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Biogeosciences Discussions

Interactive comment on "Impact of CO_2 and climate on Last Glacial Maximum vegetation – a factor separation" *by* M. Claussen et al.

Anonymous Referee #2

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The paper presents a modelling analysis of how vegetation changes between the pre-industrial state (PI) and the state at the Last Glacial Maximum (LGM) are determined by the individual drivers climate and CO_2, demonstrates how much each of the drivers contributed to the total vegetation shift in different regions and demonstrates the non-linear behaviour of the temperature sensitivity of the CO_2-effect and the CO_2 sensitivity of the temperature effect. The experiments are conducted with a state-of-the-art climate-carbon cycle model, where feedbacks with the ocean are ignored. The experimental design is simple but fully serves the point which the authors are trying to make. For me, this is the critical point. I like the simplicity of the argumentation (CO_2/climate factor separation) but the study could gain in focus, if the line of arguments would be more clearly visible throughout the entire manuscript (large parts of the manuscript address the combined effect of CO_2 and climate).

The argumentation also has to make clear that this paper presents a considerable innovation beyond the existing literature. In my view, this is not fully satisfied. It has to be made clear that this paper takes matters further than, e.g., the study by HarrisonPrentice (2003) [HP03], Crucifix et al. (2005) [CX05], and Woillez et al. (2011) [WO11]. The finding on p. 15838, l. 3-4 "the ecophysiologcial effect of enhanced CO_2 seems to be larger in interglacial, warmer climate than in glacial, colder climate" is not new and the present paper doesn't provide any additional insights into the processes at work that determine this outcome (e.g., low CO_2 effects on water use efficiency -> more arid vegetation under low CO_2?).

The issue of factor separation in the context of LGM vs. preindustrial vegetation has been addressed before with other models, although not all of them dispose of a simulation where only CO_2 effect on LGM vegetation was separated (type CTRL-R). However, WO11 do include such a simulation. I am not arguing that a common "standard" model analysis (as the simulation of LGM vs. PI vegetation) can only be published once by the first group. Confirmation/rejection by subsequent studies is important. However, the present paper lacks a clear statement of how exactly the presented results fit into the existing literature (what is confirmed? what new insights are gained?).

The comparison with model results of WO11 presented here (p. 15835, l. 17ff) is important in order to know how the two models behave differently but it does not shed any light on the range of model results in general. For such an analysis, comprehensive model inter-comparison projects are inevitable. But this is clearly not the scope here. Therefore, I suggest to avoid to put too much emphasis on the model-to-model comparisons with WO11 and instead extend the dicussion of points not adressed in previous publications. I see potential to do so as the authors already provide most of this data and analysis. In particular, the rigorous factor separation is a significant innovation beyond the existing literature and warrants publication in Biogeosciences if authors manage to sufficiently address the comments below. For these reasons, I propose major revisions without any additional model simulations. Revisions should address the presentation of results and the discussion.

General comments

- Put more emphasis on the factor separation approach:
  - Lines 4-10 in the abstract introduce the argumentation. However, it does not become clear how this line of argument is followed in the subsequent sentences in the abstract. In my view, it must become clearer what lessons are learned from this particular analysis already in the abstract. The finding "...for tropical forests, an increase in CO_2 has, on average, a stronger eco-physiological eïňĂect in warmer climate than in glacial climate." is not new and cannot be the main message here.
  - Introduce the factor separation method (section 3.3) at an earlier stage and more prominently. In my view, this is the central piece of the paper. I suggest to introduce it in the methods section, at best before the model description, as this is of secondary relevance.
  - A figure graphically illustrating f_C, f_E, g_C and g_E would be very helpful. The difference between the f and g factors is not further dwelled upon. The method introduced by Equations 1.a)-d) nicely defines what is meant by the "temperature sensitivity of the ecophysiological CO_2 effect" (could be defined as β=g_E/f_E) and the "CO_2 sensitivity of the temperature effect" (could be defined as γ=g_C/f_C). Once, this is defined as such, further analyses could provide information for which variables and in which regions β and γ are important. Lines 27, page 15834 to line 17 on the subsequent page address this issue. But could the discussion of this point be extended after it's been introduced prominently in the abstract?
- Argue why a coupled vegetation-climate model provides additional insight beyond what an offline vegetation model could do. Alternatively, this analysis could have

been done by prescribing PI and LGM climate, combined with PI and LGM CO_2 levels directly to the vegetation model. This would have been computationally much less expensive. How feedbacks between vegetation and climate (e.g., Jahn et al., 2005) affect the results does not become clear and as such, the modelling effort appears to be unnecessarily large in view of the analysis that is done.
- The model-observations comparison is somewhat selective. Fig. 1 (a) provides maps comparing PI modelled and observed tree/shrub cover. The paper mostly addresses PFTs as simulated for PI and LGM, but the reader is left in the dark about how the model performs in that respect. Can the comparison be provided by biomes, classifying vegetation types by predominant PFTs? Simulated LGM biomes could then be compared to reconstructions based on pollen records (see Figure 1 in HP03) and conclusions could be reached about which driver (climate and CO_2) explains how much of the difference between PI and LGM in a more quantitative way (see ΔW statistic in HP03). The paper would greatly gain from such a discussion of model results in the view of observational data. And it would provide further justification to why the present paper should be published after HP03 (you do have a CO_2-only simulation, they don't!).
- What processes are at work determining the sensitivity of vegetation to climate, CO_2 and their synergy? The paper generally lacks an introduction into relevant plant-physiological and ecological processes at work and does not put results into their context. The paper would gain from providing a deeper insight into what determines these global model results.

Specific comments

p. 15824, l. 7-10 These sentences are not really backed up by the further information given in the abstract. As I understand, the sensitivities mentioned here are β and γ, as defined in comments above. The rest of the information given in the abstract mainly deals with the synergy effects f_{CE}. Revisions to the abstract text are required for clarification. Further below, Eq. 2 demonstrates that the synergy is equal to the sensitivity of the CO_2 effect. This seems to be an important aspect to guide the discussion, but as such, appears too late.

p. 15826, l. 8-10 I disagree on the claim that a "systematic factor separation" had not been done before. WO11 do separate contributions from CO_2 and climate alone. It has to be made clearer what the innovation of this paper is. Is it just that the synergy term is defined and quantified? What about β and γ?

p. 15826, l. 20-22 Can you provide more support for the simplification that vegetation ocean feedbacks are negligable? Briefly discuss and refer to any analysis addressing vegetation-atmosphere-ocean dynamics.

p. 15826, l. 25-27 (This comment is basically the same as the one I mentioned above) The comprehensive model applied here simulates feedbacks between land surface and the atmosphere, such as evapotranspiration-water vapor-precipitation feedbacks. But this information is not drawn upon. Could the analysis have been conducted with a simpler model? If not, could you elaborate more on the effects of such feedbacks in determining LGM vegetation?

p. 15828, l. 1-5 Please provide an explanation for the naming convention: what does R and E stand for? Is a more intuitive naming possible?

p. 15828, l. 17-18 How exactly did you account for anthropogenic land use? Reduced the tree/shrub cover by the respective land use area fraction? Please provide this information here. If you chose to present a comparison based on biomes (as suggested in my comment above), then apply the biomization to the natural land tile only.

Section 3.2 A comprehensive presentation of simulated glacial vegetation is provided. I assume this has been taken from the "LGM" simulation. Please state at the beginning of this section, to which simulations you are referring. While model-observations

comparison is inevitable for this study, and this paragraph certainly provides respective information, the results presented here do not feed into the line of argument of the factor separation and detract attention to the more general question "can our model simulate LGM vegetation?" Would it be possible to rather present results here in light of the question "What drivers do we need to capture the full amplitude of PI-LGM vegetation changes? How would veg. look like if only climate (CO_2) changed. And do we get reasonable results if both changed?" (the latter point is presently addressed here). Of course the first points are addressed in section 3.3. By moving the factor separation technique (Eqs.1) to the Methods section, results could be provided with a clearer reference to the central question of the factor separation/synergy effects/sensitivities of factors (β,γ).

p. 15833, l. 1 "Please note that the factors diïňĂer." This is a crucial point of this analysis and should be emphasized more. Note that if you write it like this, an areal contraction in the LGM will have a positive g, while an areal contraction in the LGM will have a negative f. So of course, they are not equal! As I understand, the crucial point is that even their absolute values are not equal.

p. 15833, l. 2-3 I don't understand this: g_C should be the senstivity to climate? So why do you compare runs with same climate but differing CO_2? I assume g_C and g_E have been confused here.

p. 15833, l. 15-16 Would it be possible to provide any information on how exactly the land area changes? (change in total land/ocean/ice area). This information is also important to interpret Fig. 4. I assume, part of the changes presented in Fig. 4 are probably simply due to land area changes. Would it be possible to factor these out to provide a more concise statement about ecological effects rather than effects due to rising sea level and retreating glaciers?

p. 15833, l. 27 This seems a very interesting point: that NPP changes between preindustrial and LGM are almost completely due to CO_2 effects. Is it possible to draw more on this result? How are vegetation patterns affected differently than NPP? Maybe add a figure for the factors synergies f (g) representing NPP (in the style of Fig.6)?

p. 15833, l.15-16 Is "positive" adding to individual effects or mitigating individual effects as individual effects are negative? In Fig.6, most of the yellow bars point to negative values, so I am left confused why the authors state that “positive values dominate”. Please explain what “positive” synergy means (in terms of f and in terms of g). I guess it means that the ecophysiological effect is stronger in a warm climate than in a cold. This should be provided as an explanation here.

Minor comments

Fig. 2 Representing annual mean?

Fig. 2 color scale: the two greens are not distinguishable! and almost everything is covered by green colors.

Fig. 4, 6, 9 Figure legend is missing. Information is given in caption, but a legend greatly facilitates reading

Fig. 5 Are positive values (green colors) representing expansion or contraction when going from LGM to PI?

Fig. 7 Labels “LGM-E – CTRL” etc. are not intuitively understandable and the reader has to look it up. Using notation introduced in Sect. 3.3 would help (g_C, g_E, g_CE) or even more intuitively (“climate effect”, “CO_2 effect”, “synergy effect”)?

Interactive comment on Biogeosciences Discuss., 9, 15823, 2012.