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Interactive Comment

Interactive comment on "Continued increase in atmospheric CO_2 seasonal amplitude in the 21st century projected by the CMIP5 Earth System Models" *by* F. Zhao and N. Zeng

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My responses to the referee are listed below. Reviewer comments are in italics.

The increase of CO2 seasonal cycle in the northern hemisphere has drawn considerable interests in the recent times although the attribution of causes remain unclear. This paper explore the affect of terrestrial biosphere fluxes and its change with time in relation with climate change. The topic is of interests to the earth science community. The manuscript is generally well written, but I have some concerns which should addressed before accepting for final publication in ESD.

Many thanks for your constructive comments. In our revised manuscript, we have added six figures, a new discussion section and many other changes. Please see our final response for a list of major changes, and we hope they can address your concerns. After our work in the revision process, we are now even more confident in our original main conclusions, and are excited about our new findings. We hope you find the revised paper interesting and informative.

p780, l13-15: Why only "temperate and boreal"? Recently Wang et al. (Nature, 2014) suggested the tropical biosphere also respond to climate change. One might think that the terrestrial biosphere in the tropics will respond to the climate change in one direction only. But given that there is dry and wet seasons in tropics (analogical with cold and warm temperature in the temperate/boreal region?) we might expect some seasonally sensitive response to the CO2 flux. And there are changes in seasonal atmospheric transport and magnitude of fossil fuel emissions. I am not suggesting you to solve all of these problems in this manuscript, but some thoughts should be interesting to discuss.

Thank you for pointing out the importance of tropical biosphere. Previous analyses on CO2 seasonal amplitude change mostly focused on the temperate and boreal regions. Our analyses are however performed globally. Our figure 4 shows the spatial pattern and latitudinal distribution of ensemble NBP amplitude change. The zonal mean –NBP change plot does suggest some seasonally sensitive response from the tropical/subtropical regions. However, the tropical area's net effect on global NBP amplitude increase is small: tropical regions north and south of equator seem to cancel out (showing yellow and green shades, respectively).

We realize that we have not fully explained this issue, and our original Figure 5 (now Figure 4) may be confusing since tropics has a different seasonal cycle phase which is not reflected in our figure. Thanks again for your helpful comment, we have now largely expanded the original section (now section 3.3) and added two figures (Figure 5 and Figure S1), and we hope you find the revised version informative.

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We have cited Graven et al. (2013) in our original manuscript, whose analyses suggest atmospheric transport and magnitude of fossil fuel emissions may only contribute a few percent to the CO2 amplitude change. We also added CO2 detrended seasonal cycle of each individual model and their ensemble alongside with NBP seasonal cycles in Figure 3.

p782, l25: Need a reference here? e.g. Peylin et al. (BG, 2013)

We have added the suggested reference, thank you!

p784, l23: Clarify whether the seasonal amplitude calculation is applied to zonal mean or individual grid or global?

We have used two definitions of seasonal amplitude and one approximation in our text. The first one is computed from CCGCRV package, which we use only for average CO2 or total NBP over global and (NEW) large regions in Figure 2 and 9, and for (NEW) Mauna Loa and Point Barrow CO2 (Figure 9,10, S4,S5).

Our second definition is simply maximum minus minimum of seasonal cycle in each year/decadal average, especially if we compare the 1961-1970 and 2081-2090 periods. Examples are global/regional mean/total (Figure 3) or zonal total (Figure 5). These first two definitions are quite close for analyses of monthly values.

We also have one approximation of global seasonal amplitude change, using the difference between the changes of dormant season (October-December) averages and peak growing season (May-July) averages. This approximation is aimed at examining the spatial and zonal contribution to global NBP amplitude change only. While the amplitude of this approximation is smaller than those of the two amplitude definitions, the spatial patterns should be highly related. We believe this is a reasonable approximation judging from Figure 3c.

Hope you find this explanation helpful!

p785, l25: How is the trends in FFE is considered?

We focused on the trends of NBP in our study. For FFE, we stated this in our original Methods section: “Previous studies have limited the impact of FFE and FOA on trends in CO2 amplitude to less than a few percent change (Graven et al., 2013). Therefore we focused on examining the seasonal cycle of NBP in this study.” Our comparisons in Figure 2 and 3 seem to support that NBP change is the main driver for trends in CO2 amplitude.

p785, l2 : Not clear how the different fitted components are utilised here?

A detailed description that we cited is http://www.esrl.noaa.gov/gmd/ccgg/mbl/crvfit/crvfit.html, which did a superb job in explaining the algorithms. For our study we just use the amplitude and detrended seasonal cycles (which we use to calculate the amplitude based on the means of two periods) computed by the CCGCRV package.

p786, l12: Will some of the differences between GFDL and CanGCM arise from the method employed for NBP calculation.

The NBP given by the models all represent the net carbon flux from atmosphere to land. We have carefully checked the global carbon budget of our equation (1) to make sure the models are self-consistent (which we stated in our Methodology section).

p786, l25ff: What if some models have peak uptake outside the months from May to July? please comment how is that going to affect your analysis using the ensemble means.

Only CanESM2 has peak uptake outside May-July (please see our new Figure 3, showing the seasonal cycles of individual models). Following your suggestion, we excluded it and BNU-ESM (for which October-December is not dormant season) and got very similar spatial patterns.

p786, l27: I am glad that you spelled this out. I am most curious how the model's behaviour differ depending on the differences in timing of the seasonal minima? Then of course taking 3-monthly mean may also have some implications for the model to

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model comparison.

The models in general are quite consistent in timing of the seasonal minima (except for CanESM2), and all models have their minima in June in the future period (2081-2090). We use the difference of 3-monthly means to determine the relative spatial concentration. From the graph we showed in figure 3c, this selection is reasonable for most models. We have also tried to just use June and October, and our main conclusions still hold.

p787, l10: Replace minimums and maximums by minima and maxima, respectively, throughout the manuscript

We have made the suggested changes throughout the manuscript.

p787, l19: This package must be calculating peak to trough monthly values. Do you think these results are compatible with the three monthly means stated at the end of the methods section?

Yes, the amplitude calculation in this package yields similar (smoothed) results compared to using peak to trough values directly. If computing amplitude based on the three monthly means, the amplitude will be smaller, but the main features in the spatial patterns should be similar (we have tested).As we are only concerned about the relative spatial and zonal contribution instead of the absolute amplitude, we believe our method is reasonable.

p787, l24ff: Given the state-of-the-art/quality of the ESSMs I have reservations about the utility of this result. Please clarify. For example when the models do not represent the past well, what should be our take home message for the future projections.

In the case of CO2 seasonal amplitude change, we compared model results and surface observation, in which we found the model ensembles actually represent the amplitude change in the historical period well for both Mauna Loa and Point Barrow. This and the high model consensus on relative amplitude increase of global CO2 and NBP

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suggest the models have some skills.

Even so, when we closely examined the mechanisms, the current representation in CMIP5 models are probably not realistic. We have extensively discussed this interesting issue in our new discussion section, and advised extra caution when treating the results, which only holds if the mechanisms and future emission scenarios are correct. Specifically, we cited our recently accepted paper in Nature (Zeng et al., 2014) on a major missing mechanism-agricultural intensification, and we presented the change of ecosystem types, which is also mentioned by Graven et al. (2013), in our new Figures 11 and 12. We hope our findings allow us to understand more clearly the model deficit, the mechanisms, and uncertainties. We hope this paves a way forward for future model development and validations, and eventually better future projections.

p788, l2ff: Not for all models! Do you mean model ensemble? This itself is not any interesting, unless you say something why some models do differently from others. If the model-to-model differences can be linked with the model settings or parameterization, this paper will be of great significance. At least some attempts should be made.

Yes we meant model ensemble, and we have reflect that in our revised text. Thank you for your suggestion, we have now performed a lot of new analyses and showed spatial maps for each individual models (Figure S2, S3, S6-9) in our revised manuscript. We have also use the results of three models of sensitivity experiments to show model-to-model mechanisms, two of them included as new figures, and one attached here. In addition, we discussed on individual models and cited findings from previous literature (Anav et al., 2013; Arora et al., 2013) throughout the text. We hope the additional material in our revised manuscript is interesting to you and potential readers.

Section 3.2: Suggest deletion of Fig. 3. Looks nice but unnecessarily complicated. Since figure 4 essentially has the same information I recommend deleting. Some the text from this subsection can be moved to the next subsection.

We agree that Fig. 3 is unnecessarily complex. In our revised manuscript, we deleted

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Fig. 3 and merged the essential contents into Section 3.1.

p790, l19: What if you excluded MRI-ESM1 or MIROC-ESM model? This is one of example where statistics are more often misleading than convincing.

Indeed, with only ten models, the statistics should be treated with extra caution. In our revised manuscript, we added the following statement in section 3.5:

"Note that this result is based on the 10 models we analyzed; it is subject to large uncertainty and may change substantially with inclusion or exclusion of certain model(s)."

p790, l23ff: Without doing further analysis/discussions of the drivers of these changes in models, I doubt that this manuscript is a useful contribution. At least bring some discussions about the temperature, rainfall, and land cover change between 1961 and 2080.

Following your suggestion, we have included a new section (3.4) in our Results, analyzed GFDL-ESM2M, IPSL-CM5A-LR and MIROC-ESM results from sensitivity experiments. We have also included discussions on climate and land cover change between historical and future periods (Figure S6-S10, and related text in Discussions). We also cited Brovkin et al. (2013) for land cover change.

p791, l5: This "possibly" can be avoided if you just bring in temp, pcp and luc for analysis

We have now presented the soil temperature, soil moisture and crop area change between historical and future periods in our analysis. We deleted this sentence in our new conclusions section, because it is not part of our major conclusions. The interannual variability in the time series is another interesting issue, but it is not the focus of our study.

p791, l25: May be add "crop cycle" too here

As we discussed in our new discussion section, for CMIP5 models, land cover change

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is unlikely to increase the seasonal amplitude of CO_2. Simulating the effect of agricultural intensification on crop cycle is an aspect that we would like to see improved in future versions of the models.

p792, l14ff: Is this para needed?

Thank for your comment. We have deleted this unnecessary paragraph.

Figure 1: should the units for panel b) be ppm/mo, unless you converted ppm to CO_2 burden. Please check

We have indeed converted ppm to CO_2 burden, as we originally described in method section. We have now also added relevant description in Figure 1's legend to prevent confusing the readers (Thank you!).

Interactive comment on Earth Syst. Dynam. Discuss., 5, 779, 2014.

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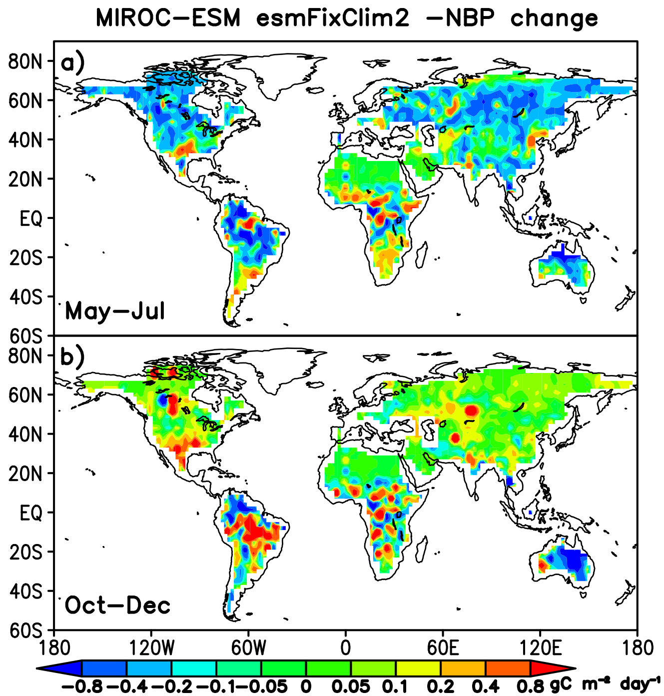

Fig. 1. Spatial patterns of MIROC-ESM −NBP changes from the esmFixClim2 experiment.

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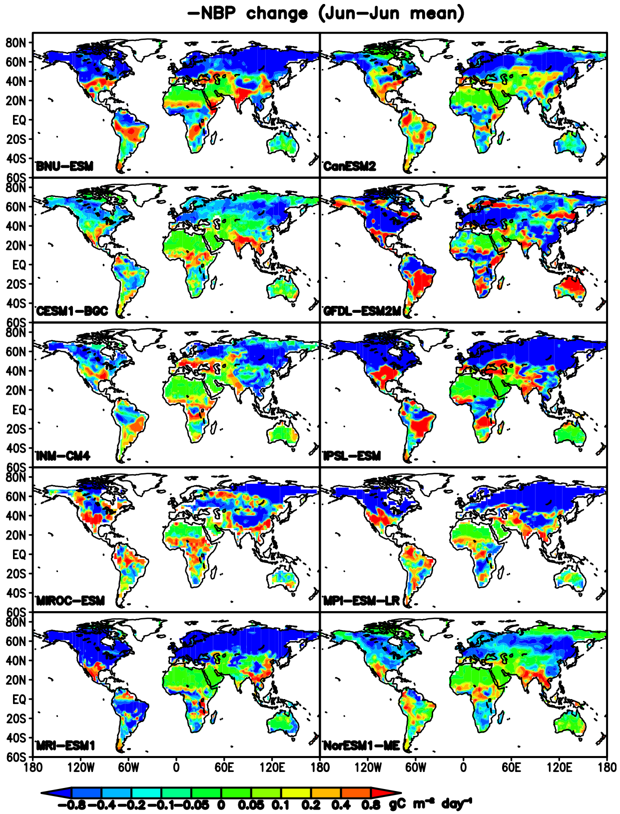

Fig. 2. Spatial patterns of -NBP changes in June for the 10 models.

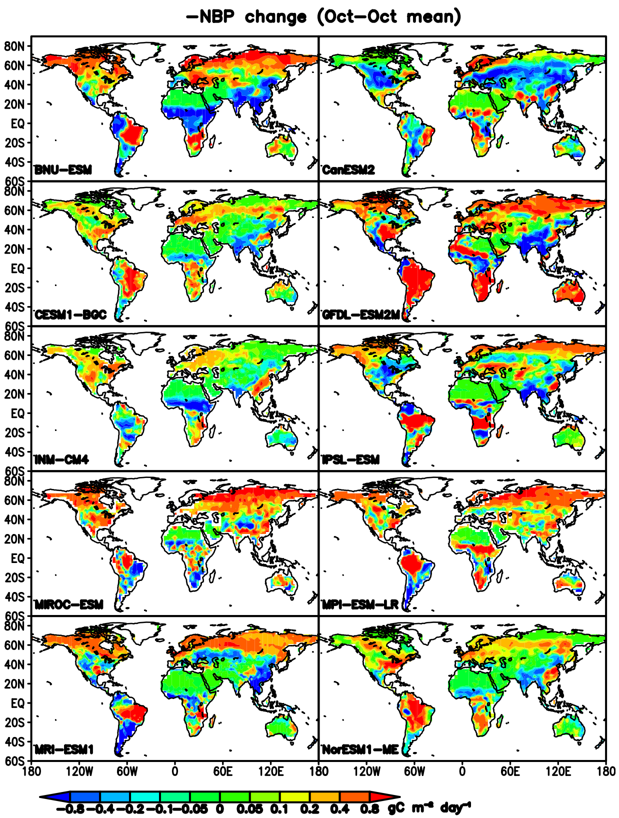

Fig. 3. Spatial patterns of -NBP changes in October for the 10 models.

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