

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 Lézine 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, 2700 cal 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.

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?

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.

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).

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?

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?

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?

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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.

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

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.

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

j) Fig. 6: A vertical velocity $w > 0$ should indicate subsidence? Or what else does w_{500} 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.

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