

# ***Interactive comment on “A quantitative approach to evaluating the GWP timescale through implicit discount rates” by Marcus C. Sarofim and Michael R. Giordano***

## **Anonymous Referee #4**

Received and published: 12 March 2018

The manuscript makes a useful contribution to the literature by exploring explicitly how different time scales for GWP relate to GHG equivalence ratios based on damage costs and different discount rates. It is clearly written and highly readable. I have no fundamental concern with the technical approach and quantitative results, but I feel the manuscript needs to work a bit harder to develop its value proposition, discussion of results including sensitivity analysis, and finally the conclusions, before it is fit for publication.

I'm comfortable with and largely endorse the comments already posted by Bill Collins and anonymous referee #2, and will try not to repeat the specific points they made.

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My main concerns where I feel the manuscript needs to work harder are as follows:

1) value proposition: it is mainly in the SI that the authors acknowledge prior work that linked GHG equivalencies based on damage costs and discount rates to GWP. I believe this needs to be brought into the main paper up-front, and the authors need to do a better job explaining where their study adds value to those existing studies. For example, one could argue that their approach is simply a reverse reading of Boucher (2012). I don't think that accusation would be justified, but neither is it justifiable for the manuscript not to recognise the fact that a range of studies have already found that discount rates around 2-3% give the same GHG equivalence between CH4 and CO2 as GWP100. In this context, in the discussion, I would have liked to see a better explanation why their GWP100-equivalent discount rate of 3.3% is higher than that derived by both Boucher and Fuglestedt et al.

2) discussion of results including sensitivity analysis: in my view, the authors should include an explicit simulation of results if climate-carbon cycle feedbacks following a pulse emission of CH4 are included. The IPCC AR5 and subsequent studies demonstrated that including this results in a significant increase in the GWP100. This is flagged (p7 of the manuscript) but appears not to have been included in the actual sensitivity analysis. It should be fairly easy to modify the radiative forcing calculations to simulate climate-carbon cycle feedbacks and it doesn't have to change the study design at all. There really is no good justification in my view not to include this, other than this is not how the GWP has been defined historically – but from a scientific consistency perspective, it makes no sense to include an effect for one gas (CO2) but not for the other. Including this in the sensitivity analysis (perhaps as a special case, since this is a binary choice rather than something that can be expressed via a pdf) would at least tell us how important this is when we are concerned about choosing GHG equivalencies based on damage functions and discount rates. I could even live with the authors running this only for a central estimate for all other parameters so we can get an order-of-magnitude sense.

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Related to this, but more difficult to do (hence I would not insist that this is done quantitatively) is consideration of the rate of change as a source of damages. Again this could be parameterised and quantified, but there is a large degree of arbitrariness how much weight to place on rate of change vs amount of change. The manuscript would be much stronger though if it could demonstrate under what circumstances including the rate of change might affect the conclusions, or whether the conclusions might be robust even if rate of change damages have been incorporated within reasonable bounds.

3) interpretation and conclusions: I would endorse some of the comments made by anonymous reviewer #2, that the authors are effectively beating up a strawman. Yes some people have argued that we should simply 'switch' to GWP20, but the more intelligent arguments are all for considering the effect of multiple alternative time horizons to inform abatement decisions and policy choices. See e.g. the conclusions in Levasseur et al 2016 (doi: 10.1016/j.ecolind.2016.06.049) regarding the use of multiple time horizons and metrics in lifecycle assessment. The discussion and conclusions need to add quite a bit of nuance to reflect what those studies actually say, and hence the degree to which this manuscript challenges their conclusions or simply adds another dimension that can help choosing the right metric for the right purpose.

There are two additional points that the discussion and conclusion needs to address:

(a) one is that a key context in which GHG metrics are used are in emission trading schemes, and to help governments evaluate policy choices that directly affect near-term commercial decisions, i.e. policy that would "alter the use of capital in the private sector". So there are very different contexts in which GHG metrics are actually used in climate policy and where different discount rates are commonly applied, and the paper would be stronger and more relevant if it recognised and addressed these explicitly.

(b) The second is a recognition that IAMs used to design cost-minimising emission pathways often use a discount rate of 5%. Given that a(nother) key use of GHG metrics is to help IAMs make trade-offs between different gases with different mitigation costs,

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this should enter into the discussion in this paper. I don't think this materially changes the conclusions since we know that different GHG metrics don't have a massive effect on total mitigation costs (although there is a systematic effect especially when moving towards GWP20), but the issue is not trivial especially for countries or sectors with non-negligible non-CO2/SLCF sources. Some discussion on this is needed.

I believe that all the above points (with the exception of quantifying the effect of including climate-carbon cycle feedbacks for CH4) can be addressed by a careful revision of the text itself. The manuscript needs to avoid what currently appears as the too-simplistic conclusion that "actually, GWP100 is largely ok, let's move on" (which is how I read P8L22). The fundamental finding from virtually all metrics papers is that the right metric depends on the application, and hence it is rather jarring to read a conclusion that continued use of GWP100 is 'reasonable' without any caveat.

I am not repeating the above points in my specific comments below and would be happy for the authors to decide how they can best address them.

Specific comments:

P1L22: insert 'emission' after gases – we're talking about emission metrics here

P2L3: 'endorsed' is too strong in my view for the UNFCCC – 'used' is more factual, I cannot recall an explicit endorsement in the sense that the UNFCCC would have explained and justified its choice.

P2L11: I believe the correct term for GTP is Global Temperature CHANGE Potential

P2L12: the reason why GTP downplays SLCFs is not primarily that it is temperature based but that it is a point metric. iGTP is very similar to GWP.

P2L22-27: editorial only: I prefer if introductions don't include the conclusions but rather focus on making the point of why the conclusions are worth having.

P3L15: shouldn't the N2O effect on CH4 forcing depend on the RCP pathway? Per-

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haps this was done but this isn't clear to me from the text.

P4L9: 'future years are cooler than present': helpful if you could indicate what years we are talking about (presumably you mean after 2200 or thereabouts, depending on the reference period/warming – meaning that much of those will be heavily discounted anyway).

P4L21: here and later, please clarify where you truncate your damage calculations (when I read this sentence, I thought you truncate at 2300, but later (P5L14) it seems you truncate at 2500). You note below that this may matter for very low discount rates. Can you quantify/illustrate this?

P6L4: I think the entire sensitivity discussion should note that projecting damages multiple centuries into the future is increasingly fraught with difficulties. The AR5 chose not to evaluate GWP500 because the authors felt that (deep) uncertainties were simply too large – but here you evaluate damages from temperature responses from forcing 500 years into the future? At least a comment on this is needed here – the discussion of what percentage of total damages occurs up to a given year for CH4 and CO2 is useful in this context and could be linked to this point about uncertainty.

P8L2: I feel the statement “We note no metric designed to tradeoff emission pulses is consistent with stabilization” is too strong. Of course, no metric in itself delivers stabilisation, but almost any metric can be used wisely enough to help countries achieve stabilisation.

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Interactive comment on Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2018-6>, 2018.

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