**Climate
of the Past
Discussions**

Interactive comment on "Simulated effects of a seasonal precipitation change on the vegetation in tropical Africa" *by* C. Cassignat et al.

D. Verschuren (Referee)

dirk.verschuren@UGent.be

This paper uses sensitivity experiments with a process-based biome distribution model to make the point that past changes in the seasonality of rainfall may have played an equally important, if not more important, role than changes in the total annual amount of rainfall to create the Holocene vegetation changes in tropical Africa reconstructed by pollen analysis. This is an important point to make, because failure to distinguish between these two rainfall variables hampers insight into the climate-dynamical processes that created those vegetation changes. As a paleoclimatologist working with lake-based moisture-balance indicators, I can attest that the same problem also haunts the interpretation of hydrological records from African lakes. Understanding to what extent i) the hydrology of a particular lake system is more or less sensitive to seasonal

variation in rainfall and evaporation, and ii) a moisture-balance proxy reflects rainfall seasonality or total annual rainfall can often resolve apparent conflicts between the hydrological records of adjacent sites experiencing the same climatic regime, or between different hydrological proxies extracted from the same site.

That said, whether this paper is a significant contribution to the resolution of this problem depends on the soundness of the work performed, the clarity of the arguments presented, and on whether the results are framed in a balanced context of the current state of the science. On the first count, the set-up of the simulation experiments in this study appears basically OK, except for the CO2 issue in modeling the altitude effect at the Burundi site, as mentioned by Dr. Prentice. Further I note that in their discussion of the conducted experiments (p.866) the authors suggest improvements to the calculation methods which could potentially produce more realistic, or more robust, results. If the authors are seriously considering this potentiality I recommend that they make these methodological adjustments and report on how it affects the outcome of the experiments in this paper. If this cannot be done for practical reasons (e.g., it would require a complete rerun of time-consuming calculations) I recommend to not discuss these adjustments in this paper but save them for the introduction of a future paper. Science advances in increments, and at issue here is whether the increment realized by this work is significant; citing unpublished data or calculations ('Gritti et al. unpublished') without proper clarification is pointless, it only undermines the significance of the present study.

As to the clarity of the arguments presented, I feel that the structure and fluency of the text needs to be improved to make the argument easier to follow by interested but non-specialist readers, so as to eventually enhance the impact of this paper. Many errors are made against proper sentence structure, as is evident from the text amendments done by Dr. Bartlein. The authors must do more effort to formulate their thoughts properly and consistently, so the reader doesn't get confused. Identifying the same entity variously as 'number of dry days', 'dry-season length', dry-season parameter' or

'driest-season intensity' does not help when you are trying to convey a message. A few examples on p.863: "the range of the driest-season parameter is rather narrow when the semi-deciduous biome dominates" (lines 22-24) should be "the range of consecutive dry days within (or 'across') which the semi-deciduous biome dominates is rather narrow"; "The range where the modern biome is potentially present increases from days 100 to 140 to days 40 to 120" (lines 26-27) will read better as "The number of consecutive dry days within which the present-day biome at Kuruyange occurs increases from about 40 (between 100 and 140 days) to 80 (between 40 and 120 days). Consider also "Moreover, all the transitions shift to lower critical values" (lines 27-28). There are in fact only two transitions; these are transitions between what, and critical values of what? Not many extra words are needed to improve clarity: try "Moreover, both biome transitions occur at lower thresholds of dry-season length". I strongly recommend that the authors carefully re-read their paper putting themselves in the position of the reader who needs to understand exactly what the authors want him to understand. Also the figures can be improved in this respect. In Figs. 3-5, I suggest to explain the color and symbol codes with legends directly in the plots; in Figs.5-6, also site names can be added to the plots, as in Fig. 3.

Finally, as concerns the scientific significance of this study, the authors claim its most important result to be that "the simulated vegetation change due to [only changing] the seasonal precipitation is more important than the changes observed in the paleodata during the Holocene at the three sites" (p.865 line 13-15). Here the pertinent question is whether the seasonal precipitation changes forced in these experiments are realistic on a Holocene time scale for the sites under consideration. At present, Lake Victoria (Fig. 2b) does not have a dry season under the criteria used (<30 mm rain/month). How likely is it that this location experienced more than 100 (up to 220 in Fig.5b) consecutive dry days at any time during the Holocene ? If not very likely, can more realistic changes in rainfall seasonality still rival changes in total rainfall as the principal driver of Holocene vegetation change ? In the bimodal rainfall regime experienced by the three sites (in theory; this is not immediately evident from the climate plots for Lake

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Victoria and Kunuyanga in Fig.2), if orbital-scale ITCZ shifts and changing monsoon dynamics resulted in the lengthening of one dry season, would then also the timing of the rain seasons have shifted such that the other dry season became shorter, and the total annual number of dry-season days more or less the same ? Or do the authors envision that lengthening of a dry season implied the (partial) failure of a rain season ? Some discussion of this climatic context in the Introduction would strengthen the foundation of the experiments being performed. Also belonging in the Introduction is the authors' statement about the reason why palynologists tend to assume vegetation change to reflect annual rainfall changes (now in the Conclusion, p.868 lines 7-12). The paper needs to give due credit to the palynologists working in Africa who have moved beyond this paradigm, e.g. Vincens et al. (2007, J. Biogeogr.), Garcin et al. (2006, QSR) and Ngomanda et al. (2009, QR). About rainfall seasonality I cite Vincens et al. (2007): "This climatic parameter is as important as the total annual amount of rainfall, and probably one of the most relevant in lowland areas".

Finally a few suggestions for relatively minor corrections: 1) The Conclusion's last sentence, referring to reconstruction of Mediterranean and Eurasian climate, clearly does not belong in this paper and can be deleted. 2) What is the ref. for the statement about C3 cultural plants on p.864 (line 22-24)? 3) That "simulations for 12 ky BP show an increase in July precipitation across Africa between 30°S and 30°N (p.855 line 26-28)" is clearly not matched by the reconstructions: the Atlantic monsoon penetrated to ~21° N in North Africa (Hoelzmann et al. 2004 in PEPIII book), the Indian monsoon penetrated to ~23°N in Arabia (Fleitmann et al. 2007 QSR), and an anti-phased pattern of early Holocene drought developed in southern Africa south of ~10° S (e.g., Castañeda et al. 2007 Geology; Nash et al. 2006 QSR).

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