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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.

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This MS makes a good methodological point, and illustrates it well using sensitivity analyses with a process-based biogeography model, BIOME3.5. I personally did not find it surprising that the length of the dry season is a more important control of vegetation than total annual precipitation. However I appreciate that much of the palynological literature (including some quantitative reconstructions) has focused on total annual precipitation, so the point is worth making, in a palaeoecological context. The MS is publishable, but in its current form it suffers from several major flaws that need to be rectified in a revision. 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. 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. 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. 4. The way in which elevation is treated is wrong, and must be removed from the MS. The error made is that CO_2 partial pressure is varied, while O_2 partial pressure is not. In reality, the partial pressure of O_2 declines as the same rate as the partial pressure of CO_2. So as O_2 competes with CO_2 for the Rubisco reaction sites, the decline in O_2 offsets the decline in CO_2, 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.) 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.)

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