Reply to Comments on BG-2012-481

Introductory remark

We thank the referees for their constructive comments which help to clarify several points and help to improve the paper.

Both referees raised the question of what is new in our study. Woillez et al (2011) and we independently did our simulations having the same idea in mind: separating the factors ‘climate’ and ‘ecophysiology’ which contribute to the difference between simulated glacial and pre-industrial vegetation patterns. For a complete factor analysis, i.e., for separating the pure contributions of n factors and the synergy arising from factors, one would need 2^n simulations. Woillez et al. published their results first, and they are the first who provided the complete set of simulations necessary for a factor separation. However, they discussed their factor separation qualitatively without putting numbers on the factors and the synergy. And they mentioned that “the relative impact of glacial and CO_2 is not simply additive”, but they do not mention the sign, nor do they quantify this effect.

So our study goes a step further. We formulate the equations of the factor separation for this case and we compute the factors explicitly. Furthermore, we show that the synergy term can be interpreted as the temperature sensitivity of the CO_2 effect. (Alternatively, and equally valid, the synergy can be interpreted as CO_2-sensitivity of the climate effect, see Eq.s 3a and 3b in the revised version of our manuscript). Hence it is the mathematical treatment that is new. With the factor separation we are able to quantify to which extent the ecophysiological effect of enhanced CO_2 increases in warmer climate than in colder climate. This could be a useful tool for further studies and model intercomparisons.

Reply to Referee 1:

Referee 1:

1) *Abstract. Line 6-8. I did not understand this sentence.*

Response:

We have reformulated this sentence.

Referee 1:

2) *p 15826, lines 10-16. The study claims that this was not done before, but they compare the results to Woillez et al. I find this confusing. Perhaps drop the claim and better explain how similar (or different) this study is to the one by Woillez et al.*

Response:

We hope, the revised text is formulated more clearly. It is the rigorous application of the factor separation which is laid out in our study (see introductory remark).

Referee 1:

3) *p 15826, line 24: "... with this constraint" unclear.*

Response:

We agree and we dropped this sentence.

Referee 1:

4) *p 15827, a brief explanation on how the different functional types differ from their ecophysiology would help to understand why the distribution of PFTs should change.*

Response:

A brief statement on physiology and albedo is added (see page 5 of the revised manuscript).

Referee 1:

5) *p 15837, I find the summary and conclusion rather long and somewhat repetitive. I would place the comparison to Woillez et al. at the end of the results section, and shorten the summary and conclusions section.*

Response:

We find this suggestion very useful, and we modified the text accordingly.

Referee 1:

6) *I noticed that the journal abbreviations in the reference list are sometimes different (JAMES vs. J. Adv. Modeling...) and that there is a spelling mistake in the Collatz et al reference.*

Response:

Corrections are done.

Referee 1:

7) *Figs. 2, 3 6: It would be better to describe these as differences, rather than changes. The notion of "change" typically has a temporal meaning (differences through time), which are not dealt with in this study.*

We agree. Corrections are done.

Reply to Referee 2:

Referee 2:

The paper presents a modelling analysis of how vegetation changes between the preindustrial state (PI) and the state at the Last Glacial Maximum (LGM) are determined by the individual drivers climate and CO2, demonstrates how much each of the drivers contributed to the total vegetation shift in different regions and demonstrates the nonlinear behaviour of the temperature sensitivity of the CO2-effect and the CO2 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 (CO2/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 CO2 and climate)

Response:

We revised the text to sharpen the focus of our paper on the rigorous application of the factor separation to the problem of differences between LGM and pre-industrial climate. Specifically, we moved the outline of the factor separation to a new Section 2.1, right after the Introduction and before presenting the MPI-ESM.

Referee 2:

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 CO2 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 CO2 effects on water use efficiency -> more arid vegetation under low CO2?).

Response:

Previous studies mentioned above (Harrison and Prentice, Crucifix et al) addressed the issue of climate versus ecophysiological effects on vegetation differences. But only the study by Woillez et al and the present study present the complete set of simulations for a factor separation, i.e., the separation between pure effects of climate and ecophysiology and the synergy between these effects on vegetation differences between LGM and pre-industrial climate.

Our statement that the "ecophysiological effect of enhanced CO_2 seems to be larger …" might be not new. But in which study is this effect quantified? Woillez et al.

(2011), who could have specified this effect, just mentioned that the ecophysiological effect varies with climate, but they did not quantify this effect.

Referee 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 CO2 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?).

Response:

Only Woillez et al. have done the simulations which are necessary to address the factor separation properly. But they have not done the evaluation of factor explicitly. Of course, one can calculate the factors from the figures presented in Woillez et al. That’s what we did to compare their results with ours. (And therefore, I invited Marie-Noelle Woillez to be included in the list of co-authors, because she kindly gave us the numbers of vegetation coverage in her simulations.)

Referee 2:

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.

Response:

Since only Woillez et al. have presented the complete set of 4 simulations that are necessary to do the factor separation, we do find it useful to compare our results with theirs.

Referee 2:

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*

Response:

We agree with the referee's statement that the rigorous factor separation is the significant innovation. We are convinced that this method is a convenient tool for model intercomparisons. We revise our text to highlight this point.

General comments

Referee 2:

• 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 CO2 has, on average, a stronger ecophysiological effect in warmer climate than in glacial climate." is not new and cannot be the main message here.

Response:

We reorganized the abstract to emphasis the use of the factor separation. And we add our estimate to which extent the ecophysiological effect diminishes the pure contributions. Giving such a number is new to our knowledge.

Referee 2:

– 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.

Response:

We agree, and we include the presentation in Section 2 by introducing new sub-sections 2.1 (Factor separation) and 2.2. (Model and model set up).

Referee 2:

– A figure graphically illustrating fC, fE, gC and gE 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 CO2 effect" (could be defined as β=gE/fE) and the

"CO2 sensitivity of the temperature effect" (could be defined as γ=gC/fC). 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?

Response:

We thank the referee for this question, because it points at some misunderstanding of the methodology we use in our study. The β and γ parameters are used in the carbon cycle feedbacks studies (e.g., Friedlingstein et al., Journal of Climate, 19, 2006) and they are taken as sensitivities of carbon storages to CO_2 and climate changes, respectively. The idea behind the feedback analysis is to compare the strength of the T-CO2 feedback among the models. Here, we do not focus on strength of climate-vegetation feedback. Instead, we use the factor separation method by Stein and Alpert. Let us illustrate it by presenting the factors f, g and the ratios of g/f.

Fig.R1: factors f, using pre-industrial climate as control

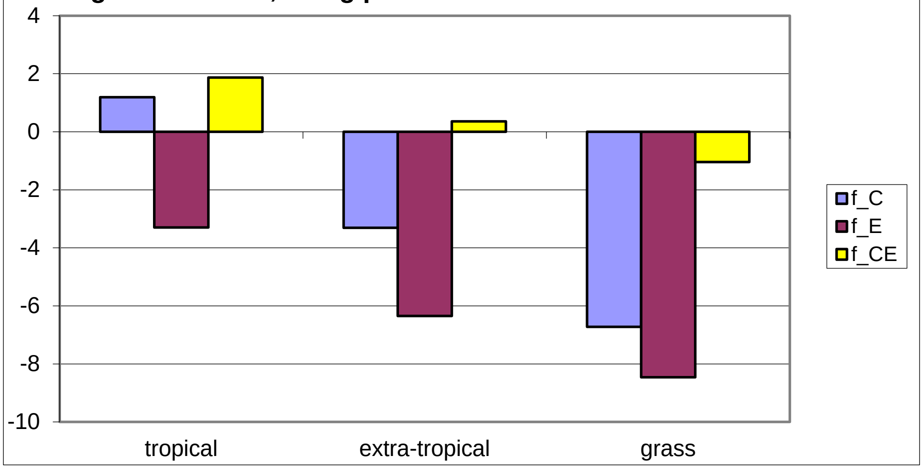


Fig.R2: factors g, using LGM climate as control

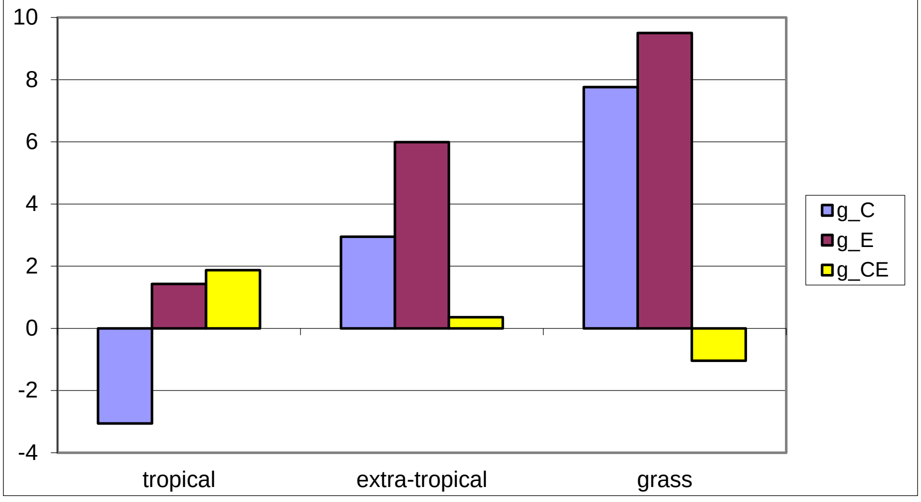
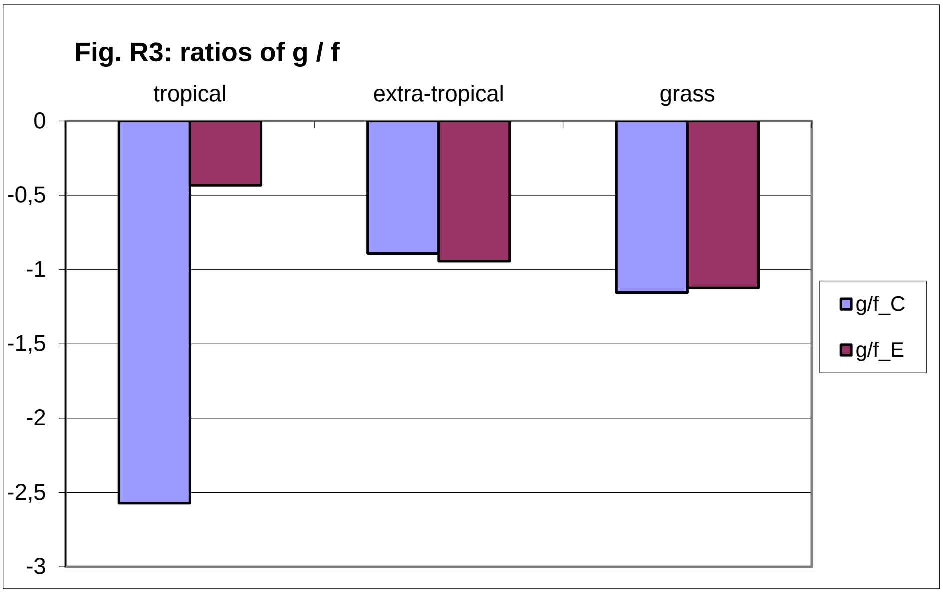


Figure R1 is a copy of the Figure 9a in the paper which shows the factors *f*. Figure R2 depicts the factors g (Eq.s 2a-c). It is obvious that *f_C,g_C* and *f_E, g_E*, respectively, are of opposite sign – as expected. When viewing from preindustrial climate, a positive difference in some vegetation coverage implies a negative difference, when viewing from the LGM climate. The absolute values /norm of *f_C,g_C* and *f_E, g_E* differ, because *f_C,g_C* and *f_E, g_E* are not synmmetric.

We think there is no grossly new information in the factors *g_C, g_E* that warrants an indepth discussion of factors *f_C,f_E* **and** *g_C, g_E*.

The synergies *f_CE* and *g_CE* are the same as deduced in the text (Eq.s 1c and 2c), as confirmed by Figure R1 and Figure R2. In the text, it is clearly shown (Eq. 3a) that the difference between the ecophysiological effect of enhanced CO_2 in warm climate and in cold climate is the synergy *f_CE* (= *g_CE*).

It does not make sense to choose the ratios *g_E/f_E* or *g/C/f_C* as suggested by the referee. As *f_C,g_C* and *f_E, g_E*, respectively, are of opposite sign, the ratios are always negative which is confirmed by Figure R3:



The ratios *g_E/f_E* or *g/C/f_C* might have some meaning, but we are convinced that the synergies *f_CE* (= *g_CE*) are more appropriate as an indicator of the difference between the ecophysiological effect of enhanced CO_2 in warm climate and in cold climate. Hence we choose to refrain from a discussion of the ratios *g_E/f_E* or *g/C/f_C.*

Referee 2:

• *Argue why a coupled vegetation-climate model provides additional in sight 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 CO2 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.*

Response:

We agree with the referee that the effect of interactive vegetation dynamics on the factor separation would be an interesting research question – like the question of the role of vegetation dynamics on LGM climate in general. The paper by Jahn et al. is not very helpful in this case, because they explored the effects of ice-sheet, CO2 and vegetation on glacial climate, the ocean was always interactive. Hence the specific question of the strength of atmosphere-ocean-vegetation feedback versus atmosphere-vegetation feedback was not resolved.

We certainly will do such a factor separation with the new system. However this question is not the scope of the present study. Further simulations would be necessary to isolate this effect, and the focus of this study would be diluted.

Furthermore we think that one should argue why an approximation is made and not, why an approximation is not made. Simulations with coupled atmosphere – biosphere models are not excessively expensive any more.

Referee 2:

• *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 CO2) 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 CO2-only simulation, they don't!).*

Response:

The land surface model JSBACH uses the "continuous vegetation" approach, i.e., each grid cell is covered by relative fractions of PFTs – like in other dynamic global vegetation models such as ORCHIDEE used by Woillez et al. Our model mainly differentiates between woody types and grasses. The woody PFTs are further subdivided into tropical and extratropical. These sub-PFTs do not overlap geographically. Hence it is possible to identify errors in tropical and extratropical tree/shrub coverage, respectively, when comparing model results with estimates from satellite data. Since most above-ground carbon is stored in tree/shrub coverage, we consider this coverage as decisive variable for validation. For further details we refer to the papers by Brovkin et al. (2009) and the most recently published validation of our model by Brovkin et al (2013). We have revised our text accordingly at the beginning of Section 3.1.

Regarding a comparison with biomes, we could try to find linear combinations of *n=1, …, N* relative PFT coverages C_n for each biome B_i by an ansatz:

$$B_i = \sum_{n=1}^{N} C_{n,i}$$

One would have to drive a biome model (BIOME-4, for example) and the offline version of JSBACH with the same climate data to solve the above set of equation. It is not clear a priori whether a unique solution exists. In any case, this procedure would make a direct comparison between results of our study and the results based on the biomes used in the Harrison/Prentice paper feasible. It would be of little help, though, for a comparison with the other studies (and other vegetation models IBIS; TRIFFID, VECODE). If aggregated to the level of trees and grasses, we can compare our results with ORCHIDEE which was used by Woillez et al. And we find a comparison with Woillez etal particularly useful, because they did a complete set of factor simulation like in our study (Fig. 9). We agree with the referee that a harmonization between PFTs and biomes used in different vegetation models would be desirable for furthere model intercomparison, but this is beyond the scope of our paper. Finally we think that a factor separation based on PFTs is not less quantitative than a factor separation based on biomes.

Referee 2:

• *What processes are at work determining the sensitivity of vegetation to climate, CO2 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.*

Response:

We briefly mentioned the processes that cause the synergy factor. By comparing the difference figures, we concluded that the competition between PFTs can hardly be the main source of synergy. We hypothesized that in our model, synergy is linked to the physiology of each PFT which is described by the Farquhar model. In this model, net primary production is a nonlinear function of temperature and CO_2. In the revised version, we add more information on the physiology of PFTs in our model.

Specific comments

Referee 2:

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 fCE. Revisions to the abstract text are required for clarification. Further below, Eq. 2 demonstrates that the synergy is equal to the sensitivity of the CO2 effect. This seems to be an important aspect to guide the discussion, but as such, appears too late.*

Response:

The sensitivity mentioned in the abstract is the synergy, not the sensitivities defined by the referee. The synergy can be interpreted in two different ways as shown in the text. We will change the abstract to make this point clearer.

Referee 2:

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 CO2 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*?*

Response:

As outlined in the beginning, Woillez et al did the simulation to produce a factor separation, but they did not compute them explicitly. We clarify this in our revision and we reduce our claim. As shown above, the ratios β *and* γ are not useful indicators.

Referee 2:

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.

Response:

Yes, we can, but only tentatively. We have not yet done a full analysis to explore the importance of atmosphere-vegetation versus atmosphere-ocean versus atmosphere-ocean-vegetation interaction. We know, however, how our model behaves in mid-Holocene climate. In this case, we see little synergy even in hot spots like high northern latitudes and the Asian monsoon system. We add this remark in the revised version of our paper (p.5).

Referee 2:

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?

Response:

As mentioned above, we can address this problem only by a full analysis and rigorous factor separation with new simulations. That has not yet been done with our model. Such an analysis is a large computational effort, because ocean dynamics with their long time scales are involved. Several multi-millennial simulations have to be done.

Referee 2:

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?

Response:

R stands for ‘reduced CO_2’ and E, for ‘enhanced CO_2’. We add this brief explanation to the text. We find this plausible. Other naming is possible, depending on personal taste.

Referee 2:

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.

Response

We implemented the reconstruction of anthropogenic land cover change by Pongratz et al. (2008). (See p.6 in the revised version of our paper.)

Referee 2:

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 (CO2) 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 and γ).*

Response:

We agree with the referee. We shorten the intercomparison with data and previous modelling studies considerably by some 50% (the new section 3.3), and we shift the formulation of the factor separation for our analysis to method section (new section 2.1). The discussion of factors and synergy (new section 3.4) is by far the largest section of our paper.

Referee 2:

p. 15833, l. 1 "Please note that the factors differ." 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.

Response:

It is worth noting that a factor separation depends on the point of view, i.e., on the choice of the control case. But that is not the crucial point. The results are qualitatively, not quantitatively, similar, regardless of viewing the analysis from the pre-industrial climate of from the LGM climate as a reference point. The crucial point is that the synergy remains exactly the same (See Figure R1 and R2). A second important point is that the synergy can be interpreted in two different ways.

Referee 2:

p. 15833, l. 2-3 I don't understand this: gC should be the senstivity to climate? So why do you compare runs with same climate but differing CO2? I assume gC and gE have been confused here.

Response:

Indeed, g_C was mistaken for g_E here.

Referee 2:

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?

Response:

We can give this information, and we do so in the revised text (in the second part of the new section 2.2). Changes in land area and changes in the area covered by inland ice are included into the climate factor. When ecophysiological available CO_2 is changed, then the land-sea mask and the area of glaciers are not changed, i.e., the simulation CTRL-R and CTRL the land area and the glacier are the same, so it is in the simulations LGM-E and LGM. A further separation between changes in the atmosphere (temperature, precipitation, …) and rising sea level and retreating glaciers would require 2^3 or 2^4 new simulations, respectively. That's beyond the scope of this study.

Referee 2:

p. 15833, l. 27 This seems a very interesting point: that NPP changes between preindustrial and LGM are almost completely due to CO2 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)?

Response:

That, indeed, would be interesting to analyse. Unfortunately, we have stored only NPP and carbon pools for each grid cell, but not NPP for each PFT in each grid cell.

Referee 2:

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.

Response:

(We assume that the referee refers to p. 15834, l.15-16.) This interpretation is correct. In our revised version, we explain it more thoroughly.

Minor comments

Fig. 2 Representing annual mean?

Yes, annual mean. (Is corrected for.)

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

It is a matter of the printer. We preferred to choose precisely the same colour coding as in the original paper by Braconnot et al. (2007)

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

Legend is included in the revised paper.

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

In the caption, it is stated "difference between LGM and pre-industrial climate". Hence when the difference is positive (green colours), the area covered by a PFT is larger in LGM climate than in pre-industrial climate.

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 (gC, gE, gCE) or even more intuitively ("climate effect", "CO2 effect", "synergy effect")?

Good point. In the revised paper we use the more intuitively labels as suggested by the referee.