

Interactive comment on “Comparison of physically- and economically-based CO₂-equivalences for methane” by O. Boucher

Anonymous Referee #1

Received and published: 18 February 2012

This paper is potentially a very useful discussion to the debate about climate metrics. However, in my opinion this paper is a considerable distance from being acceptable, and requires some fundamental revisions before it can be considered to be so, mostly because of a range of unjustified, and in some cases unjustifiable statements and assumptions. My recommendation is that it be rejected but the author should be encouraged to follow up this study with a more complete and balanced one – I believe the work may benefit from explicit collaboration. At the heart of my comments is the unjustified/unjustifiable assumption that runs through this paper that the GDP is for some reason an ideal (and independent) metric and can be used as the basis for assessing other metrics. Since the prior literature (especially Tol et al. and Peters et al) had established some kind of equivalence between GDP-type metrics and GWP, only one result could follow from such a comparison.

C25

It is easier for me to make my comments in order of appearance, rather than to separate into major and minor, but I will indicate which are major with an asterisk.

*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.

**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

*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.

*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.

*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.

C26

3,5: "DIRECTLY responsible" – as shown in Forster et al. (2007) the indirect RF of methane may almost double the 0.48 Wm⁻² value.

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

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.

*5-7 – 5-13: I was very confused by this discussion and believe it may be muddled 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.

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

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

*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.

*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

C27

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.

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.

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 Fuglestad 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.

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

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

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

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

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.

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

*18, 11: why an "advantage"?

C28

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

Interactive comment on Earth Syst. Dynam. Discuss., 3, 1, 2012.