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Climate of the Past Discussions

Interactive comment on "Past environmental and climatic changes during the last 7200 cal yrs BP in Adamawa Plateau (Northern-Cameroun) based on fossil diatoms and sedimentary ^{13}C isotopic records from Lake Mbalang" *by* V. F. Nguetsop et al.

Anonymous Referee #2

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This paper presents a new record of Holocene climate variability from Cameroon that documents considerable variations in moisture balance associated with the collapse of the African Humid Period. There is considerable uncertainty in the timing and nature of this transition across north Africa, and the authors' new well-dated sediment core provides exciting new information on the region's climate history. The authors present convincing evidence for a transition from wet conditions and a well-stratified lake during the early Holocene to dry, poorly stratified conditions in the late Holocene (Zone 1 vs.

2). However, many of the interpretations of the diatom subzones, particularly zones IA-C, are inconsistent and not strongly supported by the data. I suggest substantial revision of these.

The authors use the high abundances of Aulacoseira distans varieties to assert that the water column of Lake Mbalang was 'cold and stable' during Phase I of the lake's history (~6400-3400 yr BP). This statement should be removed. Gasse (1986, cited in the authors' text) A. distans var. africana prefers 'rather warm conditions', and that A. distans var. humilis shares the same preferences. Moreover, a cold water column is almost always an unstable water column in tropical African lakes; it is unclear if it is possible to develop cold, stable conditions. In general, I do not think the authors' data can be used to interpret past temperature changes, and suggest that all references to cold/warm conditions be deleted. Some changes in stratification do appear supported by variations in A. muzzanensis vs. A. distans, although care must be taken here as these taxa also exhibit pH and nutrient preferences.

Subzone Ia (p 315). There are several contradictory or incorrect interpretations presented here. Sentence 1 states that tychoplanktonic abundances are high indicating acidic, oligotrophic, and cold stratified conditions. There are plenty of tychoplanktonic taxa in eutrophic, alkaline, warm lakes- tychoplanktonic abundance alone does not imply what the authors suggest. Later it is suggested that the presence of A. muzzanesis implies episodic mixing, in contrast with the interpretation of stratified conditions. The explanation for the carbon isotopic changes is odd. The authors suggest that the depleted d13C values suggest closed canopy forest, although they present no data to suggest that the organic matter is, in fact, derived from aquatic plants. They then suggest that "phytoplankton with a CO2-based metabolism can also be suggested for the depleted d13C especially for some observed d13C peaks that are coincident to the increase of eutrophic pH-indifferent taxa covarying with positive d13C excursions that might reflect the presence or the vicinity of the aquatic vegetation." First, given the generally acidic conditions implied above, most if not all algae in this interval should have a CO2-based metabolism- the more likely mechanism is oscillations in trophic conditions. But more importantly, the d13C data were generated at nearly double the resolution of the diatom data and it is unclear, in figures 5 and 7, whether there is any consistent relationship between variations in d13C and variations in the diatom taxa. To my eye, the two do not appear to be correlated (the authors could try resampling their d13C data plotting only the samples on which diatom measurements were investigated to check this). It should be noted that the sediments in this zone are comprised of relatively coarse materials, and that the abundance of benthic and epiphytic taxa is high (F. capucina, S. phoenicenteron, P. viridiformis). All of this would suggest relatively low water levels. Data from neaby Lake Bambili also suggest dry conditions from 10-7 kyr BP. The vegetation data from Lake Mbalang suggest, however, that this interval was quite wet. The authors should discuss mechanisms to explain these differences.

Subzone Ib. The key features here are the rise in A. muzzanensis and several peaks (enrichments) in d13C. It is stated: "We suggest that during this time, episodes of wind stress and high temperatures were longer than before, consequently lake level was relatively low at least episodically, but benthic and epiphytic taxa could not developed due to mixed, turbid water column. The high lake level can be explained by high and probably well distributed rainfall over the year that allowed the maintenance of forest vegetation as shown by d13C data." Is the lake level high or low?- both are stated. And how do a series of 4-5 per mil enrichments in d13C argue for the maintenance of forest vegetation?

Subzone Ic. The most significant change is the appearance of the 'windblown' taxa. But how can the windblown taxa be distinguished- is there any evidence to suggest that these taxa were not produced in situ? An influx of windblown diatoms from Saharan paleolakes certainly seems feasible at this time, but should be accompanied by an increase in the abundance of windblown dust and Saharan pollen. Is this also observed in these sediments? Might there also be trace amounts of saline diatom taxa? Is it not also possible that the increase in these taxa represent a shift toward slightly higher

ionic strengths and drier conditions in Lake Mbalang?

If these diatoms (A. granulata, S. astrea) can be demonstrated to be windblown, I strongly suggest that they be excluded from the diatom percentage calculations and sum. This would allow readers to evaluate that climatic/limnological changes occurring only in Lake Mbalang.

Subzone IIb. The development of F. delicatissima is represented by a single sample, and should not be overinterpreted. The fact that other groups decline is not strong evidence, as the authors present on percentages rather than diatom concentrations.

Subzone IIc. Note that the windblown taxa also decrease substantially, which supports the inference for a better stratified water column indicated by F. delicatissima.

Discussion section. The authors need to rethink their discussion of the climate conditions that lead to lake stability/mixing. On page 319 it is stated that stratified conditions occur during low wind stress, surface warming and cool epilimnetic conditions (?), which typify conditions during the northern hemisphere summer/strong monsoon. Kling's PhD thesis is cited for this information. In fact, Kling (1987, Science v. 237 pp 1022-1024) shows conclusively that lakes in central Cameroun achieve minimum water column stability and mix during the monsoon season (August), when heavy cloud cover reduces solar radition inputs and heavy rains directly cool the surface. Kling (1988, L&O v. 33 pp. 27-40) further showed that the stability of the water column of Cameroonian lakes, including Mbalang, does not vary strongly due to wind forcing as these crater lakes are topographically sheltered from the wind and have high volume/surface area ratios. It is therefore incorrect to infer that reduced mixing is equivalent to "conditions close to boreal summer" based on modern conditions- the lakes are losing heat and becoming more poorly stratified throughout the boreal summer in the modern. In fact, in the modern regime the amount of rainfall and lake stability are antiphased. The authors show that the opposite holds over the seasonal cycle- the question is how to achieve this. It seems likely that either evaporative heat loss was suppressed during the early Holocene, or that summer insolation inputs to the lakes increased despite heavier rainfall (higher cloud cover). In any case, a bit more though about the lakes' heat budget and mixing regime is needed.

The termination of the African humid period (p. 320, lines 1-6) is generally considered to be ∼5.5 ka (see various Gasse and deMenocal publications). This is much later than implied here (7.2 kyr BP). In fact, it is not clear to my why the authors do not think that the zone 1-2 transition in their core is not simply a lagged response to insolation forcing and part of the termination of the AHP (albeit considerably later than in North Africa/Sahara).

There are an enormous number of typos, grammatical changes, and problems with the figures that will be have to be fixed before this paper is publishable. For example, in figure 5 the panels are mislabeled (5A, b, d, f). The typos and grammatical changes in the text are simply too numerous to recount here. In some figures uncalibrated 14C ages are presented (Figs 2, 4, 5), in others calibrated ages (Figures 3, 7). This makes it extremely difficult to compare results between figures. The authors should simply plot everything against the calibrated ages defined by their age model.

Radiocarbon age model, p. 312 line 9. The age at 102 cm depth is older than expected, not younger.

The % TOC and C/N data are not provided in the figures. The latter would be particularly useful as an indicator of organic matter source, ss much of the discussion centers on the interpretation that d13C can be interpreted as an indicator of vegetation type in the lake's catchment.

Interactive comment on Clim. Past Discuss., 7, 305, 2011.