

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

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I welcome Dr. Andy Reisinger's comments and his encouragement for further work. I reply to the specific comments raised here but you may please refer to my other replies as well.

Dr. Reisinger argues that some uncertainties are systematically ignored because of the form of the damage function chosen $D(\Delta T) = \beta T^\gamma$ in the GDP definition. The issue of the form of the damage function was also raised by referees #1 and #2. I recognised in the discussion paper that the chosen definition GDP is simplified (page 7, line 8 and page 19, lines 14–16) and there is a paragraph discussing issues with the structure of the damage function (page 9, line 19 to page 10, line 12). However I accept that

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this may not be enough; these issues and Dr. Reisinger's caveats need to be stressed further in the conclusion section.

To address these comments, I have also considered a sigmoid damage function which expresses the fact that impacts are initially small and then increase sharply before reaching a plateau. Although the plateau may not be a realistic feature, a sigmoid damage function has the advantage to represent the existence of a threshold around a temperature change and is worth testing here (Ambrosi et al., 2003). The damage function (in arbitrary unit) can be written as

$$D(\Delta T) = 1 + \tanh\left(\frac{\Delta T - \Delta T_1}{dT}\right)$$

where ΔT_1 is the threshold temperature change and dT defines the stiffness of the threshold (taken here to be 0.5°C). A value of ΔT_1 of 2°C and the lower bound of warming scenario are chosen for this test. Several sigmoid functions can also be summed to represent the possible existence of multiple thresholds (e.g., at 4 and 6°C) associated with double and triple total damages and with median and upper bound warming scenarios, respectively. It should be noted however that a damage function that correspond to multiple thresholds of increasing severity becomes similar to an exponent function. Hockey-stick damage functions have also been used in integrated assessment models (Tol et al., 1998). They may be appropriate in cost-benefit analysis, but would cause inconsistency here as the temperature trajectory is prescribed independently.

Fig. 1 shows the time evolution of the methane GDP under a larger set of damage functions and discount rates. It appears that the sigmoid damage function results in up and down in the methane CO₂-equivalence but these stay in the same range as for the exponent damage functions. A smaller value for dT would result in a larger range of values. However there is no literature to support the concept of a rapid transition in damage around a threshold, so a dT of 0.5°C seems appropriate as a first guess.

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Dr. Reisinger further makes the point that "the damage from climate change is not just a function of absolute temperature change, but also of the rate of change". While this is certainly true, I believe this is very difficult to include properly in a simple climate metric for a greenhouse gas like methane (which is the focus of this paper) and I do not think this has been attempted beyond the study by Manne and Richels (2001). The rate of climate change depends on the concentration pathway of long-lived greenhouse gases and methane, the emission pathway of short-lived species such as aerosols and NO_x, and natural variability. At the regional and decadal scales (e.g., 10 years), the rate of change is going to depend even more on natural variability and radiative forcing by short-lived species, and less so on global variations in methane concentrations. It is therefore somewhat illusory to believe that the rate of change can be controlled strongly by methane emissions. At the multi-decadal scale (e.g., 30 years), it is conceivable methane emissions could play a role in controlling the rate of climate change. However minimising the rate of change requires to optimise emissions over time under some constraints, and this approach is not compatible with the simple metric adopted here. It is also questionable to which extent an optimal pathway for long-lived and short-lived species can actually be followed in the real world. Including a constraint on the rate of climate change is out of scope for a simple climate metric such as that proposed in the discussion paper, but the issue ought to be mentioned in the revised manuscript.

Another statement made by Dr. Reisinger is that "the timing of change is also very likely to play a role as it affects the ability to moderate damages via adaptation". He further argues that "the use of a discount rate does NOT take care of this, since the discount rate merely tells us what value we place on the damage that has in fact occurred, not whether the damage occurs in the first place." While I agree with the first statement, I think the second statement is debatable. Discounting the future can also encompass time variations in the level of vulnerability for a given change in the climate. In the classical Ramsey model, the consumption discount rate is given by the following relationship $\delta = \rho + n g$ where ρ is the pure preference for the present, n

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is the elasticity of the marginal utility of individual consumption, and g is the growth rate of real consumption per capita. (Note that δ in this equation corresponds to the ρ parameter in the discussion paper). The first term (pure preference for the present) represents the value we place on the damages that will occur in the future, while the second term can be seen as the fact that we care less future impacts because society gets richer (and hence less vulnerable to climate change). Inversely variations to the Ramsey discounting scheme have been proposed, such as the addition of a negative term or a discount rate that decreases with the timescale (Pearce et al., 2003; Hallegatte, 2008). These modifications to the discount rate can either represent the fact that we care more about future damage or that future society may become more vulnerable to climate change (e.g. because of decreasing natural resources, a growing population or some other reason). The decrease in the discount rate over time can be justified by an uncertainty in future levels of consumption (UK Green Book, 2011) or the transition from an individual to an intergenerational discount rate (Hallegatte, 2008). In light of Dr. Reisinger's comment and this discussion, several additional sensitivity calculations have been performed where I have i) tested discount rates of 0.5% and 5%, ii) considered a discount rate that decreases over time from 3.5% (for years 1 to 30) to 1% (for years beyond 301) as recommended by the UK Green Book (2011) and iii) considered a discount rate that decreases over time but is half of that recommended by the UK Green Book. It should be noted that the 0.5% and 5% are pretty extreme discount rates, which have little support in the literature. The results of these additional sensitivity tests for a quadratic damage function are provided in the table below:

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Discount rate	GDP value
Constant – 0.5%	7.8 / 9.9
Constant – 1%	13.4 / 15.5
Constant – 2%	25.2 / 27.2
Constant – 3%	34.3 / 36.2
Constant – 5%	49.6 / 51.2
Decreasing – from 3.5% to 1%	20.0 / 22.0
Decreasing – from 1.75% to 0.5%	8.4 / 10.6

It is interesting to note the decreasing discount rate recommended by the UK Green Book leads to similar results as for the median value of the discount rate chosen in the discussion paper (2%).

Dr. Reisinger also mentions issues arising from "equity weighting, non-monetised damages, and treatment of the risk of catastrophic damages". While these issues are important to determine the total amount level of mitigation that society should aim for, I would argue that they are more difficult (and probably less important) to account for in the climate metric debate.

Dr. Reisinger further comments that "it is a bold leap of faith to claim that varying the parameters in this very limited formulation actually samples the true uncertainty space, because it does not sample the structural uncertainties that would arise if damages were assumed to be also dependent on the rate and timing of change". I certainly agree that the uncertainty in the GDP only includes parametric uncertainties and not structural uncertainties. This point will be made in the revised manuscript. However it should be noted that i) the analysis spans a rather large interval of parametric uncertainties and ii) the larger the uncertainty range the smaller the impact of additional (independent) sources of uncertainties. I still find it remarkable that a rather large interval of parametric uncertainties result in an uncertainty range for the GDP that is indeed large but probably not so large compared to uncertainties in future methane abatement

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cost.

Finally, Dr. Reisinger comments that the simplified parameterisation for the temperature response (i.e., the function δT^p in the discussion paper) is inferior to the use of an upwelling-diffusion energy balance model as done in Reisinger et al. (2010). I do not believe this to be necessarily the case. The impulse response function has been derived from a full coupled 3D ocean-atmosphere model. Four parameters of the fit have been varied to sample uncertainties in the climate response. Although this is done in a somewhat ad-hoc way, it does sample the uncertainties in the timescales associated with the mixed layer and deep ocean, as well as the uncertainties in the ratio between the fast and slow climate feedbacks. Finally I could not find any discussion in Reisinger et al. (2010) of the fact that "the reason why the uncertainties in GWPs are more limited is because some uncertainties in AGWPs of CO₂ and CH₄ cancel" and it is unclear as to why this might be the case.

References

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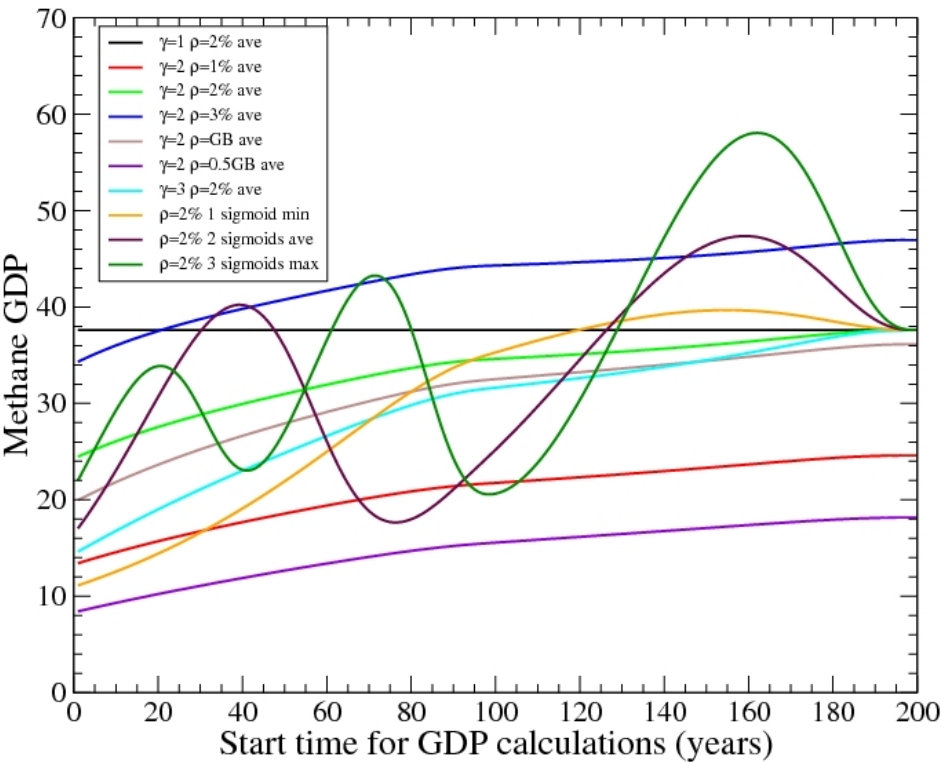


Fig. 1. Methane GDP as a function for the start time for a set of selected damage functions, discount rates and warming scenarios.

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