

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

Anonymous Referee #3

Received and published: 28 July 2015

Review of: “Earth system response to cumulative carbon emissions”

Overall Evaluation:

This manuscript utilizes a large set of perturbed parameter Earth-system model simulations combined with observational datasets to constrain transient climate response, equilibrium climate sensitivity, and the transient response to cumulative CO₂ emissions. The authors find a linear response of surface air temperature and sea surface temperature to cumulative emissions of CO₂, consistent with previous work. A linear response is also shown for ocean surface pH. Responses of overturning circulation, steric sea level rise and elements of the carbon cycle are found to be less linear. The ranges of TCR and ECS found are consistent with the CMIP5 models and previous efforts with simple climate models but not with recent studies that rely on short-term

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changes in surface air temperature. The authors conclude that ocean heat content data represents the best constraint on TCR and ECS.

I have not found significant fault with the underlying scientific questions or methods of this study. However, the descriptions of the simulations, the presentation of the results, and the overall clarity of the manuscript are poor. The use of terminology is also in places highly confused. Overall this study has the potential to add to the understanding of TCRE and to quantifying uncertainty in projected climate change but in its current state is not suited for publication in Biogeosciences. I recommend that the paper be subject to major revisions.

General Comments:

This study is heavily reliant of work presented in Steinacher et al. (2013) to the point where I had to reread that paper and its supplementary materials to understand what the authors had done in the present study. This degree of recursive reading is unnecessary for a journal like Biogeosciences which has no hard length limit. I recommend giving a full description of the perturbed parameters experiments in the methods section, instead of referring to Steinacher et al. (2013). I recommend adding a table containing the parameters that were perturbed, the prior ranges, and a description of how it was decided which parameters to perturb.

From the supplementary information from Steinacher et al. (2013) it does not appear that the authors have perturbed any ocean carbonate chemistry or biology parameters. As TCRE arises partially from ocean carbonate chemistry modulating the airborne fraction of carbon (MacDougall and Friedlingstein 2015, Goodwin et al. 2015), this limitation should be very explicitly stated in the manuscript.

The definition of TCRE used in this manuscript is confusing. TCRE is the constant of proportionality between cumulative emissions of CO₂ and change in near surface air temperature (Gillett et al. 2013). TCRE does not include the effect of non-CO₂ radiative forcing and certainly should not be used to describe changes in variables other than

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near surface temperature change, as is done here in equation 8. The response of peak temperature to cumulative emissions is the Cumulative Warming Commitment (Allen et al. 2009), which is the sum of TCRE and the Zero Emissions Commitment. I recommend that the generally accepted CO₂ only definition of TCRE be used in this manuscript.

The dataset of simulations used in this study present a good opportunity to discern the effect of non-CO₂ forcing on the linear relationship between cumulative emissions and near surface temperature change. The authors have done some of that implicitly by calculating conventional TCRE (with the 1% experiment) and the proportionality between cumulative emissions of CO₂ and temperature change with all forcing included. This could be made much more explicit.

There is no description in this manuscript of how the model was spun-up. Were all 5000 model versions spun-up or was some other method used to save computing resources? The lack of proper spin-ups crippled the analysis of perturbed physics experiments conducted by the Hadley centre a decade ago (e.g. Collins et al. 2007). Therefore, it would benefit this study to have a full description of how this was done for the present experiments.

The final section of the results discussing the effect of datasets on the PDFs of TCR and ECS is very interesting and could be expanded for clarity.

The discussion and conclusions section drifts into subjects not discussed in the introduction, such as the hiatus and the 2 K temperature change target. It may make more sense to split the discussion and conclusions section and include these subjects in a policy implication subsection. In its present form the conclusions from this study are not clearly articulated.

The relationship between cumulative emission of CO₂ and various Earth-system metrics was explored by Herrington and Zickfeld (2014). An intermediate complexity ESM was used by Olson et al. (2012) to estimate PDFs of TCR and ESC. These studies

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need to be cited and discussed within the context of the present study.

Specific Comments:

Title: The title poorly described the study conducted. I would recommend something more like: "Estimating uncertainty in Earth system response to cumulative emissions utilizing a large perturbed physics ensemble."

Page 9840 line 23: "damage from" not "damage by"

Page 9840 line 25: change to "from burning of fossil fuels"

Page 9841: The first paragraph is poorly written please re-write for clarity.

Page 9843 line 8: change to (e.g. Huber and Knutti . . .

Page 9843 line 21: TCRE is the "Transient Response to Cumulative CO₂ Emissions" not the "Transient Response to Cumulative carbon Emissions". Please change throughout the manuscript.

Page 9843 line 16: Change "interesting" to "useful"

Page 9843 line 21: Add citation to Gregory et al. (2009).

Page 9843 line 23: Spell out "versus".

Page 9843 line 26: Use "Shindell" instead of "He"

Page 9844 first paragraph: This would be a good place to mention Olson et al. (2012).

Page 9844 line 26: Table S1 seems critical to understanding this study. Please reproduce the table in this manuscript.

Page 9846 second paragraph: Why were pulses done from 389 ppm instead of the pre-industrial climate?

Page 9847 line 14: The peak temperature will not necessarily occur in 2300 CE. Is the temperature in 2300 being used as an approximation for the peak temperature? In

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many scenarios would peak temperature occur before 2300?

Page 9847: The term “convex hull” needs to be explained here. I suspect that for most readers of BGS (including myself) without explanation “convex hull” will only incite dim memories of long-ago geometry classes.

The abbreviation PDF (probability distribution function) is never spelled out in the manuscript.

Equation 8: TCRE is strictly defined in terms of temperature. Adopting this formulation confuses the meaning of TCRE. Please remove this equation and find a better way of explaining what you have done.

Page 9851 line 16: CE (Common Era or Christian Era) not AD (In the year of our Lord).

Page 9852 line 23: The defining and most useful feature of TCRE is that it is nearly path-independent (Matthews et al. 2009) so this statement seems out of place.

Page 9853 line 24 to 27: I think the authors are trying to state that the Zero Emissions commitment is nearly zero in this model. This could be stated much more clearly.

Page 9857 line 23 to 25: These questions are awkwardly worded.

Page 9860 line 27: Citations should be added to discussion of the “hiatus”.

Page 9861 final paragraph: This paragraph seems out of place with respect to the rest of the manuscript. This should at least be described in more detail earlier in the manuscript.

Temperature changes expressed in Kelvin and Celsius are used interchangeably in the manuscript. Please pick one and standardise throughout.

Figure 1: Does not include the 1% or pulse experiments which are crucial for the present study. Maybe redraft to take these simulations into consideration.

References:

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Collins, M., C. Brierley, M. MacVean, B. Booth, and G. Harris, 2007: The sensitivity of the rate of transient climate change to ocean physics perturbations. *Journal of climate*, 20 (10), 2315–2320.

Gillett, N. P., V. K. Arora, D. Matthews, and M. R. Allen, 2013: Constraining the ratio of global warming to cumulative CO₂ emissions using cmip5 simulations. *Journal of Climate*, 26, 6844–6858.

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Matthews, H. D., N. P. Gillett, P. A. Stott, and K. Zickfeld, 2009: The proportionality of global warming to cumulative carbon emissions. *Nature*, 459, 829–832, doi:10.1038/nature08047.

Olson, R., R. Sriver, M. Goes, N. M. Urban, H. D. Matthews, M. Haran, and K. Keller, 2012: A climate sensitivity estimate using bayesian fusion of instrumental observations and an earth system model. *Journal of Geophysical Research: Atmospheres*, 117

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(D4).

Steinacher, M., F. Joos, and T. F. Stocker, 2013: Allowable carbon emissions lowered by multiple climate targets. *Nature*, 499 (7457), 197–201.

Interactive comment on *Biogeosciences Discuss.*, 12, 9839, 2015.

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