

**Review of the paper submitted by Claussen, entitled :  
« Late Quaternary vegetation-climate feedbacks »**

**review done by Nathalie de Noblet-Ducoudré**

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 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 thoroughly 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 4<sup>th</sup> 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 explicitly address the urgent need to systematically combine, in future simulations, 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.
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 CO<sub>2</sub>-fertilization – CO<sub>2</sub> uptake feedback may not have the same significance in the far past and the recent one (or the future). The latter clearly happens because CO<sub>2</sub> has started to increase in the atmosphere due to Human activities, while in the past, we do not know whether CO<sub>2</sub> 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 CO<sub>2</sub> 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, 1<sup>st</sup> 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.

### **Minor comments :**

1. in the introduction (1<sup>st</sup> paragraph), there is one sentence that I think is not correct (grammatically) : « Thereby, dark daisies provide favourable ....widespread growth of daisies ».
2. It is not obvious to me what papers the citation : 'see discussions in *Climatic Change* vol. 52, 2002 and vol. 57, 2003, for example' refer to. Moreover, is the work 'falsifiable' the appropriate one ?
3. In section 2.1.1 you have included a footnote that says pre-industrial was a time with no large anthropogenic modification of the climate system ... I think I disagree with this statement since land-cover perturbation was already quite large ! This should be rephrased.
4. In section 2.1.1 still, you report on the work published by Gallimore et al. (2005) and cite a 'cooling effect' resulting from increased grass & shrub. I think it is worth explaining where (on land) and why (drought ?) this increase is simulated.
5. In section 2.1.1 still, you report on the work published by Crucifix et al. (2002) and discuss northern hemisphere cooling resulting from land-use changes. I found this reference rather strange in this section. Land-use, as you clearly state in your introduction, is a human-made perturbation while you mainly discuss, in your paper, dynamics between natural vegetation and the rest of the climate system. I think you should clarify this inclusion, or move it to a more specific section of your paper (see my major comment § 4).
6. There are 2 references to the work of Crucifix et al. (2002) .... should be a & b for clarification.
7. The end of section 2.1.1 is not clear to me : how can you say from observations (which I think you are referring to here) that the northward migration of boreal forests have some amplifying effect on mid-Holocene ? I can understand this from modelling studies but not from observations ....
8. In section 2.1.3, end of 2<sup>nd</sup> paragraph, it seems to me that the last 2 sentences contradict each other. On the one hand you say that biogeophysical feedbacks during glacial times are equivalent (in order of magnitude) to the ones experienced during the mid-Holocene. While on the other hand (last sentence referring to the paper by Brovkin et al. 2003) you say that the feedbacks tend to be stronger in colder climate. Could you please clarify ?
9. End of section 2.1.3 : you end up saying very briefly that synergies contribute to amplify the precessional signal while it seems to me that the work you report about show that synergies dominate the response ! Could you please again clarify ?
10. In section 2.2.1, reference to Eltahir and Gong is 1996 and not 1995.
11. In section 2.2.1 still, 3<sup>rd</sup> paragraph, the differences in albedo in north Africa do not result from prescription but from calculations ... I think you should say this clearly and rephrase your statement.
12. In section 2.2.1 still, 4<sup>th</sup> paragraph, you say that Wang et al. (2008) found a negative biogeophysical feedback related to background evaporation, but you do not explain how this works.
13. At the end of section 2.2.1 the reference is not complete ... end of sentence missing I think.
14. I would avoid the word '*exciting*' in the first sentence of section 2.2.2.

15. Section 2.2.2, 4<sup>th</sup> paragraph, I'm not sure the following sentence is correctly written : 'They conclude that rainfall ..... FOAM-LPJ (Fig. 5c)'.
16. I'm not sure the last sentence of section 2.2.3 is sufficiently clear for non-experts (non-vegetation experts OR non-paleo experts) : 'From these studies one can conclude ..... expected in the Sahara'.
17. You could expand further on your last sentence of section 3.1 : 'Possibilities of a positive feedback ..... due to burial ...'.
18. References : the Shukla reference is missing.
19. Legend of figure 1 would deserve more explanations. + the 'dotted black' line is not visible on the bottom picture.
20. Figure 2 : I would add the references on top of each 'box'. What are you showing here ? Global changes or regional changes ?