Interactive comment on "Differences in carbon cycle and temperature projections from emission and concentration-driven earth system model simulations" *by* P. Shao et al.

P. Shao et al.

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We appreciate the Reviewer #3's comments that help us to clarify our presentations and motivate us to do additional analyses with new insights.

Major comments

1. *The results for esmrcp85 experiment have already been analyzed by Friedlingstein et al. (2014) and Hoffman et al. (2014) and I am trying to find exactly what new science does this manuscript contributes. One new idea that is put forward by the authors is the use of ln[CO_2] instead of [CO_2] in calculating the temperature-[CO_2] feedback parameter that was introduced by Friedlingstein et al. (2006). Authors also suggest that ln[CO_2] instead of [CO_2] should be used in the carbon-climate feedback analyses. While both these suggestions make some sense, the rest of the manuscript somehow doesn't appear to be the right medium to convey this trivial point since the authors do not themselves perform any carbon-climate feedback analyses. The fact that the additional [CO_2] spread in emissions-driven RCP 8.5 scenario leads to higher uncertainty in simulated temperature change for the RCP 8.5 scenario for year 2100 has already been highlighted in Friedlingstein et al. (2014). Besides, the manuscript does not appear to present any comprehensive or new analysis so as a reader I haven't learned anything new.*

Reply: Following three reviewers' comments, we have done additional analyses (see our Reply to Reviewer #1), and new insights can be summarized as follows:

(1) Increase of global temperature range in E-driven simulations in 2100: (a) The global temperature range increase by 49% (as computed in the original manuscript) is

consistent with the finding of Friedlingstein et al. (2014), although our eight models and their eleven models share just six models. This further demonstrates the robustness of this conclusion. (b) The land and ocean temperature ranges increase by nearly the same: 42.3% over land and 41.8% over ocean, but both values are less than that for global temperature (48.5%). (c) In contrast to the statistically significant correlation (R) between model differences in 2005 versus in 2100, with $R^2 = 0.62$ in Figure 3 of Friedlingstein et al. (2014), we found $R^2 = 0.35$ in our calculation, which is not significant at the 0.05 level. Furthermore, R^2 becomes much smaller (0.10) for model differences in 1980 versus in 2100. This implies that, for the eight ESMs analyzed in our study, model [CO_2] biases during the historical period may not be a good predictor of the model differences between simulated and prescribed [CO_2] in 2100. Both points (b) and (c) are new.

(2) Seasonality of [CO_2]: (a) Evaluation of the seasonality of [CO_2] suggested in our study would be complementary to other evaluations of the carbon cycle. (b) The [CO_2] seasonality is correlated to α and α'. Point (b) is new.

(3) α and α' terms: we have (a) emphasized the use of α and α' as a diagnosis of the climate response to CO_2 change, (b) deleted the sentence on the γ term, and (c) emphasized the need of using (linear) α, rather than (nonlinear) α', in the carbon-climate feedback formalism of Friedlingstein et al. (2006). Point (a) is useful for ESM evaluations.

2. *In addition, the manuscript would benefit significantly from basic equations that would introduce the reader to how concentration- and emissions-driven simulations actually work.*

Reply: Following this suggestion, we have: (a) added the [CO2] mass balance equation; and (b) discussed how C-driven and E-driven simulations actually work. Also see our Reply to Major Comment #1 of Dr. Jones (Reviewer #1).

3. *Finally, it appears the annual cycle of [CO_2] for the CanESM2 model is calculated incorrectly or the data downloaded from the CMIP5 archive and used by the authors was corrupted. The correct amplitude of the annual [CO2] cycle from CanESM2's third ensemble member of the esmHistorical experiment is shown in Figure 1.*

Reply: To double-check our results, we re-downloaded the output files from both the CMIP5 portal and the website of Environment Canada (http://www.cccma.ec.gc.ca), and found that our results in the original manuscript were correct. Through extensive explorations, we finally found the probable reason for the discrepancy between the seasonal cycle of [CO_2] from CanESM2 in our Figure 2a versus that in the reviewer's Figure 1: our Figure 2a shows the global mean column atmospheric CO_2 (computed from the CO_2 mass in the atmosphere) with the seasonal amplitude consistent with the value reported in Zhao and Zeng (2014; their Table 2), while the reviewer's Figure 1 most probably shows the seasonal cycle of surface CO_2, as the seasonal amplitude (3.34 ppm) from 1991–2000 is close to the amplitude of surface CO_2 (3.24 ppm from 2001–2005) reported in Zhao and Zeng (2014; their Table 4). For our discussion, it is more appropriate to use the column atmospheric CO_2 than the surface CO_2.

The other comments

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1. *Line 2, Abstract. "… using all eight Earth System Models…". What does "all" mean here. Did only eight ESMs contribute to CMIP5.*

Reply: By the time of our analysis, there were only eight CMIP5 models directly uploading the outputs of C-driven and E-driven experiments under both historical and RCP8.5 conditions. However, for some ESMs, some variables (in most cases, NBP) can be derived from other relevant model outputs. Our work didn't include these models. We have removed the word "all" to avoid confusion.

2. *Line 6, "…differences in 2100 vary from 19.7 to +207.3 ppm in emission-driven ESMs". Please be explicit and say difference compared to concentration driven runs.*

Reply: Done.

3. *Lines 12-14, "… while the [CO_2] seasonality is simply neglected in concentration driven ESMs, suggesting the urgent need of ESM improvements in this area." Yes, the CO_2 annual cycle is important but why is it urgent. In my mind, the largest uncertainty in ESMs is how the terrestrial carbon cycle responds to increasing CO_2.*

Reply: Following this comment and that from Dr. Jones (reviewer #1), we have added sentences to clarify: (a) processes that dominate the seasonality of [CO_2], such as terrestrial productivity and respiration, have been analyzed before, including in our recent studies (Shao et al. 2013a,b); (b) observational data on the carbon cycle (e.g., terrestrial productivity and respiration) have much larger uncertainties than the [CO_2] observations; (c) evaluation of the seasonality of [CO_2] suggested in our study would be complementary to the other evaluations of the carbon cycle, and (d) delete the word "urgent."

4. *When reading the abstract a reader does not know how α is different from α'.*

Reply: The sentence has been revised.

5. *Lines 24-25. "This partitioning can be simulated using Integrated Assessment Models (IAMs) (Van Vuuren 25 et al., 2011a) or sophisticated earth system models (ESMs)." What about observations, don't they tell us anything about this partitioning?*

Reply: We have revised the sentence to include "…or from observationally inferred estimates (e.g. Hoffman et al., 2014)."

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6. *Lines 7-8. "IAMs were used to provide the atmospheric carbon concentration ([CO_2]) trajectories that were then used by climate models to assess the physical science basis of climate change". Replace "carbon" with "carbon dioxide". The later part of the sentence seems to come straight from the IPCC web site and thus reads a bit weird.*

Reply: Done.

7. *Lines 16-18. "The diagnosed cumulative emissions are mostly contributed by the carbon-concentration feedback that is about 4.5 times larger than the carbon-climate feedback (Arora et al., 2013)." This sentence seems totally out of place and does not follow from the sentences before.*

Reply: This sentence has been deleted.

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8. *Lines 26-28. "the separation of the carbon–climate and carbon–concentration feedbacks requires additional simulations (e.g. the simulation with 1% increase of [CO_2] per year; see Arora et al., 2013)." 1% per year increasing CO2 simulation by itself doesn't allow to calculate the two feedbacks but rather its fully, biogeochemically and radiatively coupled versions do.*

Reply: The sentence has been revised.

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9. *Lines 25-26. "... and Arora et al. (2013) gave an initial analysis of the CMIP5 emission driven runs with a brief introduction to some of the ESMs used here". NO, the Arora et al. (2013) manuscript analyzes concentration-driven simulations.*

Reply: The sentence has been revised.

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10. *Lines 8-9. "The emissions of other greenhouse gases and aerosols are treated as externally specified in both types of simulations.". In CO2 emissions-driven simulation, generally concentrations of other GHGs are specified together with emissions of aerosols.*

Reply: The sentence has been revised.

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11. - *Lines 9-10. "The accumulated differences by 2100 over land are close to or slightly greater than those over ocean in all ESMs (Fig. 3b and c)" What do Figures 3b and c has to do with this sentence which appears to talk about carbon.*

Reply: "Fig. 3b and c" has been replaced by "Fig. 1b and c", as we referred to Fig. 1b and c.

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12. *Lines 9-11. "Based on mass conservation in the earth system, the total anthropogenic carbon emission can be computed as the sum of changes in the three carbon pools (atmosphere, land, and ocean)." Equations always help. The manuscript, in its current form, does not include any equations that would introduce the reader to the basic C budget.*

Reply: Following the suggestion from three reviewers, we have added equations for E-driven and C-driven simulations.

13. *Lines 21-22. "…while the large positive differences in CanESM2 and CESM1-BGC are due to their higher climate-carbon cycle feedback than the IAMs...". Something is amiss here and this doesn't seem to be the right justification. CanESM2 and CESM1-BGC have very different carbon-climate feedbacks because CanESM2 doesn't have an N cycle coupled to its C cycle, where as CESM1-BGC does.*

Reply: The sentence has been revised to include "…and due to their different treatments of the carbon cycle, such as the inclusion (or omission) of the N cycle in CESM1-BGC (or CanESM2) (Shao et al. 2013b)."

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14. *Line 21-22. Please reword the vague phrase - "The global warming spreads from 3.11 to 5.47K…"*

Reply: Done.

15. Lines 22-23. Please reword this sentence or break it into smaller chunks. It is difficult to follow this sentence.

Reply: Done.

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It is unclear how authors have calculated their α in equation (1).

Reply: We have clarified it: α is computed through a linear regression in Eq. (1).

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