

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

C. Cassignat et al.

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“Simulated effects of a seasonal precipitation change on the vegetation in tropical Africa”

Dear Dr Rousseau,

First and foremost, we would like to thank all the referees for their serious and accurate comments on the first version of this paper. We tried to answer each of their concerns in the current version and will go through in detail for each of them. We would like to apologise for the long time spent to answer comments and we hope you will receive positively these answers.

Response to referee C. Prentice

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1. As the MS clearly sets the analysis in a particular context of vegetation change, it is important to add some comment – even if speculative – about the likely nature of the climate change under discussion. Otherwise the reader comes away dissatisfied. It seems that the climate change in question might not reflect an annual precipitation reduction, and that it might instead reflect an increase in the length of the dry season after 6 ka. But this is not said explicitly. The authors should add some words indicating what, in their view, is the most plausible explanation for the observed vegetation changes after 6 ka. Words on this topic should appear in the Abstract as well as in the main text.

We think that the likely nature of the climate change in question is explicitly exposed in the abstract and in through the text (reduction of the annual amount of precipitation and/or modification of the seasonality). However, we think that we can not indicate what is the most plausible explanation for the vegetation change at this point of the study. Both explanation are realistic and are not excluding each other. The use of Global Circulation Model for the Holocene period over the considered area should be a more accurate strategy to explore the possible development of the precipitation parameter.

2. The MS gives the impression that all previous work has assumed annual precipitation to be the major control on vegetation type in the tropics. But at least in South America, the ecological literature already emphasizes length of dry season as a major control. This should be acknowledged, and relevant citations added.

We agree and we acknowledged and added relevant citations in the first paragraph of the introduction.

3. The standard diagnostic tool for palaeoclimate modelling is now BIOME4 (see e.g. the PMIP2 website). BIOME4 has been available for about ten years, so the reader needs to be informed as to how BIOME3.5 relates to BIOME4, as well as BIOME3.

The first simulations of this study have been performed years ago, at a time where Biome 4 wasn't available. However, we think that the relations between Biome 3, 3.5

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and 4 are described in the section 2.1 BIOME3.5

4. The way in which elevation is treated is wrong, and must be removed from the MS. The error made is that CO₂ partial pressure is varied, while O₂ partial pressure is not. In reality, the partial pressure of O₂ declines as the same rate as the partial pressure of CO₂. So as O₂ competes with CO₂ for the Rubisco reaction sites, the decline in O₂ offsets the decline in CO₂, such that the net effect of elevation on photosynthesis is small. (I suspect that it is so small that other effects such as the increase in clear-sky transmittivity would be more important, although this has not been tested.)

We agree with this comment that the decrease of the partial pressure is linked to a decrease of the partial pressure of O₂. However, the assertion that CO₂ partial pressure is varied while O₂ partial pressure is not, is incorrect. The parameter modified in the simulations is the atmospheric pressure which is affecting, in the BIOME model, the partial pressure of CO₂ and O₂ simultaneously. Moreover, taking the reduction of the atmospheric pressure due to altitudinal elevation, yields to more realistic simulations of the observed vegetation composition at the site.

5. Recent research has highlighted the role of fire in controlling the distribution of trees versus grasses in the tropics. BIOME3 and its successors do not explicitly model fire; they consider it implicitly by allowing grasses to outcompete trees in dry environments. This should be mentioned, and relevant references cited. In addition, the MS would benefit if the Discussion were made shorter, concentrating on a smaller number of major points and eliminating side-issues. I leave the choice of issues to the discretion of the authors. However it seems to me, for example, that the case for using a dynamic model instead of an equilibrium model is weak. (A stronger case for using a dynamic model might be that it makes it possible to model vegetation-fire interactions in an explicit, process-based way.)

We agree and we modified the discussion according to this comment.

P. Bartlein (Referee)

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The paper does not provide actual reconstructions, but instead is focused on the plausibility of variations in the seasonal distribution of precipitation as an explanation for the observations. There are some mechanical issues in the current version of the manuscript, and I agree with Colin Prentice that the CO₂-variation experiments are flawed. Other than that, I think the paper is scientifically sounds, and provides a useful contribution. Please also note the Supplement to this comment.

We agree with the comments and modification of Pr. Bartlein (except the one relative to the simulation of the altitudinal effect on CO₂ partial pressure). We modified the text according to these comments and modifications.

D. Verschuren

On the first count, the set-up of the simulation experiments in this study appears basically OK, except for the CO₂ issue in modelling 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.

We agree with the comments of Dr Verschuren and left aside these issues relative to possible improvement 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-

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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.

We are conscious that the papers needed to be made clearer and we corrected the sentences that were not accurate or understandable.

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.

We modified the figures according to this suggestion.

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

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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 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.

We agree but, as previously mentioned, the use of a Global Circulation Model is certainly a far better strategy to answer the duality between reduction of the annual amount of precipitation or change in the seasonality distribution of this amount. In the same way, this study does not allow us to estimate what could have been the "realistic" length of the dry season. The point of this article is not to reconstruct palaeoclimate but just to bring suggestions on what might have been the driver of the observed vegetation shift and how Global vegetation model can bring insight in such study. Realistically, we think that both factors (seasonality and total amount) occurred simultaneously and changed the vegetation composition.

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

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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".

We agree and we acknowledged and added relevant citations in the first paragraph of the introduction.

Interactive comment on Clim. Past Discuss., 5, 853, 2009.