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Earth System Dynamics
Discussions

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.

This paper examines emission-driven simulations of terrestrial and atmospheric CMIP5 models to assess future trends in seasonal amplitude of atmospheric CO2. The topic is interesting and timely but the paper's contribution to understanding terrestrial biosphere processes and modeled responses to environmental change is marginal, mainly because the description of results does not penetrate deeply into the findings and largely treats the models in aggregate.

Thank you for your helpful general statement. In our revised manuscript, we have done

a number of new analyses. As a result, we largely expanded the original result section, and added a new discussions section. We hope you find the revised paper improving the aspects lacking in the original manuscript.

Aggregate patterns may be dominated by only a few models, particularly since the seasonality varies considerably between models (up to a factor of 3), and aggregate patterns may be different from individual model responses. It would be useful therefore to describe in more detail the results for individual models, also looking at the factorial experiments for those models where it is available.

In addition to presenting the ensemble model results, we now show curves (new Figure 3 and 5) and spatial plots (Supplemental figures) of individual models, and discussed the model behaviors at numerous places. We find that for the CO2 and NBP amplitude increase, our original conclusions based on model ensembles still hold for most of the individual models. Even though the magnitude of seasonal cycle can vary by a factor of 3, the phase is similar for most of the models. Following your suggestion, we also analyzed results from two sensitivity experiments for the GFDL and IPSL models, and presented in the new section 3.4 discussing the mechanisms for NBP amplitude increase. Hope you find the new results informative.

The statements on P792 about the inability to draw meaningful conclusions from 4 models and the inability to separate carbon and climate effects from factorial experiments are false. Even though the factorial experiments use different emissions than RCP8.5, they are still useful for examining mechanisms in the models. Several impactful papers have made these comparisons with 4 or fewer models.

We agree with your opinion. In our case, only three of the ten modeling groups have provided NBP results for both of the factorial experiments (esmFdbk2 and esmFixClim2), and CanESM2 cannot reproduce the correct phase of NBP seasonal cycle (discussed in Anav et al. 2013). In addition to our new section 3.4 which displays GFDLand IPSL results from sensitivity experiments, here we also attach the NBP spa-

tial patterns from MIROC-ESM (Figure 1), which has strong climate-carbon feedback (Arora et al. 2013). Comparing to the widespread response of this model in Figure S2 and S3, this figure presents no obvious increase in net carbon uptake during peak growing season for Tibetan Plateau and Russia, and no obvious increase in carbon release during dormant season for most of the Northern high latitudes (albeit under a weaker carbon emission increase condition). This result indicate climate change do play a big role in the global CO2 and NBP amplitude increase of this model. Unfortunately, we do not have the results of the model to explicitly show its response under constant CO2 and changing climate.

The authors should look more specifically at Northern Hemisphere fluxes and near-surface CO2, where the trends in seasonal amplitude have been observed. If the global increase in CO2 amplitude is 70% to 2100, what does that mean for temperate and boreal fluxes and for Barrow CO2?

Thank you for the suggestion! We think these are very interesting questions, and we added new comparison at MLO and Pt. Barrow for model-observation CO2. Our findings and comparison with the work by Graven et al. (2013) now comprises a big part of the new discussion section. In addition to the global CO2 and NBP relative amplitude change presented in Figure 2, we also repeated the calculation for 25-90N, which shows a higher increase (81$\pm$46% and 77$\pm$43% for CO2 and NBP, respectively).

It should be explained why the HadGEM model is omitted, particularly since it has been shown to have strong carbon-climate feedbacks, and its simulations showed the largest change in CO2 amplitude in Graven et al 2013. Including this model would likely reduce the correlation in Figure 6 since it has a relatively small sink but relatively large change in seasonal NBP amplitude.

We are very curious about the results from the HadGEM model as well. We believe the analyses in Graven et al. (2013) are from CMIP5 Experiment 3.2, which is driven by CO2 concentration instead of emission. For the emission-driven runs examined in

our study, unfortunately both CO2 and NBP are not available for the HadGEM models. In an earlier stage, we have contacted colleagues from the U.K. Met Office, and Dr. Liddicoat kindly explained why the HadGEM team chose not to provide NBP for ESM simulations. The diagnosed carbon flux from HadGEM landuse component is not coupled with the atmosphere module (using the land use flux from Houghton et al. 2008 instead). Therefore, even if we calculate NBP from NPP, Rh and the Land use flux ("purely diagnostic, and different from what the atmosphere sees"—personal communication with Dr. Liddicoat, Feb 6, 2014) they provided, it is not the same as net atmosphere-land carbon flux in the other models. After careful consideration, we decided it is important to keep consistency in our comparisons and omit the HadGEM model.

For Figure 6, by presenting the cross-model correlation, we were trying to show CO2 fertilization is possibly a major mechanisms for global CO2 and NBP amplitude increase in many of the 10 models. 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)."

It is interesting that the simulations appear to predict decreased seasonality in Northern tropics and subtropics, similar to the result in Graven et al 2013, but the description of this figure is not clear and it seems that it may not account for regional or interhemispheric differences in NBP phasing.

Sorry for the confusion in our original Figure 5 (now Figure 4) and its description. This figure shows that the tropics contribute negatively to the global total -NBP seasonal amplitude (October maxima minus June minima) increase, which is not to say the seasonality in the tropics has decreased. This is because tropics has a different seasonal cycle phase, which is not reflected in our figure. Thanks again for your comment, we

have now largely expanded the original section (now section 3.3) and added two figures (Figure 5 and Figure S1). We discussed the difference of the zonal figures, and presented results for individual models and for regional aggregates. We hope you find the revised version informative.

The comparison with previous work is lacking in general and particularly in the Discussion where only three references are cited. A disproportionate part of the Introduction is given to early work rather than the current state of knowledge. The writing needs to be improved overall. The text is also imprecise in several places, for example referring to the amplitude but not specifying the amplitude of what, or referring to the amplitude but meaning the amplitude change.

We have largely expanded the discussion section and included several insightful discussions from recent work including Wang et al. (2014), Anav et al. (2013), Arora et al. (2013) and Brovkin et al. (2013). Starting from the abstract, we corrected the text in many places, trying to be precise about the information we want to convey. We hope you improved clarity in the revised version.

A major revision is needed to improve the presentation and to develop the scientific insights that can be gained from this analysis.

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. 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.

P781 L5 Amplitude figures are outdated

Thanks for pointing this out. We have updated the amplitude figures to 6.5 ppm (instead of 6 ppm) for Mauna Loa, and 17 ppm (instead of 15 ppm) for Point Barrow, using 2004-2013 average amplitude computed by the CCGCRV package.

P782 L5 Need reference for decreasing trend at Barrow

This sentence comes from our interpretation of Figure 1A in Graven et al. (2013). We now feel it is not necessary and removed it from the manuscript.

P782 L8 Graven 2013 gives 15% and 35%

We derived the figures from Figure 1A. We now change the description to: "The latest analysis shows a 0.32% yr^{-1} increase in MLO amplitude and a 0.60% yr^{-1} increase in Point Barrow (Figure 1A, Graven et al., 2013). Over a 50-year period, this corresponds to an increase of 16% and 30% in MLO and Point Barrow CO2 seasonal amplitude, respectively."

P782 L17 It is unclear what amplitude this paragraph is referring to, CO2 or NEP? Randerson et al 1997 and Gurney and Eckels 2011, at least, should be cited and described for NOAA data and CO2 inversion trends.

We have modified the text to clarify that the amplitude this paragraph refers to CO2 amplitude. We have cited results from Randerson et al 1997 in this paragraph, and Gurney and Eckels 2011 when we discuss relevant results in Section 3.1.

P783 L1 Add ", in comparison to Mauna Loa CO2 observations," after "for four TBMs" P783 L8 Replace "might underestimate the amplitude" with "underestimate the CO2 amplitude change in the mid-troposphere at latitudes north of 45N".

We have modified the text as suggested, thank you!

P783 L22-25 There are grammatical errors in all of these questions.

We have corrected the questions, now it reads: "How do CMIP5 models predict the amplitude and phase changes of CO2 seasonal cycle in the future? Are the changes mostly driven by changes in production or respiration? Where do the models predict the largest amplitude changes will occur?"

P790 L15 Need reference Early work by Hall 1975 (cited in our section 1) suggested

the role of CO2 fertilization effect. Now with the addition of section 3.4 on mechanisms, we decide to start the section with our own findings, and modify the sentence to: “Our analyses above suggest CO2 fertilization effect is a major mechanism causing the amplitude increase in some models. If it is important in most models, we expect to see models with a larger change in mean carbon sink simulate a larger change in seasonal amplitude.”

P792 L1 Either this work should be included in the analysis or the paragraph deleted

We have included the suggested new analyses and moved the description of the sensitivity experiments to our new section 3.4.

P792 L14 Delete paragraph.

We deleted this paragraph as suggested. In its place we added suggestions for future improvements.

Figure 2 – There are two lines of the same color

We have redesigned the color scheme for some of our plots. Specifically we have updated Figure 2 and 8 (old Figure 5) for clearer presentation.

Figure 3 - Not clear. Why are there more than two red and black lines in each panel? Why are the time periods, particularly the future period (2006-2099), so long?

Sorry for this unnecessarily complex figure. In our revised manuscript, we deleted Fig. 3 and merged the essential contents into Section 3.1.

Figure 5 - Right panel is not explained. Does this account for different phasing of the NBP cycle in different regions?

We have reorganized and expanded our description, both in the figure legend and the main text. This figure does not account for different phasing, which is presented in our new Figure 5 and Figure S1.

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

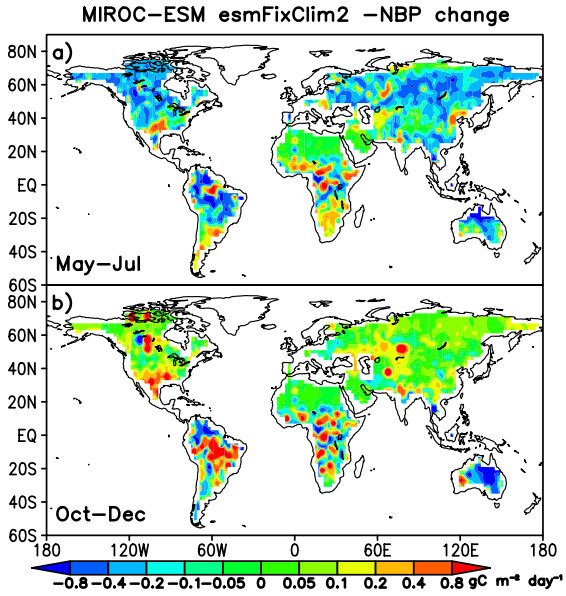

Fig. 1. Spatial patterns of MIROC-ESM −NBP changes from the esmFixClim2 experiment.