

Interactive comment on “Western equatorial African forest-savanna mosaics: a legacy of late Holocene climatic change?” by A. Ngomanda et al.

Anonymous Referee #1

Received and published: 3 March 2009

General Comments

The paper presents two very interesting pollen records documenting the late Holocene history of coastal and inland forest and savannas of western Gabon. The research is a valuable contribution to the discussion on the origin of West African savannas and the impact of past climate change and human activities on forest distribution. The interpretation of pollen records is based on exceptional knowledge of regional botany and plant ecology.

The results of this paper clearly merit publication. However, there are a number of conclusions and statements in this manuscript which appear to be inconsistent and need a major revision. I also recommend a more careful interpretation of the Lake

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Maridor pollen record, which is, according to the inverse radiocarbon dates, heavily disturbed and therefore does not allow a strict use of any age models. The manuscript would also greatly benefit from editing by a native speaker. Some paragraphs are difficult to understand due to language problems and there are several grammar errors.

Specific Comments

Chapter 1 Abstract: Discrepancies in the abstract need clarification:

The authors are stating a rapid decline of hygrophilous evergreen rainforest around 4000 cal yr BP (line 6) at Lake Nguene in the abstract. The same change is described later in the manuscript as a slight decline and a progressive replacement (2nd paragraph page 348).

I also have problems understanding why the authors write that the marked (where?) reduction in rainforest is associated with the colonisation of secondary forest dominated by *Elaeis guineensis* (line 11 in abstract). According to the pollen record from Lake Nguene, oil palm percentages increase at 2800 cal. yr BP, more than 1000 years after the rainforest decline.

It would also be helpful and avoid confusion, if the authors clearly mentioned at the beginning, where (inland-coastal) the two coring sites (Nguene, Maridor) are located.

Chapter 4. Age Model s and sedimentation rates I do not understand what is meant by this sentence (page 346, line 13-16): These inversed radiocarbon dates however confirm the neighbour date, 1630±40 yrBP (at 105 cm) and 4430±40 yrBP (at 375 cm) being close to 1600±40 yrBP and 4110±40 yr BP, respectively. Please explain.

In this chapter radiocarbon dates are indicated as yrBP, cal yr BP and 14C yr BP. Please be consistent.

I am confused by the use of the term and interpretation of podzolic soil in the 2nd paragraph on page 347 and in the discussion. The authors describe several soil horizons in core profile MAR2, but assume that the lake never desiccated. However, later in the

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discussion (page 352, line 3) the authors note that a lithological shift from podzolic soil to sand-rich clays, suggests the Maridor became a permanent lake. The reconstruction of the Lake Maridor lake history is confusing and needs revision (see also comments on discussion below).

I have problems to understand how the age model for MAR2 has been developed. The radiocarbon dates (and stratigraphy) indicate heavily reworked sediments. Linear interpolation between these dates does not appear to be useful. It is particularly difficult to understand the following sentence (page 347, line 13-15: The first chronology in dotted line is not consistent with lithology and sand content because it suggests a constant sedimentation rate between 4100 until 3460 14C yrBP. This does not make much sense, as a linear interpolation between two data points always results in a straight line suggesting constant sedimentation rates.

Regarding the rather weak age model, I do not think that it is possible to give any age estimate for a 20cm hiatus in coarse sandy sediment at all (Fig 1 and line 8, page 347).

I do not understand this last sentence of the chapter: The Oother AMS date should be necessary to affine a robust chronology. Please explain.

Chapter 5. Nguene pollen record On page 348, line 15 the authors describe major changes in zone N2 (ca. 3200-1400 cal yrBP). However, in contrast to the slight decrease at 4000 calyr BP, these changes are mentioned only very briefly. Why? What is the implication for regional climate change ? (see also comments below)

Page 348, line 19: use pioneering taxa instead of shrubs as *Elaeis guineensis* is not a shrub.

Chapter 7 Discussion

The reconstruction of lake levels of Lake Maridor (Page 354, last paragraph) is very difficult to understand. Please reword. It may also be helpful to add a figure with a sketch illustrating major steps in lake history.

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According to the authors interpretation, there are several concise changes in the size and depth of Lake Maridor. Changes in the catchement area significantly alter the pollen influx, including the relative percentages of grass and tree pollen, which need to be discussed in this chapter.

I generally agree with the interpretation that the rise in oil palm pollen was caused by climate change rather than human impact. However, the discussion ends with a weak argument: From the simple absence of archaeological data, it can hardly be concluded that the increase was not of anthropogenic origin. Please rephrase this statement.

In both diagrams, oil palm pollen percentages increase abruptly after ca. 2800 cal. yrs. If this is climatically triggered, how is it related to the major change at ca. 4000 BP discussed earlier in this chapter. Why is there a delay of more than 1000 years between the climatically induced opening of forests and colonisation with pioneer taxa? Please discuss in regard to gradual vs. abrupt climate changes.

Technical Corrections

Page 344. Line 8 : etc..) Page 344, line22, delete both Page 351 line 5, scarce and not scare Figs.: Increase font size of age and depth scale

Interactive comment on Clim. Past Discuss., 5, 341, 2009.

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