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Interactive comment on "Role of CO_2, climate and land use in regulating the seasonal amplitude increase of carbon fluxes in terrestrial ecosystems: a multimodel analysis" *by* Fang Zhao et al.

Anonymous Referee #2

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Review of the article "Role of CO2, climate and land use in regulating the seasonal amplitude increase of carbon fluxes in terrestrial ecosystems: a multimodel analysis" by Zhao et al.

This article presents interesting results in the scope of BG. It can be published after the authors haven taken care of the minor issues stated below. Therefore, I've asked for minor revisions only. However, I'm convinced that the paper could be improved substantially by a small effort, if the discussion would be extended in the two directions I try to describe in the following:

The study has two parts. In the first part it is evaluated, if nine global vegetation models (from the TRENDY project) can reproduce the seasonal cycle of land carbon fluxes as derived from atmospheric inversions (figure 1 to 3). In the second part of the article these models are used to investigate trends in the seasonal cycle amplitude of land carbon fluxes. This is done by separating the contributions to the trends for three major forcing factors: rising atmospheric CO2 concentration, trends in climate, and land use/cover change (figure 5 to 9).

In view of the large uncertainties in land carbon cycle model results, I appreciate this study very much. Combining model evaluation and factorial analysis will probably provide a better understanding of the seasonal cycle carbon fluxes (as also mentioned by the authors in the introduction, page 3 line 20-21). Unfortunately, this potential is not utilized. The findings about the performance of each model are (almost) not mentioned in the discussion of the second part. For example, the results shown in figure 5 could be discussed in view of the evaluation presented in figure 1. Some questions like the following could be posed and answered: are the models that successfully simulate the seasonal cycle of carbon uptake more similar in the CO2 fertilization factor than the models that fail to reproduce the seasonal cycle? Or is this true for the climate factor? Analogously, figure 6 could be compared with figure 2.

Finally, we want to understand by such studies, why the magnitude of the seasonal cycle in atmospheric CO2 increased. Additionally, we want to confirm that vegetation models respond reasonably to global warming and increasing atmospheric CO2. The self-evident way to reach both aims is to evaluate models and then to analyse their results by taking into account this evaluation. The later step is incomplete in the current version of the text as the evaluation is not considered in the discussion of the forced simulation results.

Another important comment I would like to add concerns figure 10. This shows a moderate correlation between the change in net land carbon uptake and the increase in the amplitude of the seasonal carbon fluxes as simulated by the different vegetation mod-

els. The authors mention that this cross-model correlation may be used to constrain land carbon uptake (page 11 line 9). I think, this is the key motivation to investigate the seasonal amplitude of carbon fluxes and it should be also mentioned in the introduction. Furthermore, the authors claim for more research on observed CO2 fluxes and atmospheric transport on a regional scale to substantiate this finding (page 11, line 11-13). I agree, but the obvious next step is to further investigate the results of the model ensemble, how the correlation is simulated. A factorial analysis of the long-term carbon uptake should be performed and then compared with figure 5. Thereby, it could be specified, which factor contributes to what extent to the correlation. Probably this is beyond the scope of the article, but at least this opportunity should be mentioned. And, this kind of analysis is commonly denoted by the keyword "emergent constraint". It would be good to cite a reference, perhaps the paper of Cox et al 2013.

Minor scientific issues

- Why is the exceptional result of VEGAS concerning the CO2 factor mentioned in the abstract (page 1 line 32)? It is not a key finding.

- "is a good indicator of terrestrial ecosystem dynamics" (page 2 line 11). This is a too general statement. It does not help in the line of argument. I would skip it.

- It would be nice to mention in section 2.1 that the TRENDY model simulations are offline simulations driven by climate data (and other input like atm. CO2 concentration) and that the models are not coupled to general circulation models. Of course this can be deduced from appendix A, but it should be also clearly stated in the main text as it is important for its perceivability (e.g. the differences in the results between the models can not be due to weather noise).

- The authors assume that the models simulate the effect of CO2, climate, and land use "linearly" (page 5 line 16-22). I think, "linear" is missleading in this context. It is more about synergy terms. For example, the trend in S2 minus S1 includes the climate effect and the synergy of CO2+climate. Furthermore, I'm not convinced that the synergy

terms are all unimportant. Therefore, I propose to simply state that the climate effect and the synergy of CO2+climate together are called "climate" for simplicity in the rest of the manuscript. Without a discussion whether the synergy terms are negligible or not. And, of course, analogously for S3 minus S2.

- Please replace "Q10 value" (page 6 line 22) by "temperature dependence of heterotrophic respiration". Not everyone is familiar with this shortcut.

- Concerning the temporal trend in the seasonal amplitude in the late 90s (page 8 line 9): I can not deduce from figure 7 that half of the models exhibit a decrease, but it is obvious that the model ensemble shows an increase.

- The models agree in the general seasonal amplitude increase, but they disagree in the contribution of the climate factor as well as the land use factor to the seasonal amplitude trend (see figure 5). I think, this disagreement even in sign should be mentioned in section 3.3. In the following subsections this important fact may be overseen due to all the details stated there.

Typos etc.

page 1 line 25: replace "during 1961-2012 for its seasonal cycle and amplitude trend" by "for its seasonal cycle and amplitude trend during 1961-2012"

page 1 line 31: replace "is a stronger" by "is the strongest"

page 2 line 9: replace "of CO2 seasonal cycle" by "of atmospheric CO2 seasonal cycle"

page 2 line 25: replace "in understanding the contribution of various mechanisms" by "to disentangle effects of various mechanism"

page 3 line 1: replace "instead of dynamic vegetation models" by "instead of biases in dynamic vegetation models"

page 4 line 14: replace "A direct comparison with fluxes from process-based models are monthly" by "Fluxes from process-based models can be directly compared with

monthly"

page 4 line 29: replace "The seasonal amplitude of Mauna Loa Observatory or global CO2 growth rate and fluxes from model simulations and inversions are computed" by "The seasonal amplitude at Mauna Loa Observatory, global CO2 growth rate, and fluxes from model simulations and inversions are processed"

page 5 line 2: replace "as seasonal amplitude" by "to define the seasonal amplitude"

page 5 line 28: replace "was defined in Eq. (1):" by "is defined as:"

page 11 line 33: replace "models' mechanical difference" by "the different parametrisations of important processes in models"

page 12 line 21: replace "high latitude "greening" over high latitudes" by "high latitude "greening""

page 12 line 24: replace "to differ for different models" by "to differ between models"

page 27: replace "in the S2 experiment (changing CO2 and climate and land use/cover) substracted by trends in S1 (changing CO2 only)" by "in the S3 experiment (changing CO2, climate, and land use/cover) substracted by trends in S2 (changing CO2 and climate)"

Please proof articles. They are missing quite often.

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