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Earth System Dynamics Discussions

Interactive comment on "Comparison of physically- and economically-based CO_2-equivalences for methane" *by* O. Boucher

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Reply to Anonymous Referee #2

The referee's comments appear in blue and our reply in black. Minor comments are not addressed here but will be considered in a revised manuscript.

This manuscript does a good job comparing some aspects of global warming potentials, global temperature change potentials, and the author's implementation of the global damage potential. Overall, it is clearly written and interesting. I generally like the logical flow and what the author is trying to accomplish. I particularly liked the presentation of the sensitivities of the GDP to various assumptions. Although my background is not in economics, I also found the discussion relating the economics of CO_2 and

CH_4 emissions reductions with the policy equivalence metric interesting and thought provoking.

I believe that this manuscript can be a useful addition to the literature. However, I do have some concerns that I believe must be addressed before publication. In addition to the specific issues listed below, one of my biggest general concerns involves the calculation of uncertainties. This is an important part of the work, but I do not feel that the uncertainties on the individual terms are suitably justified. Perhaps the most important one involves the discount rate. This is a highly contentious issue, and the uncertainty range has been assumed to be a rather narrow 1-3% per year. As I state below, I strongly believe the lower end of this should be reduced. For example, there has been some work that suggests that large climate change could lead to large areas of the tropics being uninhabitable to mammals. Even though that level of climate change is not expected in the near term, how does one discount away such an effect? Another uncertainty that I do not feel has been thoroughly probed is the damage function. This is highly uncertain, even the functional form, and will be dependent on the climate change impact of interest. Varying the exponent in a single functional form is likely not sufficient. I believe the inadequacies of the assumed parameter uncertainties make the resulting statistics (standard deviation, confidence ranges) of the GDP values far too conservative. It may even affect the mean.

I fully accept that uncertainties on the individual terms need to be suitably justified. I think that most of the uncertainties on the CO_2 and CH_4 budgets and radiative effects can be justified from the published literature. The uncertainties on the climate sensitivity are somewhat ad-hoc because they rely on a parametric fit to a more complex model. However the uncertainty ranges are reasonably large and cover both the short and long timescales of the climate system. The climate sensitivity spans a range of 0.68 to 1.44 K $(Wm^{-2})^{-1}$, which corresponds to a range of 2.5 to 5.3 K for a doubling of CO_2.

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It is much more difficult to settle on a range for the discount rate and I am grateful to the reviewer for raising this issue. It is my understanding that he Stern review, which has been heavily criticised for its low discount rate, uses a discount rate of 1.4% (i.e., 0.1% for the pure preference for the present and 1.3% for the second term expressing an increase in consumption and wealth for future generations). The UK Green Book (2011) recommends a variable rate that decreases from 3.5% for the first 30 years down to 1% beyond 300 years because of uncertainty on future consumption. Hallegatte (2008) showed that the discount rate could also decrease in time because of the transition between an individual to an intergenerational discount rate. Combining the two effects would result in a more rapid decrease of the discount rate over time but Hallegatte (2008) does not provide a lower bound for this. I have done further sensitivity tests for discount rates of 0.5% and 5%, a discount rate that decreases over time according to the UK Green Book and a discount rate that decreases over time but is half of that recommenced 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:

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%). How low the discount rate should be is actually more of an

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issue on how much mitigation humanity should be aiming for rather than what the split should be between CO_2 and CH_4 mitigation.

Accounting for a potential uninhabitability of the tropics is not just a question of how discounting is done but also a question for the form of the damage function. There is little literature support for damage functions fundamentally different from those chosen in the discussion paper. I performed some additional sensitivity studies with a sigmoid damage function. Hockey-stick damage functions have also been used in integrated assessment models (Tol and Fankhauser, 1998). They may be appropriate in cost-benefit analysis, but would cause inconsistency here as the temperature trajectory is prescribed independently. Results are presented in my reply to Dr. Reisinger.

Specific comments (page, line number):

2 23 Clarify “some degree of visibility”

It would take us too far to try and quantify this. This text can be replaced by "enough visibility for the relevant stakeholders".

6 2-4 This is an interesting question. It seems that if you raise it, you should provide some thoughts on an answer. Unless I just missed it, you have not.

The third objective can be reformulated a bit so that this issue is addressed in Section 4. It is clear that emission trading should use a pulse emission metric. It is less clear what sort of metric should be used in national inventories and policy targets.

8 2-3 It would be helpful to have references for the uncertainty ranges adopted included in the table. There needs to be justification for all of the uncertainty ranges. If nothing exists in the literature you should provide reasons for why you settled on these particular values, and perhaps some indication of how much confidence you have in the uncertainties adopted.

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See above.

8 21-22 You are probably making this assumption because you do not have future CO_2 concentrations in your model. You should tell the reader here that you will address the impact of this assumption later in the text.

This assumption is made because I wanted to look at a simple (idealised) climate metric here. As explained in the reply to Dr. Reisinger, I do not believe that these dependencies cause a systematic change in the time evolution of the methane CO_2-equivalence.

10 5 You could use a polynomial fit to demonstrate the sensitivity of the results to the functional form assumption. Since this function is so poorly constrained, and one of your purposes is to examine uncertainties, it is not sufficient to just choose one form and perturb the exponent in a rather narrow range.

See above.

10 15 I assume you mean "unacceptable". If so, provide more explanation here.

The implication of zero discounting is the impoverishment of the current generation because of the need to protect all future generations. This finding would also apply in the future so that each successive generation would find itself being impoverished in order to increase the well-being of the next. This is well explained for instance in Pearce (2003).

There is indeed a debate on the issue of discounting. However I would not justify the need for a very low discount rate by the severity of potential future impacts. There are also some good reasons for discounting the long and very long future. On the centennial timescale, one could argue that buildings will have to be rebuilt anyway. One the

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millennial timescale, other catastrophes could happen, like a nuclear war or an ice age! The risk for catastrophic climate change should be embedded in the damage function rather than in the discount rate. The issue of catastrophic climate change is more relevant for the level of future mitigation rather than for the methane CO_2-equivalence. See above for more discussion on this and possible changes to the discussion paper.

11 5-6 However, it has been shown that an effective discounting is consistent with a GWP time horizon choice, admittedly a discount rate that changes with GHG lifetime. You point this out in the conclusions, but it might be worth mentioning here as well.

Yes, there are some previous studies which can be cited as outlined by Dr. Peters in his comment.

11 8-10 This discussion could be clarified by reminding the reader that the "linear" cost function is consistent with the GWP. Section 3.3 I very much like the way you have explored the sensitivity, first one parameter at a time, and then with a Monte Carlo approach.

Yes.

13 24-26 This is indeed a noteworthy finding. Not surprising, but informative nonetheless. Table 2. More explanation is needed regarding the Monte Carlo runs for GWP and GTP. Why are there no max and min values when you do show a probability distribution?

These will be added.

15 9-11 Provide a physical explanation for why there are such outliers. I assume that several parameters just happen to fall at the correct extreme (e.g., parameters 3, 10, 11, and 12) and it happens very rarely. Also, consider expanding the Figure 4 x-axis to include all values (i.e. out to 64).

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The explanation provided by the referee is correct. This is especially true given the large number of parameters used as inputs to the Monte-Carlo simulation. It is therefore very rare for all the parameters to fall at the extreme values. Note however that even lower or higher values than our min and max are technically possible because 2-sigma values were chosen as the extremes for the Gaussian distributions in the min/max calculation.

15 19 Can you go further on the next page to support this statement? For example, it appears to me that you could show that the sustained emissions approach would yield a different metric depending on the discount rate, emission time, length of sustained period, etc. Because of this, it is more convenient to deal with pulse emissions as you have suggested.

Yes it is worth elaborating on this. One can construct a different metric by assuming sustained emissions. However I would argue that this is not just one more metric, it is a different way of interpreting the same metric.

16 11 You could clarify the justification for this equation. First, make it clear that the X and Y values are marginal cost reductions. Then you can point out that the marginal costs of reduction generally increase with increasing reduction, so that the "investor" will continue to make additional cuts until this equation is satisfied.

The equation is simply expressing the fact that someone investing in emission reduction wants their money back through a C price (whether it is a tax or an emission credit). It is comparing the present value of the costs and revenues of the investment. In that context X and Y are indeed some sort of marginal cost reduction.

16 11-17 I think of the relationship between equations (10) and (11) a bit differently. You could write an equation that is analogous to (10) for CH_4. Then, ratioing the two equations would yield equation (11). Your justification for equation (11) did not appear

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to be as obvious.

Correct.

16 14 There is no discount rate in Eq (1). I assume you mean in Eq (3). But I still do not understand this statement. If the discount rates are not the same, it appears that the RHS of equation (11) would be different from the RHS of equation (13), making it impossible to compare the costs of emission reductions to the metric.

Yes I meant Eq. (3). I think the statement is correct but the same discount rate should be used by the investor and the policymaker for their short-term actions. If a different discount rate ρ_1 was used, both the LHS and RHS terms of Eq. (11) would be discounted with a $1/(1+\rho_1)^t$ term. The LHS terms of Eq. (12) would be discounted as $1/(1+\rho_1)^t/(1+\rho)^{t\prime}$. Both the LHS and RHS terms of Eq. (13) would be discounted as $1/(1+\rho_1)^t$. The point here is that a very low long-term discount rate for ethical reasons does not prevent using a larger discount rate for short-term mitigation decisions.

17 13-14 It seems to be stronger and still correct to say that generally, the CH_4/CO_2 equivalence should not be constant over time.

This is correct but it is not really implied by Eq. (13).

17 13-19 Another implication is that the equivalence depends on the future concentration scenario.

I am not sure to understand why this is the case.

17 19-21 Please provide more discussion about this point. For example, a metric could be used (in place of the GDP) that leads to a constant CH_4/CO_2 equivalence over time. Also, couldn't a metric be used in place of the GDP that would lead to sustained emissions metric making as much sense as a pulsed emissions metric on the right

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hand side of eq (13). I admit I may be missing the point here. If so, please further clarify your statement.

There is no metric on the RHS of Eq. (13). What I meant is that $AGDP_{CO_2}$ could be replaced by any absolute pulse metric for CO_2 and GDP_{CH_4} by any corresponding pulse metric for CH_4. But it would not make sense to have a sustained emission metric in Eq. (13) as it would involve a double integration in Eq. (9).

18 5-6 I am not completely convinced that you have bounded these values. See my earlier comments on the cost function and on discounting, and on uncertainties in general.

I agree that and this statement can be caveated and should be caveated in a revised version.

18 11-12 I suggest rewording this. It sounds like you want an ad hoc way to raise the CH_4/CO_2 equivalence. Past literature has shown that for certain situations, an increase is consistent with policy goals; it is not just something imposed.

The statement is a bit awkward. However it is difficult to imagine a situation for which the role of short-lived species should not increase over time. This could be the case if i) we suddenly realise climate change is less of an issue than we thought and climate policies are softened, ii) climate impacts can be represented by a series of discrete thresholds and we have just passed one of them, iii) we engage in large-scale geo-engineering so that the short-term becomes less than an issue. Accounting for long-term uncertainties would also tend to favour CO_2 over CH_4 until the uncertainties are resolved. There is some logic that as climate change unfolds, more and more importance is given to mitigating short-lived species.

18 12-13 Because of the importance of the damage function to your results, I would like some additional discussion of it. How well can it be bounded, for example. What

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is the justification for it being convex rather than linear? Please provide discussion and additional references.

There is some logic in a convex damage function. The impact of the last third of the Amazon forest (or any other ecosystem) dying is more than the impact of the first third dying. The counter argument would be that there is less extra cost in building the second meter of 2-meter high dyke. The fact that multiple and larger potentially catastrophic impacts are thought to happen at larger temperature changes points towards a convex damage function.

18 18 They are both cumulative.

Correct. Text can be altered.

19 3-6 This makes it sound like you expect the GTP to agree with your GDP, and that the GTP is somehow inferior because it doesn't. By comparing an integrated quantity with a point-in-time quantity, you are really comparing apples with oranges. I suggest rephrasing and making it clear what the comparison really means.

There was not a negative nuance here. However it surprised me and it still surprises me that even with a large range of parameters for damage function and discount rate, the values for cumulative and end-point metrics do not overlap.

19 15 I am not certain what you mean by "across sectors".

"industrial or activity sectors"

19 14-15 Also different impacts (e.g., precipitation vs. sea level) may be characterized by very different damage functions regionally and globally.

This is correct. However since there are many different climate impacts which vary

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across regions and activity sectors and hence with different thresholds, it is likely that an aggregated damage function is relatively smooth.

References

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