

Interactive comment on “The impact of model variation in CO₂ and temperature impulse response functions on emission metrics” by D. J. L. Olivié and G. P. Peters

D. J. L. Olivié and G. P. Peters

dirk.olivie@geo.uio.no

Received and published: 6 December 2012

We first give a general response. It is followed by responses to the specific remarks of the reviewer.

General response

We would like to thank the reviewer for the constructive remarks.

We have modified the manuscript in different aspects, and describe these changes

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



here.

In the revised version of the manuscript, we include data from the inter-comparison exercise Joos et al. (2012). During the preparation of our original manuscript, we were aware of this paper (one of us is a co-author), but preferred not to use the data as it was in review. We use the data from Joos et al. (2012) in the same way as the data from C⁴MIP and LTMIP, and keep J07 (the IRF_{CO₂} used in Ramaswamy et al. (2007)) as our reference IRF_{CO₂}.

We compare our results with results from other studies, i.e., Reisinger et al. (2010), Reisinger et al. (2011), and Joos et al. (2012). To make this comparison easier, we now show 5- and 95-percentile values (instead of 10- and 90-percentile values), as used in Reisinger et al. (2010) and Reisinger et al. (2011). To present the impact on emission metrics, we now treat the horizon range from 20 to 500 yr continuously, not only the values at 20, 50, and 100 yr.

We acknowledge that the numbers in Figs. 3, 4, and 5 are difficult to read. We have replaced these figures, and the information is now presented in a different way. Figure 3 shows the (absolute) value of the metrics, while Figs. 4 and 5 show (i) the difference between the median of the metric distribution and the reference value, and (ii) the difference between the 5(or 95)-percentile value and the median. We do not show anymore the combined impact of variation in IRF_{CO₂} and IRF_T (originally shown in Fig. 5), as the spread is determined by the largest individual component. To allow the comparison of spreads caused by IRF_{CO₂} and IRF_T, we indicate the spread in GTP caused by variation in IRF_{CO₂} also in Fig. 5. We neither show the impact of variation in IRF_T onto the iGTP, as this impact is much smaller than the impact from variation in IRF_{CO₂}. We added two tables, containing the principal results for a time horizon of 100 yr.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



We explain in some more detail the numerical method used to estimate the parameters ("probabilistic inverse estimation theory"). For example, we mention the a priori values for the parameters we have used, and indicate that no correlation is assumed among the a priori parameter values, or among the results from the different years in the data from CC-models or AOGCMs.

We have considerably modified the introduction (Sect. 1). We hope to make the aim of the manuscript more clear, and additionally refer more to other work done in the field of metrics, i.e., Wuebbles et al. (1995) Tanaka et al. (2009), Reisinger et al. (2010), Reisinger et al. (2011), and (Joos et al., 2012). We think that there is now less overlap between Sect. 1 (Introduction) and Sect. 2 (Emission metrics and IRFs). Also the second part of Sect. 4, describing the impact of variation in IRF_{CO_2} and IRF_T on the metric values, has been considerably modified.

Response to comments

The comments and remarks of the reviewer are written in *italic* font. The responses of the authors are written in standard font.

Olivié and Peters derive impulse response functions for CO₂ and temperature using data from various model inter-comparison projects (C⁴MIP, LTMIP for CO₂; CMIP3, 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 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 inter-comparison 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 appropriate 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 inter-comparison 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 inter-comparison 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.

We generally agree with the reviewer on all these comments! Specifically, we now use the Joos et al. (2012) result which covers one specific comment. More broadly, this comment is essentially an issue we raise in the discussion/conclusions. Our intention was to take the information that was available at the time, use this to estimate IRFs, and see what the end result was. Before doing this, we do not know how important experimental set up will be. One recommendation from our paper is that specific model inter-comparisons (c.f. Joos et al. (2012)) are needed to derive IRFs. One could say that a weakness of our study is that we did not motivate the community to do such inter-comparisons. One could also argue that a strength of the paper is that we do the analysis and

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

report the issues.

In the revised version of the manuscript we now explain this situation better, but we believe it is important these results are reported before motivating the community to do additional model runs for another inter-comparison.

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).*

We have modified the figures.

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?*

The revised version now has a more thorough discussion of the key findings and consequences of our results.

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.*

As reference we use the metric values obtained using the IRF_{CO₂} from Ramaswamy et al. (2007) and IRF_T from Boucher and Reddy (2008). These

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

numbers refer to that. In the new version of the manuscript, we have shortened the abstract and do not mention these differences. In the main text the numbers are still mentioned, but there it is now clear what the reference is.

p. 938, l. 9 : *Usually the term CO_2 fertilization refers to the increase in ecosystem productivity in response to higher atmospheric CO_2 , rather than the reduction of CO_2 uptake by vegetation due to higher temperatures, as stated in the manuscript.*

We have corrected this in the revised version of the manuscript.

p. 943, eq. 7 : *It should be mentioned that the radiative efficiency A_X is dependent on the background atmospheric CO_2 concentration.*

We mention this now in the revised version of the manuscript.

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

We estimate the parameters f_1 , τ_1 , f_2 , and τ_2 in IRF_T by comparing the global mean temperature response of the AOGCM with that of a convolution of the IRF_T with the RF evolution. In our standard approach, we consider these 4 variables as independent. In the CMIP3* approach, we impose that the sum of f_1 and f_2 is equal to the value of λ given in Randall et al. (2007). In practice this is done by using only three parameters, i.e., f_1 , τ_1 , and τ_2 , and replacing in the expression for IRF_T the value f_2 by $\lambda - f_1$.

p. 948, l. 16-17 : *Clarify which CMIP5 experiment you use to derive the temperature IRF, i.e. the instantaneous CO_2 quadrupling, or the gradual CO_2 increase experiment?*

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

We used both scenarios simultaneously, and state this now more explicitly in the text.

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

For IRF_{CO₂}, we base the choice for four modes on the use in IPCC (2001) and IPCC (2007).

For IRF_T, we base our results on Olivié et al. (2012) who compared the use of one, two, and three modes to emulate the behaviour of AOGCMs with IRF_T. Two modes performed considerably better than one mode, while using three modes did almost not improve the behaviour. Also Li and Jarvis (2009) found no improvement for the first 500 yr when using 3 modes instead of 2 modes

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.*

Our approach is based on probabilistic estimation theory. To estimate the a posteriori values of the parameters, one optimizes a function (Tarantola, 2005, page 68),

$$2 S(x) = (g(x) - d_{\text{obs}})^T C_D^{-1} (g(x) - d_{\text{obs}}) + (x - x_{\text{prior}})^T C_M^{-1} (x - x_{\text{prior}}) \quad (1)$$

where x is the vector of parameters in the IRF, d_{obs} are the results from the CC-model or AOGCM, and $g(x)$ represents the results from using the time convolution of the IRF with the CO₂ emission or RF scenario used in the CC-model or AOGCM.

This expression shows that two contributions will be weighted for finding an optimal parameter set: (i) how close the behaviour of the IRF convolution is to the

CC-model or AOGCM results (weighted by C_D , the covariance matrix of model and observational errors); and (ii) how close the parameters are to their a priori values (weighted by C_M , the a priori parameter covariance matrix).

- 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.*

We describe more explicitly in the revised version of the manuscript the difference between CMIP3 and CMIP3*.

- p. 954, l. 2 *"The CMIP3 and CMIP3* approach ...": see previous comment.*

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

In the original manuscript, it was defined as "the change in equilibrium surface temperature per unit radiative forcing" (Shine et al., 2005). However, we acknowledge that "climate sensitivity" in the literature is defined as "the equilibrium global mean surface temperature change following a doubling of atmospheric CO₂ concentration" (McAveney et al., 2001, page 629, Section 8.6.1). To avoid confusion, we will use the term climate sensitivity parameter, as used in Shine et al. (2005) and Olivié and Stuber (2010).

- p. 957, l. 7-8 : *The use of "underestimates" and "overestimates" suggests that the metric values for BR08 are the "true" values. Replace with "is smaller/larger" or something similar.*

We have modified this in the revised version of the manuscript.

- 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*

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

same set of models.

We agree with the reviewer on this point. Joos et al. (2012) does this. It is nevertheless not straightforward to fit a temperature IRF to an emission of CO₂ (see Joos et al. (2012) for the shape of this, and the model spread). One of us is currently working on temperature IRFs from pulse emissions. As in our previous comments, we are working with the literature available and the limitations within.

p. 960, l. 17 : *"... the three distributions coincide in general rather well, but also show 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?*

We admit that the description of the differences and similarities is not very detailed, and have tried to improve this. In Olivie et al. (2012, their Tables 4, 5, and 8) the RMSE (root means square error) is used to describe the difference between the behaviour of AOGCMs and the convolution of IRF_T. This was a useful approach as the AOGCM experiments had a well defined length. Due to the infinite time range of the IRFs, we think it is not appropriate to describe the differences among IRF_{CO₂} and IRF_T with RMSE. We therefore have chosen to describe the differences among the IRFs at certain points in time in the revised version of the manuscript.

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!*

Also here, we describe more detailed the similarities and differences among the IRF_T.

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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

We wanted to describe how the CMIP3* distribution is situated with respect to the standard IRF_T from BR08.

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

With amplitudes, we meant indeed the "size of the pulse". However, as this has been investigated in Joos et al. (2012), we do not mention it anymore in the conclusions of the new version of the manuscript.

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 hand side of each panel from the figure and summarize the essential information in a table. Finally, no labels are given for the horizontal axes.*

We have replaced the Figs. 3, 4, and 5 by new figures which cover the horizon range between 20 and 500 yr. We have also put the values in a table for a time horizon of 100 yr.

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

We have now replaced the Figs. 3, 4, and 5 by new figures.

Technical corrections

p. 939, l. 29 : *Replace "large" with "long".*

We have modified this in the manuscript.

p. 941, l. 8 : *Delete "then" between "the" and "unique".*

We have modified this.

p. 942, l. 18 : *Replace "o f" with "of".*

We have modified this.

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

As the response of CO₂ shows different time scales, we wanted to avoid the use of the word "lifetime" for CO₂.

p. 957, last line : *Replace "extend" with "extent".*

We have modified this.

p. 958, l. 15 : *replace "show" with "shown".*

We have modified this.

p. 958, last line , *"manor": Do you mean "manner"?*

Yes, we have modified it in the manuscript.

p. 961, l. 12 : *Insert "such" between "species" and "as BC".*

We have modified this in the manuscript.

p. 962, l 12 : *Insert "presented" between "analysis" and "here".*

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



References

- Boucher, O. and Reddy, M. S.: Climate trade-off between black carbon and carbon dioxide emissions, *Energy Policy*, 36, 193–200, doi:10.1016/j.enpol.2007.08.039, 2008.
- IPCC: Climate change 2001: the scientific basis. Contribution of working group I to the third assessment report of the intergovernmental panel on climate change, Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 881 pp., 2001.
- IPCC: Climate change 2007: the physical science basis. Contribution of working group I to the fourth assessment report of the intergovernmental panel on climate change, Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 996 pp., 2007.
- Joos, F., Roth, R., Fuglestedt, J. S., Peters, G. P., Enting, I. G., von Bloh, W., Brovkin, V., Burke, E. J., Eby, M., Edwards, N. R., Friedrich, T., Frölicher, T. L., Halloran, P. R., Holden, P. B., Jones, C., Kleinen, T., Mackenzie, F., Matsumoto, K., Meinshausen, M., Plattner, G.-K., Reisinger, A., Segschneider, J., Shaffer, G., Steinacher, M., Strassmann, K., Tanaka, K., Timmermann, A., and Weaver, A. J.: Carbon dioxide and climate impulse response functions for the computation of greenhouse gas metrics: a multi-model analysis, *Atmos. Chem. Phys. Discuss.*, 12, 19 799–19 869, doi:10.5194/acpd-12-19799-2012, <http://www.atmos-chem-phys-discuss.net/12/19799/2012/>, 2012.
- Li, S. and Jarvis, A.: Long run surface temperature dynamics of an A-OGCM: the HadCM3 4xCO₂ forcing experiment revisited, *Climate Dyn.*, 33, 817–825, doi:10.1007/s00382-009-0581-0, 2009.
- McAveney, B. J., Covey, C., Joussaume, S., Kattsov, V., Kitoh, A., Ogana, W., Pitman, A., Weaver, A. J., Wood, R. A., and Zhao, Z.-C.: Climate Change 2001: The Scientific Basis. Contribution of Working Group I to the Third Assessment Report of the Intergovernmental Panel on Climate Change, chap. Model evaluation, pp. 471–524, Cambridge University Press, 2001.
- Olivié, D. and Stuber, N.: Emulating AOGCM results using simple climate models, *Climate Dyn.*, 35, 1257–1287, doi:10.1007/s00382-009-0725-2, 2010.
- Olivié, D. J. L., Peters, G. P., and Sain-Martin, D.: Atmospheric response time scales estimated from AOGCM experiments, *J. Climate*, 25, 7956–7972, doi:10.1175/JCLI-D-11-00475.1, 2012.

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



- Ramaswamy, P. F. V., Artaxo, P., Bernsten, T., Betts, R., Fahey, D. W., Haywood, J., Lean, J., Lowe, D. C., Myhre, G., Nganga, J., Prinn, R., Raga, G., Schulz, M., and Dorland, R. V.: Climate change 2007: the physical science basis. Contribution of working group I to the fourth assessment report of the intergovernmental panel on climate change, chap. Changes in atmospheric constituents and in radiative forcing, Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 129–234, 2007.
- Randall, D. A., Wood, R. A., Bony, S., Colman, R., Fichet, T., Fyfe, J., Kattsov, V., Pitman, A., Shukla, J., Srinivasan, J., Stouffer, R. J., Sumi, A., and Taylor, K. E.: Climate change 2007: the physical science basis. Contribution of working group I to the fourth assessment report of the intergovernmental panel on climate change, chap. Climate models and their evaluation, Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 589–662, 2007.
- Reisinger, A., Meinshausen, M., Manning, M., and Bodeker, G.: Uncertainties of global warming metrics: CO₂ and CH₄, *Geophys. Res. Lett.*, 37, L14 707, doi:10.1029/2010GL043803, 2010.
- Reisinger, A., Meinshausen, M., and Manning, M.: Future changes in global warming potentials under representative concentration pathways, *Environ. Res. Lett.*, 6, 024 020, doi:10.1088/1748-9326/6/2/024020, 2011.
- Shine, K. P., Fuglestedt, J., Hailemariam, K., and Stuber, N.: Alternatives to the global warming potential for comparing climate impacts of emissions of greenhouse gases, *Climatic Change*, 68, 281–302, doi:10.1007/s10584-005-1146-9, 2005.
- Tanaka, K., O'Neill, B. C., Rokityanskiy, D., Obersteiner, M., and Tol, R. S. J.: Evaluating Global Warming Potentials with historical temperature, *Climatic Change*, 96, 443–466, 2009.
- Tarantola, A.: Inverse problem theory and methods for model parameter estimation, Society for Industrial and Applied Mathematics, Philadelphia, 342 pp, 2005.
- Wuebbles, D. J., Jain, A. K., Patten, K. O., and Grant, K. E.: Sensitivity of direct global warming potentials to key uncertainties, *Climatic Change*, 29, 265–297, 1995.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)