

Interactive comment on “Late Quaternary vegetation – climate feedbacks¹” by M. Claussen

N. de Noblet (Referee)

nathalie.de-noblet@lsce.ipsl.fr

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See attached PDF file for detailed comments. Here you'll only find the main ones.

The paper presented by Martin Claussen summarizes the knowledge we have gained, over the past 15 years, on the active role biosphere has played in the climate system, over the last glacial-interglacial cycle. This paper is therefore a review article that tries to be as exhaustive as possible on the published literature on that specific subject. It more specifically points to the continuous lack of accounting for both biogeophysical (i.e. energy, radiation & water) and biogeochemical feedbacks (i.e. impacts of the atmospheric content of greenhouse gases) in such studies. This prevents our community from significantly quantify the integral impact of dynamic vegetation on climate transitions and states since both feedbacks go in opposite direction, as far as we can tell

¹Invited contribution by M. Claussen, EGU Milutin Milankovic Medal winner 2005

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from the studies published so far.

This paper is therefore very important, specially now that IPCC scenarios suggest that vegetation should be accounted for by all models in the simulations that will be produced for the next assessment report (AR5). Of course, it is mainly land-use induced land-cover changes that have been suggested for inclusion, but the various studies reported by Claussen illustrate that such incorporation may lead to significant climatic impacts.

Because such paper is so important, I think it has to be written in a very balanced way, and for a public that is not just the expert one. It has to convince atmosphere-ocean modellers that biosphere is indeed essential. This is why I think this paper needs some significant revisions : it is too much expert-oriented. The discussions are uneven, and the items that the author himself have worked on are more throughouly scrutinized than the others. What we get out of the paper though, is the desire to read more literature on the subject and this is a very good outcome of this work !

You will find below some more specific comments, but based on what I just said, I accept this manuscript provided the authors go through some 'major' revisions. Those essentially include some re-organisation, the lengthening of some parts, and some clarifications. They are what I would call minor but they need to be done.

Major Comments :

1. The 'biogeochemical aspects' section (§3) includes 2 different parts : the first one (3.1) that indeed reports only on the biogeochemical feedbacks and that is 'analogous' to section 2, and the other 2 (3.2 & 3.3) that compare biogeophysical & biogeochemical feedbacks. The latter 2 deserve to be included in a specific 4th section for clarification. I know that section 3 will end up being very small, but it will then more clearly point to the lack of modelling studies that have addressed this aspect ! And this should be clearly stated by the author. Adding a 4th section would also allow the author to more explicitey address the urgent need to systematically combine, in future simulations,

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biogeophysical & biogeochemical feedbacks of vegetation on climate.

2. The way the different time-scales are linked/addressed is not always clear to the reader. The author reports on transitions, snap-shots, and continuous variations and I think this should be announced in the introduction with a logic chosen for reporting.

3. In section 2 it is not always clear when the author reports on atmosphere-vegetation only simulations (with prescribed present-day SSTs) or on coupled atmosphere-vegetation-ocean ones ... while I think it needs to be clarified because including ocean feedbacks do not always reinforce the outcome of the atmosphere-vegetation only models.

4. Reference to land-use induced land-cover changes is made in your introduction and in section 2. As you say, it is a human-made perturbation and does not result from continuous dynamics between natural vegetation and the rest of the climate system. I think reference to what happens now to the land is important and should be discussed in this paper, but it deserves a separate discussion and not to be mixed up with paleo-changes. The latter should only be used to highlight what may happen due to land-use changes.

5. Sections 2.1.2 & 2.1.1 are not really independent from each other and I would recommend grouping them.

6. There are 2 points that bother me in section 2.2.1 : the first one is that the author discusses a northward shift of Sahelian vegetation, i.e. a greening of the Sahel, while the physical explanations given, based on Charney (1975) & Otterman (1974) are for a desertification process. Although experts are used to twist around this explanation I think it deserves clarification for non-experts ; moreover at that time of the Quaternary, vegetation was not progressing northward but southward, i.e. reducing its extent in the Sahara/Sahel. Talking about a 'greening' of the Sahara is therefore, chronologically speaking, not true. Again experts are used to this notion while non-experts need to be enlightened.

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7. I have trouble with section 3.1 since it mixes up results from simulations that have explored the very recent past (last 150 years) and the near future (next 100 years) with the paleo-perspective that is mainly discussed in section 2 with almost no reference to the 'present' climate. I can understand why the author has done this : the main results on the importance of biogeochemical feedbacks result from IPCC-like simulations (C4MIP project). But although I think these references are important and necessary here, they should be brought in with caution. One example : the CO₂-fertilization – CO₂ uptake feedback may not have the same significance in the far past and the recent one (or the future). The latter clearly happens because CO₂ has started to increase in the atmosphere due to Human activities, while in the past, we do not know whether CO₂ changes happened prior or after vegetation changes ! On the one hand we discuss effects of increased GHG on vegetation-atmosphere interactions, on the other hand we question how did vegetation participate to the observed past changes in atmospheric CO₂ concentration. Those are different questions.

8. I think the author should include more discussion in the conclusion on 2 main aspects : isolate more clearly what a) we know for sure, b) is still debatable, c) needs to be certified by running other simulations with more models ; some recommendations regarding additional feedback studies to be carried out in the near future

9. In your conclusion, 1st paragraph, you speak of land-surface models included in GCMs as if now they were all accounting for dynamic calculation of leaf seasonality and long-term vegetation changes. You and I very well know this is not true ! At least it is certainly not true yet for all IPCC models. It may be true for the ones that simulate paleo-climates though. If so it has to be stated clearly since I hope that not only paleo scientists will read this paper.

Please also note the Supplement to this comment.

Interactive comment on Clim. Past Discuss., 5, 635, 2009.

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