**Climate
of the Past
Discussions**

Interactive comment on "Pollen, biomes, forest successions and climate at Lake Barombi Mbo (Cameroon) during the last ca. 33 000 cal yr BP – a numerical approach" *by* J. Lebamba et al.

H. Hooghiemstra (Referee)

hooghiemstra@science.uva.nl

Received and published: 15 February 2011

The 33 ka long pollen record of Lake Barombi Mbo belongs to the most informative pollen records of Central African rainforest area. Environmental change was qualitatively inferred from the pollen record by Maley & Brenac (1998) and the present paper tries to improve on this reconstruction by analysing the data set quantitatively. Indeed, there is a global need to develop ways to reconstruct past climate change more quantitatively in order to allow comparisons with climate reconstructions based in physical proxies. These authors used the biomisation method downcore, calibration by modern analogues, and the arteficial neural networks method.

General comments:

Although the aim of this paper is "to provide a more complete and precise intrepretation of the polen sequence" it is difficult to evaluate to which degree these authors were successful. It would be helpful to show in consice form the vegetation dynamics in a 'main pollen diagram' in order to allow the reader to make comparisons. But perhaps here is a first problem as the 8 environmental categories in Maley & Brenac (1998) differ from the 7 environmental categories in the present paper. The authors should explain why this difference has been introduced.

The title speaks about "forest successions". To my knowledge 'succession' means the sequence of vegetation types independant from external drivers, such as climate change. Examples: (a) An open lake may finally result in a 100% cover of peat land: this is internal (basin) succession and may happen under a variety of climatological conditions; (b) after a huge rainforest tree has fallen, the open gap in the canopy is filled up by successional stages independant from climatic conditions. Internal "succession" is in contrast to external driven "vegetation change"; I guess the authors discuss in this paper 'vegetation change".

The Result section 4.1 is a description of affinity scores for 6 periods and leads the reader through figures 4 and 5. It would be helpful to show these periods also in figures 4 and 5. Readers will classify this part of the paper as little informative while the authors will consider this section as obligatory as is a 'description of changing percentages in a pollen diagram'. It would make sense to consider to provide this information in a concise table which even might be given in the supplemetary information. I am not able to assess the quality of figure 6 and the descriptive text to figure 6 in section 4.2. For this important part of the paper a reviewer with different expertise is needed.

I would re-name the 'Discussion' section into 'Quantitative environmental reconstruction' section, and separte the interpretation from the real discusion. This reorganisation of the text makes more clear what has been inferred from the Lake

Barombi Mbo record, and what comes from elsewhere. Here, I am missing the original pollen record. Most palynologists are trained in analysing a pollen percentage diagram and it would be very helpful to show at this very place what the new methodologies have added to the original interpretations (the first aim of this paper).

The 'Conclusions' section includes text which is a repetition of the previous reconstructions of environmental change. It should be sharper, shorter, and more to the point.

In summary, this paper is a welcome attempt to provide pollen records with a quantitative interpretation. The authors have not used this opportunity to compare the quantitatively re-interpretated pollen record to a physical based proxy record from a sediment core in the equatorial Atlantic to show that the present exercise indeed helps to make new interdiciplinary connections. Perhaps this is too much requested for the present paper but it is a logical follow-up step. It would make sense to address this issue in the conclusions.

Specific comments:

Abstract:

The abstract needs much re-formulation: do not start the abstract with the aim of this paper; 'more complete and precise' than what ? ; there is no need to introduce acronyms in the abstract; a "mature" forest, and a forest "of secondary character" is unclear terminology; the text can be sharper and more informative.

Introduction:

I do not like such irrealistic objectives as "prediction the evolution of the important natural forest ecosystem". I have not seen any predictive conclusion in this paper.

If there are 'numerous quntitative reconstructions' (page 2706, line 2) what does the present paper add ? Here, the development (I guess there is) in methodologies should be explained. Or is the novely only geographical (. . for the first time in central Africa . .) ?

Environmental setting and data sources:

The chosen acronyms in this paper are quite confusing and I wonder if the authors follow the international literature. For example Table 2 is difficult to understand and would benifit a lot when acronyms are left out !

Pann : Mean Annual Precipitations (why not MAP ?)

PETann : Potential EvapoTranspiration =?? Mean annual Potential EvapoTranspiration? (why not MA.PET or something like ?)

Why is Mean Annual Temperature not reconstructed as it may allow comparisons with marine $\partial 18O$ records ? I am mising a justification of the 3 variables (Paan, PETann, alpha) chosen in this paper.

Methods:

Although methodology has been explained elsewhere, it would be helpful to repeat here the essentials to avoid a black box. Biomisation method, MAT, but ANN in particular should be better explained.

MAT ; Modern Analogues Techniques. However, MAT frequently means Mean Annual Temperature! This is confusing. Is an acronym to refer to this method really necessary?

Once an acronym have been introduced (to reduce text) it should be used consequently. In this paper the authors write text in full up to the Conclusions section while acronyms have been introduced in the beginning of the paper. Please correct all over the paper.

Results:

Do not start with "These reconstructions . . " Which reconstructions ?

on Figs. 4 and 5 = in Figs. 4 and 5

It is unclear to me how the eight 1998-vegetation categories should be compared to

the six present categories.

1998-paper:

1. pioneer forst

2. semi-deciduous forest

3. Biafrean and Evergreen forest

4. Montane Forest

5. Savanna Tree

6. herbs in Forest

7. hrbs in open formations

8. quatics and hygrophilous herbs

this paper:

1. tropical Rain Forest TRFO

2. tropical Seasonal forest TSFO

3. savanna SAVA (The SAVA record in Figure 4 differs much from the SAVA record in Figure 5 without explanation)

4. tropical Mature Forest TMFO (what does this category mean ? needs adequate explanation)

5. tropical secondary forest TSFE (is this a natural category ?, or reflecting human disturbance ? How long is the human occupation history in Cameroon ?)

6. tropical Forest regrowth TFRE (what is the difference with secondary forest ?)

Discussion (better: Quantitatine Environmental Reconstruction)

Difficult to understand as categories are unclear to me (see above).

How does the present reconstruction support the 1998-reconstruction ? or differ from it ?

In this section quantitative environmental reconstructions are mixed up with disscussions in relation to Anhuf et al. (2006) etc etc. Please separate reconstructions and discussions.

The text of this chapter is dificult to follow without graphical support of some figure.

Is a vegetation change between ca. 3000 and ca. 1200 cal yr BP really "abrupt" ? (2791: 15)

Conclusions:

What is the meaning of the first sentence ? What does it complement ?

2721: 21 Does "mature" mean "undisturbed" / "natural" ? Better to say so.

Heinrich events were not introduced before, neither discussed in the environmental reconstruction section. This new issue cannot be introduced in the Conclusions section.

The text is wordy and lacks clarity: needs re-formulation

2722: 26 and 27: '18% of the samples' cannot be compared with '10 samples'

References:

There are many mistakes in the (abbreviated) journal names.

Technical corrections:

2705: 28: precipittions = precipittion (at many places in the ms)

2707: 3; during summer northern hemisphere = during northern hemisphere summer

2707: 19: western african = western African

Table 1 in fact does not show a chronology (that is Fig. 3) but shows 'Radiocarbon ages and sample specific information'. Omit 0.5 years which makes no sense.

Fig. 3 shows text upside down

2909: 13: This methods was developed . . . Which method ? the text should be clear without reading the caption.

"rainfall" and precipitation' are mixed up while there is no difference indicated

Finally, the final text should be corrected by a native English speaker; the present level in not adequate.

In conclusion, this paper makes an interesting step forwards to arrive at quantitative reconstructions from pollen records. This issue fits the scope and serves the audience of CoP. Clarity can be improved all over the paper. I advice major revision and re-review.

Henry Hooghiemstra (15 Feb 2011)

Interactive comment on Clim. Past Discuss., 6, 2703, 2010.