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Earth System Dynamics Discussions

Interactive comment on "The impact of model variation in CO_2 and temperature impulse response functions on emission metrics" *by* D. J. L. Olivié and G. P. Peters

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General comments

Olivié and Peters derive impulse response functions for CO2 and temperature using data from various model intercomparison projects (C4MIP, LTMIP for CO2; CMIP 3, CMIP5 for temperature) and explore the impacts of the spread in IRFs on common emission metrics such as global warming potential (GWP), global temperature potential (GTP) and integrated global temperature potential (iGTP).

I have several of concerns with this study, which are detailed below:

1) Derivation of IRF from model-ensemble simulations. The authors derive CO2 and temperature IRFs from various intercomparison projects with very different experimental setups. Accordingly, the spread in the derived IRFs is large. The authors briefly mention that some types of simulations (e.g. exponentially growing CO2 emissions as in the C4MIP experiments) are not apprpriate for deriving IRFs, yet there is no discussion about the suitability of the derived IRF distributions. I.e. are all derived IRF distributions equally suitable for the construction of reduced-form models or computation of emission metrics? Another limitation is that this study does not allow for a clear separation the effect of model differences (all intercomparison projects include different subsets of models) and differences in experimental setup (e.g. size of the emission pulse, timing of emissions, coupled versus uncoupled simulations) on the spread in the derived IRFs. I.e. how does the size of the emission pulse, the timing of emissions or the consideration of climate-carbon cycle feedbacks affect the IRF? I think a more useful approach to the derivation of multi-model IRFs would be a dedicated model intercomparison exercise (see Joos et al., 2012). Such an approach would allow to a) use the best possible experimental setup for the derivation of IRFs, b) explore the effects of different experimental setups on the resulting IRF. Furthermore, the same models could be used to derive temperature IRFs, eliminating the inconsistency in the calculation of emission metrics (GTP, iGTP) introduced by combining CO2 and temperature IRFs from different models.

2) Exploration of how the spread in IRFs impacts emission metrics. The interpretation of the results presented in this section is hampered by the poor presentation of results, particularly in Figs. 3, 4 and 5, which are illegible (the font is way too small and the panels contain too much information).

3) It is unclear what the general conclusions are from this study. I.e. what type of experimental setup is more appropriate for the derivation of CO2 and temperature IRFs? Is the assumption of linear IRFs adequate, and if so under which conditions? What emissions metrics are most/least sensitive to the choice of IRF, and why?

Specific comments

Abstract, l. 19-20: “20-35% lower metric values” and “up to 40% higher values”: what is the reference values? The comment applies to several other statements in the abstract (e.g. l. 22, l. 27), for which the reference value is unclear.

p. 938, l. 9: Usually the term CO2 fertilization refers to the increase in ecosystem productivity in response to higher atmospheric CO2, rather than the reduction of CO2 uptake by vegetation due to higher temperatures, as stated in the manuscript.

p. 943, eq. 7: It should be mentioned that the radiative efficiency Ax is dependent on the background atmospheric CO2 concentration.

p. 948, l. 11, “we use the estimated climate sensitivity as an additional constraint”. How is this constraint applied?

p. 948, l. 16-17: Clarify which CMIP5 experiment you use to derive the temperature IRF, i.e. the instantaneous CO2 quadrupling, or the gradual CO2 increase experiment?

p. 949, l. 7-9, “We use a CO2 IRF with four modes . . . and a temperature IRF with two modes”. Please justify your choice.

p. 949, l. 16-17 and p. 950, l. 2-3 “. . . also taking into account how much the IRF parameters deviate from the some a priori values”. Why do you do this? And which a priori values did you use? Please explain.

p. 953, l. 28-29, “considerable difference between CMIP3 and CMIP3*”: Why are the two different? Since no detail is given on how the constraint on climate sensitivity is applied, this statement cannot be understood.

p. 954, l. 2, “The CMIP3 and CMIP3* approach. . .”: see previous comment.

p. 954, l. 6, How is climate sensitivity defined in this analysis?

p. 957, l. 7-8: The use of “underestimates” and “overestimates” suggests that the met-

ric values for BR08 are the ‘true’ values. Replace with “is smaller/larger” or something similar.

p. 959, l. 1, “. . . this problem is ignored in the metric literature”. This is not a valid reason! It would be much cleaner to derive the CO2 IRF and temperature IRF from the same set of models.

p. 960, l. 17: “. . . the three distributions coincide in general rather well, but also how specific differences”. This sentence is incomplete. Also, what does it mean to “coincide rather well in general”? What criteria do the authors use to establish whether distributions are similar or not?

p. 960, l. 22, “. . . is in general rather similar”: see comment above. I wouldn’t call the distributions “similar”: e.g. the CMIP5 distribution becomes much wider for time > 100 years!

p. 960, last sentence, “Although for large time. . .”: Why is this worth mentioning?

p. 962, l. 4: amplitudes of what? Emission pulse amplitudes?

Figures 3, 4 and 5: These figures are illegible! The panels need to be enlarged considerably. Also, these figures contain too much information. I suggest to remove the relative distributions on the right had side of each panel from the figure and summarize the essential information in a table. Finally, no labels are given for the horizontal axes.

Figure 3 caption: Clarify that the number to the right of the left bars is the ratio of the median to the reference value.

Technical corrections

p. 939, l. 29: Replace “large” with “ long”.

p. 941, l. 8: Delete “then” between “ the” and “unique”.

p. 942, l. 18: Replace “o f” with “of”.

p. 957, l. 14, “CO2 has characteristics of a longer lifetime”: Why not say “CO2 has a longer lifetime”?

p. 957, last line: Replace “extend” with “extent”.

p. 958, l. 15: replace “show” with “shown”.

p. 958, last line, “manor”: Do you mean “manner”?

p. 961, l. 12: Insert “such” between “species” and “as BC”.

p. 962, l 12: Insert “presented” between “analysis” and “here”.

Interactive comment on Earth Syst. Dynam. Discuss., 3, 935, 2012.