

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

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This manuscript presents a methodology to better assess the greenhouse-gases emission metrics, by considering and removing the "climate-carbon" feedback that is implicitly used in previous estimations and in previous IPCC recommendations. The methodology is well exposed and rather straightforward, the scientific discussion is clear and well written. Therefore, I have no comment on the technical content of this paper.

We thank the referee for acknowledging the technical quality of the paper.

In contrast, I have some major comments on the overall presentation, introduction and conclusion: these critical comments must be accounted for by the authors before considering publication. Indeed, greenhouse-gases emission metrics is a very "subjective"

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tool that should be presented as such. It is possible to build a very accurate subjective methodology, but this certainly does not help to provide an objective one.

Nowhere in this paper do we pretend to create an “objective” metric. The subjective aspects of emission metrics have been largely discussed in the literature (e.g. IPCC AR5 WG1 Chapter 8), and the topic falls out of the scope of our paper. Here, we discuss scientific and technical issues regarding the inclusion of the climate-carbon feedback in metrics.

I therefore strongly disagree with the general tone of the paper, given in the introduction: page 1, line 20: "However, including carbon-climate feedbacks, particularly in absolute metrics or for short time horizons, gives a more realistic representation of the response"

This sentence has been changed: “Including or excluding the climate-carbon feedback ultimately depends on the user’s goal, but consistency should be ensured in either case.”

It now reflects the fact that we do not recommend a particular approach, between including and removing the feedback. We do recommend, however, a consistent approach, and therefore to update the IPCC metric estimates.

I also strongly disagree with the conclusion that: page 15, line 15: "To avoid potential biases in metric values, we suggest to include the climate-carbon feedback in metric estimates".

This has been changed as well. The concluding paragraph now is:

“Ultimately, whether emission metrics should include the climate-carbon feedback is a decision for the user, and we only recommend consistency in the way feedbacks are included or excluded. The trade-off between simplicity and transparency on the

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one hand, and accuracy of representation on the other hand, has to be weighed by the final user. But metric users should also keep in mind that IRFs and emission metrics are extremely simple models of a complex system, and that sometimes it may be beneficial to use more complex models that better capture multiple and interacting feedback processes.”

The very concept of a unique simple metric for GHG is both UNREALISTIC and BIASED. Refining this concept will not change this fundamental fact. The purpose of GWPs or GTPs is to provide a unique simple metric to compare the "climatic impact" of the many different anthropogenic greenhouse gases (GHG). Obviously, from a scientific perspective, this amounts to comparing oranges and apples. I understand that such an exercise is necessary from a policy perspective, and that scientists should help and provide numbers. Still, I am not convinced that comparing "very accurately" oranges and apples is either necessary or desirable. At the very least, when comparing them, scientists should keep insisting on the differences.

Our paper is embedded in the existing literature on emissions metrics. The basic premise of the paper was to reassess the way that feedbacks are included in metrics, an issue noted as requiring research (IPCC AR5 WG1 Chapter 8). As the reviewer notes, metrics may be “necessary from a policy perspective, and that scientists should help and provide numbers”. We see this paper as fulfilling a request from policy makers, to show and correct a mistake made by the IPCC. We additionally show several metrics (GWP, GTP) and different time horizon, and show the importance of feedbacks. We do not recommend one metric over another; that is not our role.

The most important (and arbitrary) parameter is the chosen time horizon: do we value more the current generation (20 years from now) or future generations (500 years from now)? This is a moral question, not a scientific one. Therefore, in the 2001 IPCC report, we read, for instance concerning methane (CH₄), a range of values: GWP₂₀ =

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62 ; GWP100 = 23 ; GWP500 = 7 (IPCC 2001, page 388, Table 6.7) Interestingly, the range given in the 2014 IPCC report (AR5) is “narrower”: GWP20 = 84 ; GWP100 = 28 (IPCC 2014, page 731, Table 8.A.1) which does not reflect scientific advances or a more accurate assessment of the metric, but simply a different a priori choice, with the 500-year horizon not being discussed anymore in the last AR5 report. Similarly, using the GTP metric (the effect at final time t) instead of GWP (the effect integrated between gas injection and time t) is a rather arbitrary choice. The use of the global mean temperature (in GTPs), or global mean radiative forcing (in GWPs), is also quite arbitrary, since local impacts do not necessary scale linearly to such global averages. Of course, all these points have been discussed in the literature many times and are well known to specialists. Still, I believe they are so critical and so often overlooked by non-specialists (policymakers, BUT also many climate scientists), than they need to be heavily stressed in papers on GHG metrics like the current manuscript. In particular, the reassessment of GWPs (or GTPs) performed in this manuscript, in order to "remove the carbon-cycle feedback in the denominator", does change the numerical values by, typically, a few percent or less, something very much smaller than, for instance, the arbitrary choice of a time horizon. This needs to be explicitly stated and probably strongly emphasized in the manuscript: comparing GHGs is much more a moral and subjective choice (eg. long-term versus short-term) than a scientific question. Providing accurate estimations of a subjective metric does not lead to an objective metric.

We have added a sentence in the discussion/conclusion to recall that the time horizon remains an important choice when calculating emission metrics: “[Variation in the metrics’ value from including/excluding the feedback] are also less in magnitude than those induced by the choice of the protocol used to calculate the metrics, such as the background conditions (e.g. Reisinger et al., 2011), or by the choice of a given time horizon (see e.g. table 2)”.

The fact that there is a first order uncertainty does not prevent studying a second order one, especially since the first order uncertainty is of political nature whereas the second

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order one is of scientific nature.

The very concept of GWPs/GTPs is based on a simple linear view of the climate system (impulse response functions, transfer functions, Laplace transforms, ...). In order to be physically relevant, it requires the quite strong assumption that there is NO feedback at all in the system (ie. GWPs are fully independent on climate or other GHG levels). Of course, GWPs/GTPs can be diagnosed from complex non-linear systems, but their use as a simple metric is based on the assumption that the climate responds linearly to each individual forcing.

The original purpose of emissions metrics was to compare GHG emissions at the margin (e.g., IPCC FAR). The general concept is to compare one additional kg of different GHGs. In practice, because of the signal-to-noise ratio, large pulses are used to estimate IRFs (e.g. Joos et al., 2013). Though, test are performed to ensure the pulse is not so large as to introduce non-linear responses (e.g. Joos et al., 2013, but also our figure 3).

IRFs and metrics do include some types of feedbacks, with the strong limitation that they are implicitly and linearly accounted for. For instance, the climate IRF implicitly includes the water vapor, lapse rate, cloud-cover and sea-ice feedbacks.

Our paper also demonstrate that it is possible to account for more feedbacks by developing further the IRF framework. And as the work of Joos et al. (2013) shows, despite the feedbacks, it is still possible to look at a linear (marginal) response.

The aim of the paper is therefore to remove the feedbacks in the carbon cycle to better "fit" into the concept of linear systems and therefore provide a more "accurate" quantification of GWPs/GTPs.

This is not the goal of the paper. The first goal is to correct the IPCC mistake by making

the GWPs and GTPs consistent in the way they include the climate-carbon feedback. The second goal is to discuss how the metrics are affected by including or excluding the feedback.

But at the same time, climatologists usually insist in describing climate as a complex non-linear system, with many feedbacks (in particular between climate and the carbon cycle, precisely the one discussed in the paper). This is a point that deserves some extended discussion: To what extent GWPs/GTPs are sound concepts for climate? And to what extent are they simply imperfect tools designed to answer the heavy policy requirement for a metric?

The discussion suggested by the referee is way out of the scope of the paper. Our paper is not a review nor a perspective on the topic. It is based on the existing literature and solves one previously identified issue of the emission metrics.

Further, IRFs and metrics are designed to be used at the margin where linearity holds, and they are used here to compare pulses of GHGs (not emission scenarios where linearity becomes a problem).

I have also a more specific problem with the IRF for temperature. The impulse response functions for carbon (Appendix C.1) have all the same structure: a constant term (= percent carbon staying in the atmosphere "forever") and several decreasing exponentials (= capture of carbon by vegetation and ocean). In contrast, the impulse response functions used for temperature (Appendix C.2) have no constant term. In other words, a basic fundamental ASSUMPTION in the GTP computations is that climate change is fully reversible: whatever the size of the initial radiative "spike" forcing at time zero, climate recovers to its initial conditions after a few centuries. I have some major difficulties to admit such a strong HYPOTHESIS, which stands against all my knowledge in climate science... These response functions are obtained from atmosphere-ocean only

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GCMs simulations (without feedbacks from the surface vegetation changes, land ice cover, deep ocean changes, etc. . .) by fitting one-way experiments (abrupt or gradual 4xCO₂ experiments with stabilization). Is this supposed to be realistic? Interestingly, there are no reversed experiments, even though the IRF functional form assumes reversibility: is this climate reversibility assumption based on something else than just simple convenience?

Again, I understand the requirement for a metric to compare GHG. Obviously, this implies some arbitrary choices and some drastic simplifications of the climate system. Still, I have difficulties with the logic of "fitting" the climate system into a simple linear fully reversible system. I certainly do not share the scientific concept behind. At the very least, these fundamental assumptions should be explicitly stated and discussed in the manuscript.

It is true that full reversibility is implicitly assumed, so we have added the following in the introductory paragraph of the section dedicated to IRFs, to remind the reader of the implicit reversibility of the model: “[IRFs] represent a fully reversible system [. . .]”. With IRFs, however, this reversibility is not instantaneous, and such a model is fully capable of showing the kind of hysteresis one can observe in complex models.

Here, in the specific case of emission metrics, the idea that those are calculated as the contribution of a marginal emission of the considered gas is also important. The marginal emission of the gas is assumed to occur under a given background, but it is not assumed to affect this background. Therefore, under the metric framework there is no issue of irreversibility. The issue only arises if one wants to use the IRFs as first-order models of the climate system, which is not the case in this paper.

Note also that the constant term in the carbon-cycle response is not a proper irreversibility. It is an apparent irreversibility within the time-frame of the experiment used to calibrate the IRF (1000 years). IRFs over a longer time-frame have been proposed, in which case the constant term becomes also a decaying exponential with a time-scale

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much longer than 1000 years (about 80.000 years if we stick to only one exponential to describe this long-term response).

There is a real danger to misrepresent the response of the climatic system, in a "very accurate" BUT certainly not "objective" fashion, as a linear response to the superposition of independent GHG forcings that are not allowed to interact with each other, nor with climate. I am not sure that scientists should blindly misrepresent the real world, only to fit policy requirements of a simple metric. At least, they should be extremely cautious and stress the limitations of the GWPs/GTPs concept.

“Remember that all models are wrong; the practical question is how wrong do they have to be to not be useful.” (Box G .E. P. & Draper N. R., 1987, Empirical Model Building and Response Surfaces).

Though the quote might be overused, it remains – we believe – a fundamental truth about modeling. We argue that the (political) demand for metrics such as GWPs and GTPs makes those useful de facto. We acknowledge the many limitations of those simplistic models. But these have been discussed extensively in the literature, and it is not within the scope of this paper to revisit the question.

I am not a specialist of GHG metrics. I am writing this review just after the interactive comment from M. Sarofim et al. was posted online, and I strongly agree with it. The added value of a more "accurate" assessment of GWPs/GTPs, as presented in this manuscript, comes at the cost of simplicity and reproducibility. Though the scientific methodology presented in this paper is sound and well presented, I am not sure this is the best way to fulfill the requirements of GHG metrics. Fundamentally, GHG metrics are only a "rule of thumb" to decide which GHG is "better" or "worse", from some subjective perspective. Scientists should not try to disguise this "rule of thumb" into an objective, quantified, assessment.

As stated by the reviewer, emission metrics are used to compare GHGs, to decide if one is better or worse than the other from some subjective perspective. Yes, that sums up an emission metric. But, policy makers have a need for such a tool (such as in emission trading). All the authors on this paper are fully aware of the limitations of emission metrics, and have written on the subjective aspects (some extensively). We are in no way trying to “disguise” this subjectivity. This paper is well embedded in the existing literature. The paper is of a technical nature and discusses a technical issue, and therefore readers would go elsewhere for a more detailed discussion of the subjective aspects of metrics (many of which we mention and cite). It is not in the scope of the paper, nor the interests of the readers, to discuss a topic that has been well discussed, reviewed, and assessed by others.

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