**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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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

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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 C^{4}MIP and LTMIP, and keep J07 (the IRF$_{CO_2}$ used in Ramaswamy et al. (2007)) as our reference IRF$_{CO_2}$.

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$_{CO_2}$ 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$_{CO_2}$ and IRF$_T$, we indicate the spread in GTP caused by variation in IRF$_{CO_2}$ 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$_{CO_2}$. We added two tables, containing the principal results for a time horizon of 100 yr.

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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 reviewer

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

Summary

This is a useful piece of work but needs improvement in the presentation. I would largely endorse the on-line review comments by K. Tanaka. Possibly, if the role of the paper was better defined relative to Joos et al. (2012) then the focus on IRFs might be easier to justify.

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Wording

- *I would argue that metrics (as single numbers) are convenient rather than necessary – see opening sentence of abstract and section 1.*

  We have modified this, and do not use the word "necessary" anymore.

- *The sentence "Analytic .. (IRFs)." seems out of place – but maybe just the word "Analytic" is unnecessary. (p937)*

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

- *p937 L19–23: "The IRFs .. temperature IRF." It is not clear whether this is meant to be a generic description or a summary of what is done in this paper.*

  Originally this was intended as a generic description. This has now be reworded, as the introduction (Sect. 1) has changed considerably.

- *p939 L17: "non-linearities in the ocean" → "non-linearities in the behaviour of the ocean"*

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

- *p940 L23–25: IRFs are a condensed way to describe any linear system.*

  We have modified the introduction and this is now said explicitly.

- *The term "climate sensitivity" has been long-established (especially in IPCC reports) as meaning the amount of equilibrium warming from doubling CO_2. However unfortunate one might consider this, a paper such as this one must either acknowledge this prior usage OR use an alternative term for λ (e.g.*

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"climate sensitivity parameter" as used by Olivié and Stuber (2010)) – or, of course, do both. (p943 L6 and p948 L8).

We acknowledge that "climate sensitivity" (McAveney et al., 2001, page 629, Section 8.6.1) is defined as "the equilibrium global mean surface temperature change following a doubling of atmospheric CO_2 concentration", while Shine et al. (2005) define the "climate sensitivity parameter" as "the change in equilibrium surface temperature per unit radiative forcing".

We now use in a revised version of the manuscript the term "climate sensitivity parameter".

- p945 L1–2: *This sentence is trying to combine too many cases. The "and eventually CO_2" only applies to two of the four metrics for non-CO_2 species.*

We rewrite this in the new version of the manuscript.

- p950, L11: *suggest: "response" → "available output"*

We have modified this.

- p952, L20: *"The IRF ..." which one? Four IRFs (2 from C^4MIP plus J07 and LTMIP) are mentioned in the previous sentence.*

We meant the $C^4MIP(u)$ and $C^4MIP(c)$ IRFs – we have rewritten this sentence in the revised manuscript.

- p957, L12: *"short" →"briefly"*

We have modified this.

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- p957: *overall, I think that this part of the discussion could be shortened*

We have shortened this discussion in the revised manuscript.

- p961, L2: *"The impact from .. has a similar impact ..". This sentence needs to be reworded.*

We have reworded this sentence.

Comments/queries on the science

- *General: Metrics for comparing greenhouse gases are usually intended for potential substitutions of emissions and so are generally intended for relatively small changes.*

We agree with this and mention this now explicitly in the revised manuscript.

- *p938, L6–7. It is not the negative feedback that creates the non-linearity, it is the saturation of this process at higher CO_2.*

We agree with this, and change the manuscript accordingly.

- *p938 L8: main non-linearity associated with ocean circulation is feedback from warming*

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

- *938 L9: "reduces the CO_2 uptake" → "reduces the net CO_2 uptake" – since arguably higher temperatures often increase both photosynthesis and respiration, but increase the latter more.*

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

- *p939 L6: this equivalence does of course, assume linearity*

  We agree with this remark, and mention it in the revised version of the manuscript.

- *p939 L15: possibly worth noting that Hansen and Sato (2012) has talked about additional longer time scale associated with cryosphere processes.*

  We have added a reference to Hansen and Sato (2012) in the text.

- *p940 L2: IPCC used Bern CC response to define a GWP of 1.*

- *Knutti (2010) gives a discussion of why inter-comparisons are not set up to lead to probability distributions – the authors need to engage with those comments.*

  We refer to Knutti (2010) now in the introduction.

- *p959 L1–2: saying that the "combination of potentially inconsistent responses is done routinely", is disappointing. The Monte Carlo techniques used here have the capability of dealing with such lack of independence in a way that other approaches might struggle to follow – a missed opportunity.*

  We are aware of the limitations of our approach. At the moment, we do not see how we can avoid some of these inconsistencies by using Monte Carlo simulations.

Abbreviations

The large number of abbreviations and acronyms makes for disjointed reading. Some suggestions for mitigating this problem (without undue expansion of the length) are:

- *"SCM" is not used often (p940, L13; p949, L11) – it could be written out in full.*

  We do not use this abbreviation in a new version of the manuscript.

- *Not clear whether "models" could replace AOGCM – the context CMIP3 etc. usually carries the implications of what sort of model.*

  In the revised version of the manuscript, we stille use the term AOGCM.

- *Note also that "BC" is not defined in the abstract (which should be self-contained). It may be worth spelling out "black carbon" in all contexts except column headings in tables. (p938 L20, p955 L2)*

  We replaced "BC" by "black carbon" in the abstract, but we have kept "BC" in the main text.

- *p946, L1, L5: "CC" seems to be undefined except as part of C^4MIP – it seems a bit strange to only expand part of the C^4.*

  We agree that it might be more consistent to use the term "C^4-models" in the text. However, we would like to use the more commonly used abbreviation CC-models.

- *p960 L5: MLO-AGCM apparently undefined. Presumably mixed layer ocean (but for the carbon cycle community, MLO means Mauna Loa Observatory (and the*

NOAA code for CO_2 data from there)).

MLO has been defined on l10 p948, and only used once in the remaining of the text.

Queries

- *p942, L1: in what sense is the fit "best"?*

  This conclusion is indeed a bit strange – one can obtain even better fits using more modes. We rephrase this in the revised version of the manuscript.

- *p952, L1: it is not obvious why this "square root" factorisation is necessary (or even useful) – I would have thought that an eigenvector decomposition (to independent normally-distributed components) would have been more useful – please clarify.*

  Imagine a distribution of the form

  $$\sim \exp\left(-\frac{1}{2}x^T \Sigma^{-1} x\right) . \tag{1}$$

  When we want to do a Monte-Carlo simulation, we would like to sample vectors x, such that their probability of occurrence agrees with this distribution. If we decompose $\Sigma = L\,L^T$, then the distribution can be rewritten as

  $$\sim \exp\left(-\frac{1}{2}x^T \Sigma^{-1} x\right) = \exp\left(-\frac{1}{2}x^T (L\,L^T)^{-1} x\right) = \exp\left(-\frac{1}{2}(L^{-1}x)^T\,(L^{-1}x)\right) . \tag{2}$$

  This last expression corresponds with a normal distribution (with an apparent covariance matrix I) in the variable $L^{-1}x$. It means that if $y = L^{-1}x$ is a random

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  vector where al its components are normally distributed and uncorrelated, then $x = L\,y$ will be in accordance with the initial distribution in Eq. 1. By creating random vectors y using a random generator, one obtains by applying L a sample vector x with the covariance properties of the system being fulfilled.

  One can in principle use the decomposition $\Sigma = S\,\Lambda\,S^T$ (where Λ is a diagonal matrix and $S^T = S^{-1}$ as Σ is symmetric) and rewrite Σ^{-1} as $\Sigma^{-1} = (S\,\Lambda\,S^T)^{-1} = (S^T)^{-1}\,\Lambda^{-1}\,S^{-1} = S\,\Lambda^{-\frac{1}{2}}\,\Lambda^{-\frac{1}{2}}\,S^T = (S\,\Lambda^{-\frac{1}{2}})(S\,\Lambda^{-\frac{1}{2}})^T$ such that the reasoning of above can be followed (replacing L by $S\,\Lambda^{-\frac{1}{2}}$).

  We have now changed the revised version of the manuscript, by saying that the matrix L allows a very easy transformation from a normally distributed vector y to a vector x whose frequency of occurrence is in agreement with the prescribed distribution.

Referencing

There are several points where the referencing is inadequate:

- *p949, L11: Tarantola (2005) is a substantial book. A page or section number should be specified (and a name for the method if possible).*

  The two principal formulas used can be found in Section 3 of the book (Tarantola, 2005). We mention this now explicitly.

- *IPCC (2007) is not an appropriate reference. If (as seems not to be the case here) a reference to the whole volume is required it should be by editors (Solomon et al). However in this paper, most of the IPCC references should be to individual chapters, referenced by chapter authors (both to give credit to those who wrote them and more importantly to help the reader) with the reference*

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in the text supplemented by page number or (better) section number (or table number as appropriate) – again to help the reader.

In the original manuscript, IPCC (2007) in almost all cases, was aimed to refer to specific chapters of the report. Therefore, we now refer to the specific chapters, and eventually added Table or Section information.

- *Same remarks apply to citing Houghton (1990) – p937, L13*

We specify now that it is chapter 2.

- *Same remarks apply to citing IPCC(2001)– p938, L27*

We refer now to the specific chapter.

The authors and/or editors should also check to ensure that the journal abbreviations comply with ESD standards.

Suggestions for clarification

- *For consistency, $B(t)$ and $E(t)$ should be written as $B_X(t)$ and $E_X(t)$ – Eq. (3) and p941, L5.*

We have modified this in the manuscript.

- *Equations 17, 18 and p951, L5: maybe use m for number of models since n is used for number of exponentials in IRF.*

We have modified this.

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- *I think that in Figs. 3, 4, and 5 the graphical presentation (in the right hand columns) of percentage spreads does not convey much. Perhaps it would convey more if such information from figures 3 and 4 was combined into a restructured figure 5 so that the spread was shown as "total" (as now) alongside the contributions from uncertainties in CO_2 IRFs (from fig 3) and temperature IRFs (from fig 4).*

We have modified the Figs. 3, 4, and 5. To be able to compare better he impact of variation in $\mathrm{IRF_{CO_2}}$ and IRF_T on GTP, we show them in the same figure panel. For iGTP, we do not show this comparison, as the impact from IRF_T is always smaller than the impact from $\mathrm{IRF_{CO_2}}$.

Issues with English

- *singe" → "single" p936 L3(4)*

We have corrected this in the manuscript.

- *"life time" → "lifetime" – p936 L25, p938 L19 (twice), p941 L8, p957 L14*

We have modified this. We additionally changed it on p941 l9, p941 l 13, p952 l7, p955 l3, p957 l14, p957 l26, and p962 l11.

- *p941 L16: "fast" → "rapid"*

We have corrected this in the manuscript.

- *"undimensional" → "dimensionless" – p945 (943), L21 – p 944, L9,12 (13)*

We have corrected this.

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- *"extend"* → *"extent" p957, L29*

  We have corrected this.

- *"manor"* → *manner p958, L28*

  We have corrected this.

Minor typographical errors

- *"o f"* → *"of" p942 L18*

  Ok

- *"acoupled"* → *"acoupled" in Cox reference*

  Ok

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