Biogeosciences Discuss., 12, C4151–C4155, 2015
www.biogeosciences-discuss.net/12/C4151/2015/

Biogeosciences
Discussions
Open Access

Interactive comment on "Earth system responses to cumulative carbon emissions" *by* M. Steinacher and F. Joos

Anonymous Referee #4

Received and published: 8 August 2015

Review of "Earth system responses to cumulative carbon emissions"

This paper addresses the issue of linearity/non-linearity in the response of the climate system to cumulative CO2 emissions – with potential implications for the discussion of carbon budgets for reaching specific climate targets. Further, this study infers probabilistic estimates of climate sensitivity and of the transient climate response. For the purposes of this study, the authors have performed large model ensembles with an Earth System Model of intermediate complexity in a Monte-Carlo framework. To capture uncertainty, they consider a spread in forcing scenarios as well as in model climate system responses by perturbing key model parameters. To constrain their model output they have used a large set of differing observational constraints. While the linearity of climate response to cumulative emissions has been also investigated in other stud-

ies, the authors add new variables to the discussion, i.e. surface ocean pH, calcium carbonate saturation states, and soil carbon. My general impression is that the paper addresses important research questions and delivers interesting information on the performance of a large model ensemble under various emission pathways. Yet it could benefit from putting more focus on the discussion of certain model aspects (e.g. the issue of linearity) rather than more describing simulation results.

Specific comments: - Given the focus of this work, I am missing a more in-depth discussion of non-linearities in the model response. The authors discuss non-linearities mainly in the context of the impact of non-CO2 forcings and negative emissions. I consider it crucial to underline that the model used may not simulate non-linearities due to model limitations. Only a short comment in the Discussion section is made about potential model limitations. I think the paper would benefit from discussing (as far as feasible) non-linearities which are not captured by the model due to (needed and justified) simplifications in the model design (e.g. regarding the model description of carbon cycle feedbacks). Although to a lesser extent, the aspect of incomplete description of Earth System feedbacks is also an issue for state-of-the-art complex climate models (e.g. for permafrost-carbon feedbacks).

- The authors underline that they add new variables (surface ocean pH, calcium carbonate saturation states, and soil carbon) to the discussion of linearity in Earth System responses. Yet in the manuscript these climate variables are only shortly discussed when describing the simulated model simulations results (Figs. 2,5,6). A few implications of those results would be interesting to discuss. Also in this context: A description of what "surface aragonite saturation state" is and why it is of interest would be a helpful information to include into the manuscript.

- Section 2.4 and figure 4: To me it is not obvious that the issue of total vs. fossil-fuel only carbon emission is a key point for this study. I probably would put this discussion into a supplementary information section. If kept in the main manuscript, Figure 4 should be discussed in more detail (in the current manuscript there are only two short

references made to Figure 4).

- Additionally, in a supplement a figure could be shown illustrating the various emission scenarios used in this study (showing the temporal evolution of CO2 emissions).

- Fig. 2c: An improved explanation would help better clarifying the meaning of Fig.2c for interpreting modelling results.

- Forcing design: Is the assumption of constant CO2 concentration and radiative forcing after 2150 made in all scenarios? E.g. RCP8.5 stabilizes only after 2200. Then the labels in the figures should be modified accordingly (the now labelled "RCP8.5" scenario differs from the conventional RCP8.5 scenario) and the figure legend should hint to the stabilizing at 2150.

- Constraining of model responses: I wonder to what extent uncertainty in aerosol forcing is captured in the performed model simulations. Uncertainty in aerosol forcing is a key factor when observationally constraining climate sensitivity and transient climate response. I understand that the ensemble describes various emission scenarios with differing pathways of aerosol emissions. But how do these emissions translate into a spread of direct and indirect radiative forcing in the model?

- When using the "CO2 Group" constraint, how is the multi-modal distribution of ECS (and of TCR) to be interpreted? I would have expected no effect on ECS and TCR as I understood that these are calculated based on simulations with prescribed CO2 concentrations (and therefore CO2 parameters should not affect the results)?

- Discussion / conclusion section: The hiatus discussion is not directly linked to the rest of the manuscript and seems like describing a new issue. Maybe placing into an extra subsection?

- The paper refers to "cumulative carbon emissions" but should refer to "cumulative CO2 emissions" which is the focus of this study.

Minor comments/ suggestions:

- Abstract: use consistently 1 Digit estimates for indicated temperature changes

- Page 9843: definition of TCR: refer to "atmospheric CO2 concentration". Although it is clear what is meant, I would check the manuscript where reported changes in "CO2" should be rather described as changes in "atmospheric CO2 concentrations" to avoid misunderstanding.

- Page 9843: Definition of ECS: ECS itself does not depend on the rate of ocean heat uptake, while observationally constrained estimates of ECS do. . .

- Section 3.3.: What is the choice for the priors for TCR and ECS?

- Page 9847: A quick explanation of "convex hull" in the applied context would be helpful.

- Page 9847, L.28: non-linearity for low-end scenarios hard to see in shaded area. - Page 9851, L.12: ". . .will be discussed later" Indicate in which section the discussion can be found. - Page 9854. L.18: "The median transient response. . ." - Page 9856. L3: It seems in the following peak responses are discussed. Then the legend of figure 5 should indicate this. - Page 9856. L 24: Reference should be given to Fig.6e/f. For the response of soil carbon a linear regression seems to only insufficiently describe soil carbon dynamics. . .

- Fig.2a,b: Here it could be helpful to shortly discuss/explain why (early time) SAT responses are declining with increasing peak emissions (from 100 to 5000 GtC) despite increasing CO2 concentration anomalies.

Spelling / Readability: - Many of the figure legends (e.g. Fig.3,b&d) , axes description (e.g. ylabels in Fig.2) are hard to see. - Fig.5,6: subplot labelling a), b) etc. is missing

- Page 9846, L21: "define" instead of "defines" - Use consistent notations, e.g. "confidence interval" or "c.i." - Page 9856. L21: "to an increases" -> "to increases". . . - Page 9857. L15: "1500 yrs"