

## ***Interactive comment on “Accounting for the climate-carbon feedback in emission metrics” by Thomas Gasser et al.***

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The authors present a new and elegant approach to including climate-carbon cycle feedbacks consistently in the estimates of emission metrics, and more specifically, absolute metrics for non-CO<sub>2</sub> components. The paper makes several important points associated with the treatment of climate-carbon cycle feedbacks in the calculations of emission metrics performed for IPCC AR5. The text requires some editing (although I like the style of writing), but the argument is clear and the results are well presented. I think this paper potentially has a strong impact in the field of emission metrics and may influence the next IPCC report but can also lead to confusion among metric users as I discuss below. The paper requires a revision by reflecting the comments below before being recommended for publication in Earth System Dynamics.

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We thank the referee for his review and support.

I start with one broad comment, followed by several minor ones. The paper begins with the issue that the treatment of climate-carbon cycle feedbacks was inconsistent in representative metric values in IPCC AR5 (i.e. Table 8.A). More precisely, such feedbacks are accounted for in the estimates of absolute metrics for CO<sub>2</sub> but ignored in those for non-CO<sub>2</sub> components, resulting in an inconsistency when they are put together to calculate relative metrics. This inconsistency is, to be sure, clearly indicated in multiple places in IPCC AR5, but my observation is that the inconsistency has created confusion among metric users. Some studies that follow (e.g. (Cherubini et al. 2016; Levasseur et al. 2016)) support a use of alternative metric values taking climate-carbon cycle feedbacks consistently into account (i.e. Table 8.SM.15 in the Supplementary Material of IPCC AR5), even though alternative values are available only for a subset of the components of interest. Now, the paper reveals that the approach to incorporating climate-carbon cycle feedbacks for non-CO<sub>2</sub> components adopted in IPCC AR5 was actually wrong because the natural carbon sinks are assumed inactive for the additional CO<sub>2</sub> release through climate-carbon cycle feedbacks (e.g. Figure 2). This finding essentially disqualifies all the alternative metric values in IPCC AR5.

Given the situation above in the recent past, this paper may create a new confusion among metric users dealing with climate and environmental policies and assessments. I would therefore request a more detailed clarification of what has happened and what should be done for the metric values in IPCC AR5 in their view. I think that this paper is a right place to do so because some of the authors have been closely involved in the writing of the metric section of IPCC AR5.

Hopefully this comment can be taken in a constructive way, but the paper can be more explicit about why the treatment of climate-carbon cycle feedbacks ended up with being inconsistent in IPCC AR5. The paper describes how it is inconsistent in sufficient details (e.g. Page 5, Lines 3-9), but it is unclear to me why this has happened. For

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instance, why was it not possible to estimate an IRF for CO<sub>2</sub> response without climate-carbon cycle feedbacks? If this were available, this might have allowed one to estimate metrics 'consistently' without climate-carbon cycle feedbacks. This might have been an alternative solution, if not a best one, in light of the inherent linear limitation in the IRF approach that is discussed in Section 5.2. In practice, it is probably not feasible to re-do an experiment requiring many models. But, looking back, was there a lack of coordination at the beginning? Furthermore, what about the method to account for climate-carbon cycle feedbacks for non-CO<sub>2</sub> components in AR5? This method has not been sufficiently tested before the adoption and is based just on a section of one peer-reviewed paper (Collins et al. 2013), whose main contributions lie elsewhere. How was the ad-hoc decision process leading to the adoption of this approach made? Is there anything useful that can be learned from for the next IPCC report? What are the recommendations for metric values? I noticed that the paper does contain some text recommending the new estimates (page 12, lines 15-17), but it is buried in the middle of the paper and I am not sure what are the intentions. I raised some of the questions that may arise if the paper is officially published, although not all of them may not have to be answered in this paper. Clarifications suggested here should be helpful for metric applications, and ultimately the IPCC AR6.

The reviewer raises a fascinating and important point, but we fear, well beyond the scope of this paper. Essentially, the reviewer is passing comment on IPCC processes. The IPCC assesses the literature, and by doing so, places appropriate confidence on different findings. This comment is far broader than just the feedback value from Collins et al. (2013). The GWPs have been update in all five ARs, sometimes due to shifting background concentrations and sometimes due to improved scientific understanding. Thus, users of metrics should have an expectation that GWPs (and GTPs) will change in the future, due to both shifting background concentrations and improved scientific understanding. The inconsistency with the feedbacks has occurred in all ARs, and neither the literature (including reviewers) nor the IPCC has elevated this issue sufficiently for new analysis to be performed, until Collins et al. (2013). Yes, one could question

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the IPCC and scientific community processes, a worthy endeavor but well beyond the scope of this paper. But also, the IPCC AR5 WG1 Chapter 8 has clearly noted that values change and will continue to change.

As to making recommendations, following the same logic that our paper should remain a research paper, we have decided not to make any. We now believe that our initial recommendation, although motivated by being the most up-to-date and comprehensive in terms of modeling, may not be the best – as shown by the comment by M. Sarofim and colleagues. Now we limit our paper to a short discussion as to why one would want to exclude or include the feedback, considering this is not our choice to make. We understand this won't please the metric-user community, but it appears that the choice is ultimately more a political choice than a scientific one (just as for the time horizon).

Relevant text added: “Therefore, the choice of including or excluding the feedback ultimately depends on the user’s needs. On the one hand, for the sake of simplicity and transparency, the feedback could be excluded from the evaluation of GWPs, since it avoids the trouble of the five convolutions shown in figure 4. On the other hand, if absolute (e.g. time-varying) metrics are used as a first-order model of climate change, one may prefer including the climate-carbon feedback to have a better representation of the system.”

Page 1, Lines 11-14 The argument concerns only a set of works using IRF to estimate emission metrics. There are also a body of relevant works based on other approaches like simple climate models (e.g. (Tanaka et al. 2009)) and more complicated ones (e.g. (Gillett and Matthews 2010)). These particular studies do consider climate-carbon cycle feedbacks to calculate emission metrics. The statement can be revised to be more restrictive.

We have added the following sentence to the first paragraph of the ‘mathematical

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framework' section (in which IRFs are presented as a means to calculate metrics):  
“Note that emission metrics can also be estimated thanks to complex model simulations (e.g. Tanaka et al., 2009; Sterner and Johansson, 2017), with the strong caveat that the approach lacks the simplicity and transparency of the IRFs.”

We also note that part of the discussion (section 5.2) is dedicated to the interest of model-based metric estimates (that can include feedbacks in a much easier way than IRFs).

Page 2, Line 2 Another area that I could think of is the ecosystem community (e.g. (Neubauer and Megoñigal 2015)).

Yes. Added.

Page 2, Line 13 If there is any reference to support this statement for the last century, please add.

It was unclear that the references to support this statement were the same as for the next sentence. So we have slightly altered the two sentences to put the references at the right place.

Page 2, Line 21 “whose” instead of “which”?

Changed.

Page 3, Line 9 I don't think the underlying models exhibit a hysteresis within the range of IRF calibrations.

Yes they do! The inertia of the simplest IRF (one decaying exponential) is enough to exhibit hysteresis. If a symmetric forcing is applied to any of the IRFs used in this

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paper, the resulting response will show hysteresis if looked at in the (forcing, response) plane.

Page 4, Line 6 In practice, this pulse emission is large. As in Appendix A, it is 100 GtC in the case of CO<sub>2</sub>.

Agreed. A sentence has been added: “Note that the assumption of a very small pulse may be inconsistent with the way the IRFs are actually derived, as it is currently the case for CO<sub>2</sub> (see appendix A).”

Page 5, Line 26 Should it be  $a(t')$  instead of  $a(t)$  in the integral?

Yes. Corrected.

Page 7, Line 16 It would be helpful if the authors provide a few sentences on how climate-carbon cycle feedbacks are modeled in OSCAR, rather than just a reference. Do the feedbacks act only on soil carbon? What about NPP? Do they directly affect the ocean carbon uptake?

Done:

“OSCAR includes the following climate-carbon feedbacks: the effect of temperature and precipitation change on net primary productivity of land ecosystems, their heterotrophic respiration, and the rate of occurrence of wildfires; and the effect of temperature change on the carbonate chemistry and the stratification of the surface ocean.”

Page 10, Line 3 Please elaborate on how this equation was derived.

The way we derive this equation is explained in the three previous paragraphs. We find difficult to elaborate further. But we have extended one sentence in the third paragraph

to make clearer where the Dirac- $\delta$  function comes from, and we have put the final equation in a separated paragraph starting with “based on the above” to improve clarity.

Page 10, Lines 9-10 There are many earth system processes that are nonlinear. As something that has been discussed intensively before, I would point out the buffering of ocean CO<sub>2</sub> uptake under rising atmospheric CO<sub>2</sub> concentration. But this nonlinearity can be modeled by a revised IRF approach that treats the atmosphere and the mixed layer as one box (Hooss et al. 2001).

True, but our point was about reversibility. We have added some clarification: “This is however likely unrealistic, given all the existing processes, such as vegetation migration (e.g. Jones et al., 2009) or permafrost thawing (e.g. Koven et al., 2011), that can produce some degree of irreversibility in the system but are ignored here.”

Page 11, Lines 4-5 Related to the comment above, the authors should refer to the relevant debate on the linear limitation of IRF (Joos et al. 1996; Hooss et al. 2001). A detailed biogeochemical discussion is given in Section 2.1.2 of (Tanaka et al. 2007).

We disagree that this section is the place where to mention this ‘debate’. We demonstrate that the IRF we derived is only an approximation and therefore has a limited domain of validity, and later (in the conclusion) we remind the reader that more complex models should be used in some cases. We find this sufficient as our paper is not a review on IRFs. Note also that we do discuss the interest of more complex model in sections 5.2.

Page 12, Line 2 This should be “figures 5 and 6” because there is no figure 7 in the current manuscript.

Corrected.

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Page 12, Lines 15-17 Related to my major comment, if this is really a recommendation for metric users, this needs to be more highlighted in the text. Metric users would otherwise be left wonder what are the values that should be used for applications.

As explained above, we have decided to not recommend any particular metric. This is not the goal of this paper.

Page 13, Line 20 This is just a minor note, but TOTEM (Ver et al. 1999; Mackenzie et al. 2011), which is one of the models used to derive the IPCC AR5 IRF (Joos et al. 2013), accounts for nitrogen and phosphorus limitations.

Yes. Although this does not play any role in the experimental setup of Joos et al. (2013) – or in ours – since there is no N or P deposition during the establishment of the IRF; just as there is no land-use change. And so we argue that these three drivers (and others) are therefore not accounted for in the IRF, even if the response has been calibrated on a model that in principle includes the drivers.

Page 14, Lines 29-30 I fully agree with this statement.

Thank you.

Page 15, Lines 16-17 I am coming back to the first minor comment. Although I somewhat hesitate to repeat this point because of the conflict of interest, the paper should discuss studies that estimate emission metrics based on models other than IRFs at least at some length. Examples are (Manne and Richels 2001; Tanaka et al. 2009; Gillett and Matthews 2010; Reisinger et al. 2010; Johansson 2012; Smith et al. 2012; Tanaka et al. 2013; Sterner et al. 2014), and there are many more. The current manuscript narrowly focuses on IRF-based studies. I believe that adding more relevant studies should enrich the discussion in this paper and make the argument more

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convincing.

We have added a reference to the very recent and only paper we know of that is a model-based study of the climate-carbon feedback in emission metrics (Sterner and Johansson, 2017).

Page 27 Figure 4 is not discussed in the paper

It is now. It was just a numbering issue.

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