show then, independently, that the seasonal winds (monsoon versus trade wind) and associated precipitation (summer rains versus winter rains), together with the length of dryspells play a fundamental role in the evolution of the Saharan vegetation at the end of the Holocene. Regarding vegetation, the models (not shown in the original manuscript paper) indicate that the bare soils which occupied 20% of the box at 6ka progressively expanded to occupy up to 90% at present. They also show much more varied vegetation at 6ka with notably noticeable C3 plants, which then disappear from the diagram. These C3 plants likely included high altitude plants (Ericaceae) which expanded in the Saharan massifs during the mid-Holocene. We added these precisions in the revised manuscript.

Minor comments: a) p. 2415, l.6-8: Liu et al. (2007) indeed questioned the existence of a strong feedback between vegetation and precipitation. But their conclusion deals with the cause for an abrupt decline in vegetation cover, not the cause for abrupt hydrological changes. In their model simulation they found a noisy, on average gradual transition in precipitation and an abrupt decline in vegetation coverage for the grid cells around Lake Yoa – quite in contrast to the gradual decline in vegetation coverage at Lake Yoa and the abrupt change of the aquatic system as described in terms of salinity of Lake Yoa. Liu argues that the vegetation collapse is not caused by strong feedback with precipitation which would lead to a bifurcation. If there was a bifurcation, then an “unstable collapse” (according to Liu’s et al. 2006, terminology) of vegetation and precipitation would emerge. In a “weak feedback case”, a “stable collapse” would lead to an abrupt decline of vegetation, but a more gradual decline in precipitation.

Our goal was not to analyze eventual feedbacks but to describe and understand changes in vegetation and related (pertinent) climate parameters at the end of the Holocene at Yoa. In practice we are not able from our simulations to isolate the vegetation feedback. They were designed first to discuss the feedback from lakes in the work by Krinner et al. (Submitted). We used then the simulations that are the more complete (including feedback from ocean, vegetation and lakes) compared to what we know from the real word to only discuss 3 time slices. In the simulation changes in climate between the 6ka and 4ka simulations are larger than changes in vegetation, may be because the 6ka lakes were imposed at 4ka, and also because the model only consider a limited number of pfts, and not the whole variety of vegetation types recorded at Yoa. Transient simulations would be required to go one step further on this question. However what is interesting here is to show that in the simulations, 4ka appears as a transition climate, from a period where monsoon rain brings most of the moisture supply during summer and present day, where northerly winds are predominant, and that this has implication on wind directions, the characteristics of convective events and of dry spells.

b) p. 2415, l. 9: DeMenocal et al. (2000) were not the first to suggest an abrupt termination of the African Humid Period. It was actually a prediction based on theoretical considerations by Brovkin et al. (1998) and Claussen et al. (1999). This prediction was thought to be “validated” by deMenocal et al. (2000) – see Liu et al. (2007).

These references will be added.

c) p.2423, l. 3: BIOME-4 is coupled asynchronously with the Atmospheric GCM. How was this done? How many iterations were necessary to achieve an equilibrium? Has an equilibrium really been achieved?

The simulations are described in Krinner et al. submitted. This was done following an anomaly mode as in Texier et al. 1997. There were two iterations of asynchronous coupling, that is, 2x(42 years GCM + Biome 4) + 1*42 years GCM. Precipitation rates were very similar between the second and third iteration, indicating that equilibrium was achieved.

d) p.2423, l. 19/20: “SST boundary conditions come from the IPSL climate model” – what does this mean? Has the IPSL climate model provided transient SSTs, or

snapshot SSTs?

The IPSL model provides a > 200 year climatology computed from the last part of the > 700 year long simulations of the different climates. The differences with the PI climate are superimposed on modern climatology.

e) p.2423, l. 25: The authors consider a mean climatology of 41 years. Hence there was no time left for the atmospheric GCM to adapt to new SSTs? Why did the authors not consider the commonly used 30 year climatology and a spin-up time of 11 years? Do the 41 years include the asynchronous coupling?

The asynchronous coupling is between the atmospheric GCM and BIOME4. There is no asynchronous coupling with an oceanic model, so there is no need to adapt to new SSTs. The 41 years do not include the asynchronous coupling. Each simulation with the zoomed model is 42 years long (the first year being discarded as spinup) and we present only the climatology from the last iteration. Because the GCM-only runs are only atmospheric runs, a one-year spinup is considered sufficient, particularly in the last GCM iteration. Indeed, we could have used 30 years periods, but there is no reason to discard 11 more years. 30 (or sometimes 20) years is considered a minimum length for AGCM runs to establish reliable climate statistics, but there is no maximum length.

f) p. 2424, l. 10: I would not consider a peak in June to occur “slightly earlier”, if data indicates a peak in August. With a monsoon period of 5 months, 2 months really do matter. It would be helpful to see the observation plotted in Fig. 4 for comparison with model simulations.

You are right the vocabulary is not the right one. It is a “maximum” rather than a “peak”.

g) Why is Fig. 7 discussed before Fig.6? I suggest to swap figures.

The order of the figures and their recall in the text has been carefully checked.

h) p.2426, l. 7: Unfortunately, Fig. 3 is very hard to read. Hence, I cannot verify the statement of a progressive retreat of tropical plant communities when looking at Fig. 3. When reading the figures in Lézine et al. (2009), in particular their Fig. 5, I can find an abrupt decline in the "sum of tropical pollen types" while other pollen series, Poaceae, for example, change gradually.

No, we cannot speak about an abrupt decline when tropical plants represent only less than 5% of the total pollen sum. From 6ka to 0, we do not see any abrupt change in vegetation. It is probable that the decline of the tropical plants community at Yoa started earlier. We have here only the end of the process.

i) p. 2425 – 2427: It is not obvious to me what the indentation should discriminate.

This has been corrected

j) Fig. 6: A vertical velocity w > 0 should indicate subsidence? Or what else does w500 mean? I guess it is the vertical velocity in the p-system where vertical motion of isobaric surfaces in the direction of increasing pressure is considered positive. This needs to be clarified.

k) All figures are hard to read, because they are rather small.

They will be enlarged

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