

Interactive comment on “The impact of spatiotemporal variability in atmospheric CO₂ concentration on global terrestrial carbon fluxes” by Eunjee Lee et al.

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Dear Reviewer,

Thank you for the June 6th correspondence. We appreciate the helpful comments. In responding to them, we feel we have greatly improved the manuscript.

We have carefully addressed your comments and suggestions. Please find below our responses to them, prefixed with an arrow sign (=>). The figures in the letter are labeled and numbered with “L” (attached as a supplement to this letter). The page and line numbers refer to the revised manuscript, if not noted otherwise.

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Sincerely,

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The authors have produced a generally clear and well written paper that describes a set of model simulations that are designed to investigate the importance of spatio-temporal resolution of CO₂ forcing for global terrestrial carbon cycle models. They find that increased CO₂ forcing resolution has little impact on global aggregate GPP and NBP, but may be important in some regions and seasons.

Overall the paper represents a valuable contribution to the field. However I do have some concerns, or some suggestions that could increase clarity. My main concern is the design of the experiments; where variability in space or time is reduced, from 3hourly spatially varying CO₂ to 390ppm CO₂ that do not vary in time or space. In this line of variability reduction, the middle step includes removing interannual variability (trend + annual anomalies around the trend). I think the paper could be more clear if it ends with what models are commonly forced with, global, annual CO₂ concentrations that changes between years. Subsequent reductions in variability could be reported also, but those are less interesting.

=> Thanks for the comment. We agree with the reviewer and have accordingly modified the experimental design (please see Figure L1 attached to this letter)

We performed a few additional simulations (maCO₂, magCO₂, and magtCO₂; the additional simulations are highlighted in red in the Figure L1 for the convenience). The magCO₂ uses the “annually changing co₂ concentration (without spatial information)”, which is a popular and conventional way to prescribe atmospheric CO₂ in many other LSM and TBM modeling studies. We split the interannual variability into two (i.e., annual anomalies and the trend) as suggested by the reviewer. The magtCO₂ case

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removes the high frequency component of the interannual variability but keeps the longer-term trend.

The differences in GPP and NBP produced in each experiment relative to that produced in the magCO₂ experiment (i.e., the one that applies the commonly utilized approach) are shown in Table 2. The results section (section 3.4) was also revised with the modified experimental design. Please see the revised manuscript for further details.

Page 7, line 23: it is not clear if global averages of CO₂ are preserved or not through the reductions. Interpolation of monthly means may change the sum of daily values (or 3 hourly). A clarification on this would be good.

=> In removing diurnal variations (from 3hCO₂ to dCO₂), there was no interpolation applied but simple daily mean values were used for every time step for a given model day. For 3hCO₂ and dCO₂, the global averages of CO₂ are conserved.

In removing daily variations (from dCO₂ to mCO₂), the interpolation to the monthly means results in a slight increase of the global average of CO₂ but the increase is very small (0.0009 %). Thus we consider the difference negligible. It was clarified in the revised manuscript (Page 7, Lines 31-32).

Minor comments:

Page 1, line 30, and continuing on page 2: sentence is unclear.

=> The part was revised as below (also see Page 1, Line 30 through Page 2, Lines 1-3 of the revised manuscript).

[Previous] Studies disagree on partitioning of the land carbon sink between the tropics and the extratropics, for example, tropical ecosystems as carbon sinks (Stephens et al., 2007; Lewis et al., 2009; Schimel et al., 2015; Houghton et al., 2015) or sources (Baccini et al., 2017).

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[Revised] For example, studies disagree on the partitioning of the land carbon sink between the tropics and the extratropics. Some studies consider tropical ecosystems to be carbon sinks (Stephens et al., 2007; Lewis et al., 2009; Schimel et al., 2015) and others consider them to be carbon sources (Baccini et al., 2017; Houghton et al., 2018).

Page 5, line 24 and throughout the paper: NBP is usually positive for a sink.

=> As suggested, the sign of the NBP now follows the convention (i.e., positive NBP means a carbon sink) in the revised manuscript. The signs of the NBP values in Figure 2, Equation 6 and in the text were revised accordingly.

Page 6, line 31: recycled instead of multiple loops? e.g. "with recycled 1981-2015 MERRA-2 forcing data"

=> For clarify, the part was revised as "consisting of repeated cycles of the 1981-2015 MERRA-2 dataset" (Page 7, Line 5).

Page 7, line 3: omit "simply"

=> The word was deleted.

Page 7 line 20: "every land surface element" is not clear

=> It was revised as "every tile" (Page 7, Line 27 of the revised manuscript). Please note that the tile structure was introduced and explained earlier in Page 5, Lines 16-18.

Page 8, line 17-18: Why not use the same mask for both datasets? Regridding may be needed.

=> We agree with the reviewer. In the revised manuscript, a regridded land mask (please see Figure L2 attached to this letter) was used for a better match to the MTE-GPP landmask. The numbers and Figure 2 were revised using the revised landmask. As a result, the zonal GPP in Figure 2c in the revised manuscript shows a better agreement.

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Page 9, first paragraph: Why zonal GPP evaluation and seasonal NBP evaluation?

=> A new figure showing seasonal GPP evaluation (Figure 2d) is included in the revised manuscript. We also included zonal GPP evaluations for DJF, MAM, JJA and SON (Figure S1) in the supporting information. Please see Sections 3.1 and 3.2 for further details in the revised manuscript.

Page 9, last row, “this turns out” could perhaps be expressed better.

=> We revised it as “These results are at the low end of the observations...”. Please see Page 10, Line 20 in the revised manuscript.

Page 10, line 12 “by the way, is perhaps not a surprise” could also be expressed better.

=> The sentence was deleted in the revised manuscript.

General; synoptic and daily are both used for the same reduction of variability, I recommend using one of to be consist

=> The word “synoptic” is replaced with “day-to-day” in the revised manuscript. Please note that there is one exception where the word “synoptic” remains (Page 11, Line 18) when it is used to describe the horizontal scale of weather, not to refer the temporal variability of CO₂.

Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2018-187/bg-2018-187-AC2-supplement.pdf>

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