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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Technical note: On comparing greenhouse gas emission metrics" *by* Ian Enting and Nathan Clisby

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Author comment: Response to referee 1

1 The description of Technical Notes is given in the instructions to authors for *Atmospheric Chemistry and Physics* (to which our note was submitted). The apparent lack of a Technical note option for *Earth System Dynamics* seems irrelevant for consideration of our paper.

2 The Laplace transform has been a standard part of undergraduate STEM education for at least 50 years. It has proved a powerful tool that we would commend to anyone who wishes to **extend** our analysis. Nevertheless, in order to ensure

wider understanding of our analysis we have, at each point, provided alternative ways of describing our results. We also give illustrative examples showing how the various metrics operate in the time domain. We propose to add an appendix (draft to be posted separately) giving a frequency-response interpretation in order to aid communication with a wider audience.

3 We would agree that the issue of emission equivalence metrics has been "going down a rabbit hole" (in the sense of *Alice in Wonderland*), as shown by the discussion in successive IPCC reports. What our note does is provide a way of comparing some of the recently-proposed metrics in a way that isn't based on the use of specific climate models and/or specific scenarios.

The additions that we propose in response to reviewer 2 (see response regarding section 3.4 in AC4) indicate how our form of analysis might be applicable for considering metrics based on temperature changes.

In general terms, a metric (for CO_2-equivalence of CH_4) can alternatively be regarded as

- a *statistic* of the CH_4 emission history that captures an equivalent CO_2 influence on climate;
- an *index*, derived from the CH_4 emission history that captures an equivalent CO_2 influence on climate;
- a mathematical transformation (which we write as $\text{GWP}_0 \times \Psi(p)$) of the methane source $S_{CH4}(t')$ to give an 'equivalent' CO_2 source $S_{CO2-eq}(t)$ that generates such an 'index' or 'statistic; — for practical reasons $S_{CO2-eq}(t)$ should depend on $S_{CH4}(t)$ only for earlier emissions, i.e. $t' \leq t$ (see 3.i).

We propose to note this after the additional material noted .

3.i We would agree with the reviewer's comment about the impracticality of "providing credits or debits on the assumption that a country or actor will keep its emissions constant for all times". However none of the metrics that we discuss do this. Credits or debits are based on what actors are doing at the time, in the context of what they have done in the past. We propose to emphasise this characteristic and its importance in the new section on practical implications (see response below on comment on line 165)..

3.ii We see the issue of whether nations (or other actors) sign up for unsustainable targets (and then opt out) as distinct from the choice of metrics. This is confirmed by the history of the Kyoto Protocol, with Canada withdrawing and Russia and NZ not taking on second round commitments

3.iii On the questions of practicality and effectiveness we see our analysis as a tool from clarifying debate – separate from either side of GWP vs. GWP*. See comment regarding line 165.

3.iv At several points, the reviewer notes that our discussion shows the GWP metric doing what it is defined to do. This seems to be missing the point (and says little more than that we appear to have coded our calculations correctly). The point is that the **definition** of GWP leads to a poor specification of equivalence of influences on climate (cf Wigley 1998).

Line by line comments

Line 64 Source is 5th IPCC assessment. (as noted in code).
Proposed change: *reference IPCC, or better still IPCC source*

Line 66 **Proposed change:** *... CO2 contribution, using a GWP of 1, from the oxidation ...*

Line 73 The analysis is specifically for small perturbations. For larger perturbations, the departures from non-linearity are not just from recent line-by-line calculations

but go back to the analysis by Arrhenius of observations by Langley.
Proposed change: *in line 20: with the effect of small perturbations linearised as*

Line 87 not sure if it is the result or the implications that the reviewer doesn't understand. Assuming the latter, we have expanded our words.
Proposed change: *quite close (Enting 2018). Thus in the context of emissions growing with e-folding rate, p, GWP$_H$ with $H = 1/p$ gives approximate FEI equivalence.*

Line 123 The parameter b is not dependent on the annual growth rate.
Proposed change: *See proposed words in response to comment on section 3.4 by referee 2.*

Line 137 Need for greater clarity noted.
Proposed change: *... long term. This has led to the development of metrics based on rates of change. However, [as discussed in new section below?] for emissions trading on shorter timescales, political acceptance is likely to favour metrics that also have equivalent influences in the short term.*

Line 145 In part this comment represents aspects of the mis-interpretation that we discuss in detail below in connection with figure 3. With regard to GWP, it is doing what it does, and that in terms of the influence on climate at one particular time, it is a poor specification of equivalence in this case. (see general comment 3.1v above). This is not a new result – we cite Reilly et al, 1999 (and propose to add Wigley 1998) as an example of a study that points out the problems. The point of Figure 3 is that the other metrics do a lot better. The qualitative behaviour of the various cases could be anticipated from the curves in Figure 1, but we think that a specific quantitative example is valuable.
Proposed change: *....defining emission equivalence for constant sources.*

Line 151 **Proposed change:** *After $t = 150$ the forcing from equivalence defined by* *the Cain et al. 2019 metric (dashed line) starts to increase,. This is due to the contribution that corresponds to 0.25 times GWP when* $S_{CH4}(t) \approx S_{CH4}(t-20)$.

Line 174 noted, bit see general comment 3.iv above.
Proposed change: *CH_4 contribution to radiative forcing ....*

Line 159 We disagree, and find aspects of the comment incorrect. Targets are commonly set in terms of CO_2-equivalent concentrations, related to temperature changes by the equilibrium climate sensitivity. CO_2 concentration equivalence is defined in terms of radiative forcing. We have added words to note the connection. A 'metric' is a different thing from a 'target'. The reviewer's comments about types of metric is irrelevant to our words about targets. The term 'objective function' is usually used to denote a function that is minimised in an optimisation calculation. Most metrics (with the exception of those that incorporate economic aspects) are neither calculated nor defined in that way.
Proposed change: *... radiative forcing targets, commonly expressed as CO_2-equivalent concentrations, can be ... [This whole paragraph may become part of new section on practical aspects]*

Line 156 We think the Lauder et al. analysis is still valid for the specific case for which it was undertaken. However the approach is less suitable for wider application, in part because of the forward-looking aspects. The reverse trade would give present credits for future promises. Lauder et al also considered only the specific case of relating constant CH_4 emissions to one-off CO_2 emissions and does not treat the more general case of non-zero rates of change of CH_4 emissions. Thus the Lauder result becomes a special case of GWP*.
Proposed change: *None*

Line 165 We see our note as providing better understanding GWP vs GWP* and similar metrics. We are keen to keep this analysis separate from discussions of what

might be politically achievable. We think such deeper analysis needs to be done by others with greater expertise in such areas.

However, we propose a new section that brings together mathematical aspects that may bear on practicality and political acceptability.

Fig 3 This comment represents a mis-understanding of what GWP is doing.

a for a source $S(.)$ the atmospheric content at time t is $M_X(t) = \int_0^t R(t-t')\, S_X(t')\, dt'$

b The integrated radiative forcing at time t is $a_X \int_0^t M_X(t')dt' = a_X \int_0^t \int_0^{t'} R(t'-t'')\, S_X(t'')\, dt'' dt'$

c For a pulse source, $\delta(t)$, relation (a) reduces to $M_X(t) = R_X(t)$ and relation (b) reduces to $a_X \int_0^t M_X(t')dt' = a_X \int_0^t R('t)dt'$

d For a unit step source, (a) reduces to $M_X(t) = \int_0^t R(t')\, dt'$

– The fact that the radiative forcing from a unit step (which is, apart for the ramp up, what we are plotting) is the same as the integrated radiative forcing from a unit pulse (which is what defines GWP) reflects the fact that combinations of convolution integrals are commutative and associative. This is an obvious property when using Laplace transforms, but we propose to emphasise it more. Details are given in our response to reviewer 2, regarding section 3.4.

– Thus for the sources of CO_2 and CH_4 (scaled by the GWP for time horizon H) the radiative forcings from a unit step will be equal at time H, and the integrated forcings from a pulse source will also be equal at time H. It is the former case that we plot.

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