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We thank Daniel Johansson for constructive comments. For simplicity, we present his comments in black and our responses in red.

General Comment: This is a valuable paper in the sense that it summarizes clearly the approach of using linear impulse response functions (IRFs) to estimate the GWP, GTP and iGTP/IGTP emission metrics. The title of the paper is somewhat misleading since it is not directly a synthesis; it is more of a background paper on how emission metrics are calculated. Given this the novel issues are limited. However, it is useful piece since it presents how emission metrics based on IRFs are calculated in a systematic and clear way. This has been lacking in the scientific literature. The paper would benefit from a discussion about other approaches to compute metrics such a using reduced complexity carbon cycle and energy balance models instead IRFs. Such approaches are used in a range of metric studies (e.g. Reisinger et al, 2010, Tanaka et al, 2009, Azar & Johansson, 2012). A clear justification why the paper is limited to an IRF based approach would be valuable.

Both reviewers find the title somewhat misleading. To improve the title we are, therefore, removing the word “synthesis.” We also change the wording to focus more on simple emission metrics and to avoid confusion whether or not this article discusses metrics of varying complexity. Hence, the new title is: “Simple emission metrics for climate impacts.”

The reviewer correctly points out that a discussion about different approaches to compute metrics could be useful. There are various types of models that can be used for these calculations. One could apply models that include explicitly physical processes or apply simpler measures or metrics that are calculated based on complex models. Our focus on analytical expressions is not intended to give signal on one method being better than others. However, we think than including all other types of metric approaches is another paper in itself. Hence, we agree with anonymous referee #2 that our scope with the paper was to present analytical expressions of metrics, and, thus, we do not intend to include an extensive presentation of more complex metrics. That is the chosen focus of the paper. However, we agree that the article was lacking an overview of the different metric approaches and an explanation of why our focus was on analytical metrics. We will, therefore, include a paragraph fairly early in the article (in Section 2) that comments the different metric approaches and that we focus in the paper on the simple analytical metrics. The added paragraph in Section 2 (also due to the other reviewers' comment):

“While we here focus on simple analytical metrics using simple parameterizations of the climate system, there is a variety of alternative approaches to develop emission metrics (Tanaka et al., 2010). One can conceptualize “climate models” as spanning from simple analytical models to complex general circulation models or earth-system models (IPCC, 2001; Held, 2005). Reisinger et al. (2010); Tanaka et al. (2009); Azar and Johanson (2012) are examples of studies that use reduced complexity carbon cycle and energy balance models. In general, the more complex the models are, the better they are to handle the processes in the climate system, but at the cost of increasing computational time making them unsuitable for most common metric applications. Simple climate models with shorter computational times are often used as the basis of emission metrics (Tanaka et al., 2009; Wigley, 1998; Manne and Richels, 2001), but these are difficult to represent in reduced analytical form. We focus in this article on analytical expressions to be able to provide a single consistent and transparent analytical framework that can handle a broad range of metric calculations. Despite the simplicity of these metrics, the key parameters are based on more complex climate models ensuring the metric values are realistic.”

Specific comments:

Page 873, line 5-7: The authors write “A limitation of using RF directly is that it does not capture the transient response in the atmospheric concentration when medium to long-lived gases are studied.” This is a little bit unclear. I would say that RF never captures any transients, whether the forcer/gas is short-lived or long-lived. Dynamics is not accounted for in the RF concept as such. With “transient response” I think the authors refer to atmospheric perturbation time of the forcers but I am unsure if they also want to include something more in this.

We agree that this sentence is confusing. We have, therefore, deleted that sentence and replaced with:

“The RF is often used in literature to compare the RF at two points in time, such as the change in RF between current and pre-industrial times (Forster et al., 2007, Figure 2.20).”

Page 876, line 18-19: The authors write “The choice of reference gas is difficult, and the long term behavior of CO₂ is one of the main reasons for needing a value based TH in normalized emission metrics (IPCC, 1990; Lashof and Ahuja, 1990).”. I agree on that the choice of reference gas is difficult, but I find the argument that “the long term behavior of CO₂ is one of the main reasons for needing a value-based TH in normalized emission metrics” is somewhat peculiar. Of course, TH has a large importance on the absolute metric value for CO₂ and consequently for the normalized metric when CO₂ is the reference gas, but arguing that CO₂'s perturbation lifetime is “one of the main reasons for needing a value-based TH” seems to be reverse logic. If we value future impacts differently than impacts today a valued based TH would be needed independent on the life time of CO₂. Reducing the issue of TH to a mere practical issue on how to deal with the long term response of CO₂ is to simplify the discussion too much.

We think this comment is meant for P875/L18-19. We understand the reviewer's point on this sentence, as it is not well written. You are right that there are many issues on time horizon regardless of if CO₂ is the reference gas or not. Our point, which we didn't spell out clearly, was to come back to the origin of GWP. The GWP concept was originally a climate analogue to the ozone depletion potential (ODP), which compares the steady-state ozone depletion for a sustained emission relative to a reference gas. The ODP integrated to infinity (steady state with no discounting). This is not possible with CO₂, as long as you parameterize the IRF for CO₂ to not decay to zero. Hence, a time horizon (less than infinity) was needed because CO₂ became the reference gas. We have improved the text by shortly discussing ODP and also state that choosing CO₂ as a reference gas is a value choice.

Paragraph added in Section 2 to describe ODP and the time horizon issue:

“Together with the climate impact of an emission metric, the time-horizon (or more generally the discount function) is one of the key value based choices in metric design. The Ozone Depletion Potential, which serves a similar purpose to the GWP for ozone depletion (IPCC, 1990), did not use any discounting in effecting choosing an infinite time horizon (Cox and Wuebbles, 1989). In contrast, the GWP requires a discount function for CO₂ to ensure the metric values are finite, and this is because it is generally assumed that a pulse emission of CO₂ does not decay to zero (Lashof and Ahuja, 1990;Archer et al., 2009).”

Sentence added to Section 2.6 describing that the choice of reference gas is always a value choice:

“The choice of reference gas is a value based choice, but an obvious choice is to use the trace gas of primary concern, namely carbon dioxide (IPCC, 1990). There is no obvious need to have only one reference gas or let CO₂ always be the reference gas.”

Page 881, line 16-18. The authors write “The short integrations in CMIP3 make it difficult to estimate the longer time constant, and, hence, the climate sensitivity derived from the IRFs differs from the climate sensitivity of the climate model (Olivie et al., 2012)”. I have not read the paper by Olivie et al. (2012) (seems interest though and I plan to read in the near term future), but one should at least be able to estimate the “effective climate sensitivity “ from the CMIP3 experiments. This value may

however be somewhat different from the “equilibrium climate sensitivity”. I presume that the interesting comparison is between the “effective climate sensitivity” and the parameters derived for the IRFs and not between the “equilibrium climate sensitivity” and the parameters derived for the IRFs.

The reviewer is right in that a climate sensitivity can be calculated, which would be on limited data due to the short time period in the models. We have deleted this paragraph and replaced with a short paragraph after Equation 7 to clarify that the temperature response in the short term can be independent of the total climate sensitivity:

“The time scales to reach the climate equilibrium are given by d_i , and often the time horizon H is less than the longest time scale. Thus, the combination of c_j and d_j are most relevant for the temperature response rather than the equilibrium climate sensitivity λ which may not be reached until after a thousand or more years (Olivié, Peters, Saint-Martin, 2012). While much attention is given to the equilibrium climate sensitivity, the most important characteristic of the IRF for most metrics may be the short term (TH<100year) dynamic behavior.”

Page 881 line 16 – page 882 line 11. I think the discussion here would benefit from a more general discussion about the relationship between the response time of the climate and climate sensitivity. As is well known from rather old literature there is a strong relationship between climate response time and climate sensitivity, see for example Hansen et al, 1984 and Harvey (198X). This aspect is also analyzed in the context of iGTP/IGTP in Azar & Johansson (2012). As the discussion is in the paper now it is rather hard to follow and may cause some confusion.

We agree that a more general discussion of the relationship between the response time of the climate and climate sensitivity would be beneficial. We have therefore deleted the first paragraph and replaced that with an improved discussion after Equation 7:

“The time scales to reach the climate equilibrium are given by d_i , and often the time horizon H is less than the longest time scale. Thus, the combination of c_j and d_j are most relevant for the temperature response rather than the equilibrium climate sensitivity λ which may not be reached until after a thousand or more years (Olivié, Peters, Saint-Martin, 2012). While much attention is given to the equilibrium climate sensitivity, the most important characteristic of the IRF for most metrics may be the short term (TH<100year) dynamic behavior.”

Page 884 line 16-27: I find this discussion somewhat confusing and I cannot really see the point with it. Can the authors suggest any kind of practical impact assessment where it would be relevant to base the radiative efficiency and impulse response function on pre-industrial conditions?

We agree that a discussion about pre-industrial conditions can be seen as irrelevant. We would like to point out that calculations of the radiative efficiency and impulse response function is sensitive to the background assumed. In a changing world, we, therefore, need to be aware of if the parameters should be based on a constant world for simplicity or changing metric values depending on emissions and atmospheric concentrations. Our response to your comment is to just briefly mention the idea of using the pre-industrial level, but mainly have a more general discussion of changing RE. The improved paragraph is changed to:

“Compared to pre-industrial times, the RE in 2005 is 40% lower and may be 50-100% lower in 2100 depending on the future scenario (Figure 5). Even if emission metrics are based on a constant background concentration, the background is usually different when metric values are updated (Reisinger et al., 2011) leading to a different RE. For a scenario background, the RE will change as a function of time within the metric calculation. In both constant and scenario backgrounds, the changes in concentration and hence RE are partially offset by changes in the IRF as a function of concentration (Caldeira and Kasting, 1993; Reisinger et al., 2011). For impact assessment, it can be argued to base the RE on a pre-determined fixed concentration such as pre-industrial concentrations (e.g., Huijbregts et al., 2011). This would ensure that the metric values only change due to updated

scientific information, but would mean that the relative weights of GHGs are based on pre-industrial conditions.”

Page 886, line 21: The authors write “..this context are emissions linked to ozone formation or destruction,..” Please write out that you refer to TROPOSPHERIC ozone (this is at least how I read it).
Agreed, we have included “tropospheric.”

Page 888, line 17. I think “altitude” should be “latitude” here.
Changed to “latitude.”

Page 890 equation 18. I think “,” before “{“ is misplaced.
Agreed

Page 895 line 9. Although I do not want to push my own work I think a reference to Azar & Johansson (2012) is relevant here.
Agreed, reference included.

Page 898 line 15-18. I think the piece of text is somewhat misleading, Manne and Richels did not investigate GWP, they did suggest the GCP (although not using that acronym).
OK, modify text to account for the issue raised: “Some emission metrics have been based on economic models. Manne and Richels (2001) investigated how constraints will affect the pricing of different LLGHGs and compared the consequences relative to the GWP. Recently, the Global Cost Potential (GCP) and Cost-Effective Temperature Potential (CETP) were developed (Johanson, 2012) which show similar characteristics to the Manne and Richels (2001) study.”

Page 902 line 20: A reference to Azar & Johansson (2012) could be relevant here as well.
Agreed, reference included.

Page 906 line 1-3. The authors write “In general, the climate impact is governed by species with strong, but short-lived impact and weak, but long-lived impacts”. What do you want to suggest with this statement? SF6 for example is both strong and longlived?
You are right that this sentence is misplaced. We have moved the sentence to Section 4.3, as this sentence refers to Figure 11. What we meant was that the emission weighted metrics typically have this behavior.

References (in addition to those which are in the paper)

Hansen J., Russell G., Lacis A., Fung I., Rind D., and Stone P.: Climate Response Times: Dependence on Climate Sensitivity and Ocean Mixing, *Science* 229 (4716): 857-859, 1985.

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Tanaka, K., O’Neill, B.C., Rokityanskiy, D., Obersteiner, M., Tol, R., 2009. Evaluating Global Warming Potentials with historical temperature. *Climatic Change* 96, 443-466.

I hope you do find my comments useful!

Daniel Johansson

Interactive comment on *Earth Syst. Dynam. Discuss.*, 3, 871, 2012.