Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-187-RC1, 2018


Biogeosciences
Discussions
Open Access

# *Interactive comment on* "The impact of spatiotemporal variability in atmospheric $CO_2$ concentration on global terrestrial carbon fluxes" *by* Eunjee Lee et al.

**Anonymous Referee #1**

Received and published: 22 May 2018

Review of "The impact of spatiotemporal variablity..." by Eunjen Lee et al., submitted to Biogeosciences

General comments

Lee et al. have studied the effect of different atmospheric co2 concentration forcing datasets on GPP and NBP of a global vegetation model, Catchment-CN. Their control case has 3-hourly temporal resolution, and then they make different cases with coarser temporal evolution, and finally also by omitting spatial resolution, in order to see what role these different resolutions play. They evaluate these different runs at global scale, and also regional assessment is done, based on TransCom regions.



The text is well written. It's more like a sensitivity study, than a new frontier exploration. The main motivations seem to be assessing how much sense the new CMIP6 run recommendations make and challenging results from a study by Liu et al. and as such I consider its publication to be justified, as the work and analysis seem to be sound and the topic is important.

However, there are some points in the manuscript that I think need further work. E.g. I found the comparison to FACE experiments problematic, as the methodology used was not clearly explained and also, it is not really something that is highly relevant for this study (that addresses short term / small changes in atmospheric co2 concentration). Also the different test cases were not that clearly motivated. I therefore recommend publication with major revisions and I hope the authors would address the following points.

Major comments

p. 2, l. 13: It is not clear for me, what is meant by that quantification of drivers in the models would help to improve them. The forcing makes the models to behave exactly like they have been coded. Could you clarify your point a bit?

p. 3, l. 20: What do you mean by 'flux sensitivities'?

Section 2.1. You should also mention, how the fires are treated in the model. Now their role is mentioned on p. 9, l. 8 and shown in one equation, and raises questions.

p. 7, l. 29-30: I'm not sure I interpret your tests correctly. Why did you not study, how much annually changing co2 concentration (without spatial information) changes the results compared to your control? Because that test actually is the difference between the current way of doing simulations and the control case here, and I'd consider that to be important. Or is this difference clearly seen in your results? If so, maybe you could clarify your message.

Section 3.1.: Do you have knowledge, how CLM4 is going in this respect? If you have same latitudinal pattern, then it would be due to CN-dyanmics, if it's different, then it would be due to other features. Just a thought, that it might be interesting addition, in case you have that information available.

l. 8, 16-18: And how much is the contribution from Sahel in Catchment-CN in that latitude? You could explain this difference in a bit more detail, it seems quite big GPP for Sahel region. How about comparing the results with the same land mask, would that be possible?

p. 8, l. 19-27: Could you make a table, where you compare your modelling results for the actual time periods of your references here? If I understand correctly, you had over 30 PgC difference in global GPP when adapting CLM to Catchment-CN. Is that really right? Because these revisions for CLM4.5 were then not adapted to Catchment-CN, or were they? If my first interpretation was right, could you comment, why you had such a change in your adaptation.

Section 3.2.: For the supplement plot I'd add GPP with MTE-GPP for the different seasons as a function of latitude also. This would highlight more, if the low northern hemisphere summer values are caused by respiration or gpp.

p. 9, l. 8: Why not make a subplot to Fig. 3, where you show annual cycles of GPP, respirations and fire? Now the seasonality of GPP is not really visible anywhere.

Section 3.3.: I find the way that FACE experiments have been used to "evaluate" the model performance a bit problematic, but maybe this is just a matter of more in-depth explanation by the authors. The authors claim here, that the NPP response for enhanced CO2 concentration is similar than with other models. But this is after several years of experiments, when other factors, such as nitrogen cycle come into play. If I've understood right, in this study the aim was to see how Catchment-CN responses to different CO2 fields and therefore I'd suspect the response of the model to CO2 responses in short time scales is relevant. E.g. Zaehle et al. (2014) mention that the NPP response of CLM4 (which is the basis of the Catchment-CN biochemistry) is too



low after the first year. It would be this response that is more relevant considering the aim of this study.

The comparison to FACE experiments is not properly described. Did you do site level runs, or did you just do a global model run with increased CO2 concentration and then take data from the corresponding catchment? And when did you increase the CO2 concentration? Beginning of 2001, which is the time of your simulation, or already on the last years of the spinup, when the actual FACE experiment starts at the sites? That makes a difference in the light of N cycle, that you have on your model. Also, the p. 13, l. 12-14 comment is only partly true, as in this longer time scale you are using in your evaluation also includes N cycle feedbacks. For the short time scales (& without stepwise large CO2 enhancement) it is true and actually for your aim also. I'd guess it's more a matter of how the stomates respond to increasing co2, i.e., much shorter time scale phenomena, that is important to your results, than what you're showing here.

p. 11, l. 14: What do you mean by this sentence? Usually in all modelling experiments annually varying co2 concentration would be used, it is not clear why this section 3.4.3. is relevant.

p. 11, Section 3.4.3: It is not clear for me, what was the motivation behind this test. Could the reason be explained somewhere more clearly?

Fig. 8.: The largest changes in regions seem to be in R5, where perhaps the actual GPP is not that big. Could you also say something, where the highest changes are in absolute GPP values. And these are now changes in respect to the control and not the preceding test simulation?

p. 14, l. 10: Can you explain this sentence about the 'rectifier effect' a bit more.

p. 14, l. 12-14: But unfortunately the current DGCMs have larger problems, having exactly right co2 concentration won't solve them. . .

p. 14, l. 15-20: Yes, this is quite basic knowledge, but usually this is connected to large

$CO_2$ changes (e.g. FACE experiments) or extreme events. It would be interesting to hear some further motivation, why you consider that your current model runs (where the changes in co2 conc are nevertheless quite modest) promising tool to be used in this direction.

Overall, this is a bit off from the scope of this study, but one also starts to wonder, what kind of influence the atmospheric transport model will have on LSM results. In this study CT results are used, so they are result of inversion, but would the authors consider it worthwhile mentioning, based on their results, what kind of errors are to be expected, when doing e.g. a coupled run with land and atmosphere models.

Minor comments

Many of the figures are having only unit instead of the variable name on the axis. I recommend adding the name, so that it's not necessary always to check that from the caption.

Table 1: Could you also have delta between the different tests and CTR?

Fig. 2: a & b) You're having the model output and MTE-GPP shown on different spatial resolutions. For a visual comparison, I'd show them on the same resolution.

p. 4, l.1: It would be interesting to know, how many PFTs you have available.

p. 4, l. 12: You could list the environmental variables effecting your photosynthesis calculation. Now the temperature dependence of Vcmax is not mentioned (that I'd suspect to be included).

p. 4, l. 20: Do you mean canopy temperature by the vegetation temperature? So, it's not air temperature, but you resolve for canopy temperature, right?

p. 5, l. 13: You could mention the range of m.

p. 21: And how is GPP tied to the photosynthesis? How do you upscale the photosynthesis for larger scale? And do you have considerations for photorespiration, or was



that considered already earlier?

p. 5, l. 27: You already mention this on p. 4, l. 3.

p. 7, l. 29: typo, averaging...

p. 8, l. 4-6: Why are you having this sentence here? I'd find a more logical place for it to be in the Methods and model description.

p. 9, l. 11: What does "dominating temperate or boreal forests" mean?

p. 9, l. 23: Why are you talking about fields?

p. 10, l. 12: "perhaps not a surprise" doesn't sound very scientific argumentation, I recommend rephrasing

Section 3.4: Why not add the experiment names to the section names? It would make it easier to follow.

p. 11, l. 11: typo, interannual

Fig. 8. In my version the longitudes are in middle of the map, it might look better that they were along axis (if they need to be shown here).

Fig. S3: Was this the concentration at the lowest atmospheric level? Maybe could be mentioned in the caption

---

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-187, 2018.