

## ***Interactive comment on “Comparison of physically- and economically-based CO<sub>2</sub>-equivalences for methane” by O. Boucher***

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### **Reply to Anonymous Referee #1**

The referee's comments appear in blue and our reply in black. It is noted that the referee does not question any of the calculations made in the manuscript but rather their interpretation. Likewise the referee does not comment specifically on Section 4 which is shedding new light in the debate on pulse versus sustained climate metrics. I accept that some of the rationale behind the study was unclear. My intention was not to discredit a particular climate metric. This said, I also have some disagreement with the referee on some of the issues raised, even though these were only tangential to the objectives of the manuscript. Overall it is certainly possible to revise the manuscript in

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a way that will resolve most of the issues raised although some points of disagreement may remain.

**\*2,4: “relative merits”** – I do not believe this paper comes close to assessing the relative merits of the metrics. This can only be done in the context of assessing the extent to which they effectively serve a particular policy purpose, and that aspect is not considered in this paper.

I accept that this sentence is awkward and should be changed in the revised version of the paper. The objectives of the study are better described in the introduction (page 6, lines 6-11) and indeed it was not the objective of the discussion paper to discuss how the metrics relate to particular policies. This said I think there are aspects of climate metrics than can be assessed independently of the policy purpose for the following reasons:

- a) It is interesting to estimate how different climate metrics translate into different CO<sub>2</sub>-equivalences for particular gases, irrespectively of the policy purpose which is not always explicated, not necessarily agreed by all countries, and/or changing in time. There is already a large body of literature on comparing CO<sub>2</sub>-equivalences for particular gases.
- b) Not every climate metric has been developed with a climate policy in mind. Moreover there is a requirement for climate metrics even in the absence of a climate policy. This somewhat weakens the argument made by the referee.
- c) I think that the paradigm that "climate metrics depend on the policy purpose, therefore there is no best climate metric" has somewhat hindered progress and debate on the issue. There are several intrinsic qualities that a climate metric needs to have to be useful. Among these qualities are i) transparency and robustness (is the calculation transparent? are the results robust to physical assumptions?), ii) visibility on future time evolution and iii) adaptability to a potentially changing policy context.

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\*\*2,14: "...falls outside this range..." – following on from the above comment, there is a negative nuance in this phrase which is unjustifiable. The GTP could fall outside the range of the GDP because it (can) serve a different policy purpose. In this case it is an entirely positive thing that it does fall outside the range. In any case, nowhere in the paper is an attempt made to justify the use of 100 years as the GTP time horizon (except by analogy with the use of the 100 year GWP in the application of policy, but this is not sufficient), and so this comparison is spurious.

There was no intent to imply a negative nuance in this sentence. However it is still an interesting fact that the ranges for the 100-year GTP and the GDP do not overlap even though a pretty large set of parameters was used to sample parametric uncertainties in the two metrics. The choice of a 100-year time horizon is not completely arbitrary. A 100-year is more or less a typical timescale on which climate change poses itself as a problem and can be resolved. Even for an ambitious and rapid scenario such as RCP2.6, the mitigation effort keeps increasing until 2060 and remains high in 2100 (van Vuuren et al., 2011). Climate change impacts may be an issue on timescales of several hundred years but it is difficult to project policies on such long timescale, therefore a 100-year time horizon remains a reasonable choice. It is surprising that even with a large range of parameters for damage function and discount rate, the values for cumulative and 100-year end-point metrics do not overlap. I will highlight the issue of the time horizon in the revised manuscript.

\*2, 14-16: "It is legitimate to increase : : :" I do not know what this means – it is presumably legitimate if it serves a policy purpose but I get the feeling from the paper that the author believes there is some fundamental reason that it should increase.

If one accepts that climate change is going to become more and more of a problem over time or is going to require more and more of an effort over time, then it seems rather logical that the CO<sub>2</sub>-equivalence of shorter lived species increases over time. Of course it is possible to construct a climate metric where the methane CO<sub>2</sub>-equivalence

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goes down or goes up and down (e.g. by using a sigmoid damage function or by having a constrain on the rate of climate change). However overall it looks sensible that a climate policy puts an increasing weight on short-lived species as climate change unfolds.

It is possible as well that increased knowledge calls for some revision on the climate policy, which as a result brings down the methane CO<sub>2</sub>-equivalence. This would be the case for instance if the climate sensitivity turns out to be less than expected (which would buy us some time) or if a threshold has been passed unintentionally and there is limited additional damage to be expected until one approaches the next threshold.

\*2,18: "some ad hoc shortening : : :" – I would say that this is close to nonsense. In the context of a target-based climate policy, which seems to be the actual regime the international community favours, there is a well-justified reason for shortening of time horizon, as the target time is approached. This underlies the Manne and Richels approach, the Shine et al (2007) approach and also the Johansson (2011 Climatic Change DOI 10.1007/s10584-011-0072-2) approach – the latter paper is not referred to in this manuscript and should be, as it is rather important.

Thank you for pointing out the Johansson (2012) paper. The two papers complement each other as Johansson (2012) considered the GCP metric while this paper focuses on the GDP.

It is unclear whether a target-based policy is really the actual preference of the international community but this is not the debate here. A target-based policy can indeed provide a target time. However the target time depends on the chosen concentration pathway and the climate sensitivity parameters. One can use a model to find the optimal concentration pathway for a given climate sensitivity in order to obtain the year at which the target climate change is reached, as done for instance in Johansson (2012). However it may not always be easy to define when the target time is. For instance

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the RCP26 scenario shows surface temperature change reaching a plateau between 2060 and 2100 and you would generally expect a successful climate policy to reach the climate target asymptotically. There is an issue on what to do as the target year is approached or passed unless some more sophisticated metric is introduced as done in Johansson (2012). In conclusion I agree shortening the time horizon is not an ad-hoc procedure (I accept the text was a bit awkward here) but it involves a number of assumptions on the concentration pathway (or abatement costs) and climate sensitivity.

\*2,19: "natural increase" – again it is assumed that this increase is fundamentally desirable and again it ignores the fact that in a target-based regime other metrics (the GWP included) could have a natural increase as the target time is approached.

See above.

3,5: "DIRECTLY responsible" – as shown in Forster et al. (2007) the indirect RF of methane may almost double the  $0.48 \text{ Wm}^{-2}$  value.

Yes, this sentence refers to the RF associated with atmospheric methane concentrations, rather than with methane emissions. This will be clarified.

3,26: Since tropospheric ozone is not emitted, this argument is weak, as it should really be applied to the precursors .

Yes, this should be reworded.

3-21 to 4-11: This discussion indicates that the author believes the behaviour of the metric should support the initial conviction of someone proposing a policy response.

Right.

\*5-7 – 5-13: I was very confused by this discussion and believe it may be muddled  
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although this is not my area of expertise. Is cost-effectiveness anything to do with climate damage? I thought the concept of cost-effectiveness is to ensure that some pre-specified policy goal is achieved at least cost. According to Tol et al. cost-effectiveness is not served by the GDP and Johansson's paper, referred to above, is particularly relevant here, as following his approach one would lead to a quite different conclusion to that reached in this paper.

Yes I was confused by Kandlikar (1996). This will be corrected.

5-13: "reconciled" – I think strictly, it should be "reconciled under a restrictive set of assumptions"

OK.

5-25: "reduction" – many countries did not have to reduce their emissions under Kyoto – they had to limit them – for example, starting alphabetically, Australia.

OK.

\*5, 8-9: Given the definition of the GDP in the next section, there is only one outcome of such a comparison and this is clear from the earlier literature – perhaps Peters et al. is the starting point here. The author should not imply that it is somehow a fair comparison when the outcome is known.

I assume the referee refers to page 6 but I am not quite clear what the reviewer means here. Peters et al. did not consider the GDP. Their iGTP corresponds to the GDP with a linear damage function and no discount rate. The discussion paper considers a wider set of uncertainties than previous work. The revised version of the manuscript will also include a second set of damage functions and time-varying discount rate (as presented in the reply to Dr. Reisinger's comment).

\*7,10: What is the rationale for time integrating-damage? Does the literature support time-integrated damage being a useful concept? Assuming a constant background and ignoring discounting, this tells me that when using a quadratic damage function (itself barely justified especially in a global mean context), a 2 degree change for one year gives the same (integrated) impact as one degree for four years, or a half a degree for 16 years. Is there any justification for this? If so, some citation to the literature supporting this is essential. Perhaps this is the reason that transparent physical metrics, for all their faults, have an advantage. At the very least, it is necessary to clearly spell out the heroic assumptions being made here to support the GDP being even plausible as a metric.

The rationale for time-integrated damage is quite obvious. Vulnerability to climate impacts depends on their repetitiveness. A society can in general cope with one crop failure but repeated crop failures can trigger a famine. Many ecosystems can survive to one drought but not to repeated droughts, etc. Many climate impacts (glacier melting, sea-ice melting, Greenland melting, sea-level rise, permafrost thawing) respond to cumulative amount of surface warming. There is ample literature to support time-integrated damage. The concept also underlies most cost-benefit analysis for climate mitigation and somehow underlies the suggestion for a mean global temperature change potential (MGTP) by Gillett and Matthews (2010).

This said it is correct that there is relatively little in the literature to justify a particular time-integrated damage function (e.g., Weitzman, 2010). An exponent function is often chosen to approximate the fact that the damage function is presumably convex (e.g., Tol and Fankhauser, 1998). This is why we consider a range of exponents, not just a quadratic function and present the sensitivity to the exponent of the damage function, not only in a reduced range of 1.5 to 2.5 but also for linear and cubic damage functions. We now also present the sensitivity of the results to a sigmoid damage function (see reply to Dr. Reisinger).

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10, 10-12: This discussion needs a careful caveat – what is true for the AGDP is not necessarily true for the GDP, as there you are balancing the relative behaviors of the target and reference gas.

Yes, this is true in relative terms only (the text can be modified to highlight this better).

10, 22: It needs to be clearly acknowledged that the use of time-horizons in the GWP is equivalent, in a complex way, with the application of discounting – this is clearly spelt out in Fuglestvedt et al (Climate Change 2003) – the author does acknowledge this, in passing, in the conclusions (without supporting reference) but it needs to be more clearly stated here.

This is a good point.

\*11,1-3: “Neither :: are straightforward special cases” – we do not expect them to be from the prior literature

The sentence does not imply that they are expected to be straightforward cases. I am not sure what the point of the referee is.

\*11, 19: Why 100 years for the GTP, with no caveat?

See above for a discussion on the time horizon of the GTP. It is certainly appropriate to mention here again that the GWP and GTP do depend on the time horizon.

11,23: “fairly close” - it is fairly close, by design!

The GDP was not designed to provide results close to the GWP. The 2% discount rate and quadratic damage function correspond to mid-range choices for these parameters.

\*\*13,10-11: “a clear advantage” – why is this an advantage? And “ad hoc shortening” –

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this can be achieved by a shortening of the time horizon as a target date is approached. This is not ad hoc – it is entirely rationale.

See above for a discussion on the time horizon and target time. The text will be modified correspondingly.

15, 21: It is not clear in the equation whether the individual pulses add to the  $\Delta T$  trajectory or not (and hence affect the damage due to subsequent pulses) – with non-linear damage functions, this could be important.

The individual pulses do not add to the  $\Delta T$  trajectory. There is nothing in the equation that suggests it does. The AGDP is for a (small) marginal increase in emission which does not affect the concentration trajectory.

16, 4: "his" – I hope that the argument is gender invariant :-)

Thanks.

\*18, 11: why an "advantage"?

See above.

\*19, 5-6: There are uncertainties in the GTP but these are dwarfed by the uncertainties in the GDP.

I think the point that the uncertainties in the GTP are not well quantified hold, no matter whether they are smaller or larger than for other metrics. Moreover what matters for the metric is not just the absolute uncertainties but the relative uncertainties. The **relative** uncertainties in the GTP are not dwarfed by the **relative** uncertainties in the GDP.

19,19: "radiative effect only"? I thought methane was an ozone precursor, which then has air quality impacts which then has knock-on consequences for the carbon budget,

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via plant damage (see Collins et al. JGR 2010) - maybe these impact can be shown to be relatively small but at the very least the "only" should be replaced by a "predominantly"

Yes, this effect is small but can be mentioned for completeness.

## References

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