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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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The intent of the paper is very useful and it deserves further work to get it published.

I have a major concern though that the research, in the way it is designed, systematically ignores or underplays uncertainties of some of the metrics considered. This is a problem because the article then uses the absolute numbers and the apparently limited uncertainties to arrive at conclusions that really are not justified in their present form, or that would need significant additional caveats.

The most important area in which uncertainties are systematically ignored is the way that GDP is calculated here. It is fine to define the GDP as a metric that measures the damage caused by a pulse emission of a gas, but I don't think it is fine to do this via a

simple damage function of the form D(ΔT) = b$\times\Delta$Tˆy without significant further caveats that affect the conclusions that can be drawn. Rather simplistic integrated assessment models make similar choices for damage functions, but they have also been criticised extensively for the simplifications that they make and it affects the conclusions that can be drawn from results based on such simple damage functions. I certainly don't think it is justified to use this simple damage function in this article without further discussion of its limitations that will need to flow through to the conclusions of the paper.

To explain this concern further: there is plenty of evidence that the damage from climate change is not just a function of absolute temperature change, but also of the rate of change. In addition, the timing of change is also very likely to play a role as it affects the ability to moderate damages via adaptation (i.e. if we have a 1 degree warming tomorrow or in 100 years surely would make a difference to the damage it causes; but this is not captured by the current formulation of the damage function). Note 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. Similarly, the exponent y introduces a time dimension only indirectly in that ΔT increases over time and hence damages increase over time, but it does NOT tell us whether the damages would be greater or lesser if they occurred earlier or later. Additional concerns relate to the fact that the article does not make any reference to the rather extensive literature concerned with distribution of damages and issues arising from equity weighting, non-monetised damages, and treatment of the risk of catastrophic damages.

So, to summarise this concern, I feel that a significantly greater caveat needs to be placed on the uncertainties derived for GDP in this study (and also its median value). It think it's fine for this research to use the simplified damage function just to make it numerically tractable (and there clearly is literature that uses this approach), but 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 think the current approach is well suited to describe the way GDP would change over time (which is likely to be a more robust feature), but it has to be a lot more wary of claiming that the correct absolute value or its uncertainty have been established by this approach. The factors that are important but not included in this work are simply too great. Once this is acknowledged and conclusions softened and modified accordingly, I think this will become a much more robust and relevant contribution to the current discussion about the relationships between different metrics.

A similar though more focused and hence minor concern is that about the apparent small uncertainty of GTP. On page 14 lines 17-23, the author concludes that the difference between his unertainty range for GTP and that derived by Reisinger et al (2010) implies that the uncertainties in GTP are not well understood. Given the very limited and simplified parameterisation employed by this paper, compared to the use of an upwelling-diffusion energy balance model by Reisinger et al, I think the conclusion is simply that if only a very limited number of parameters are included, then one gets much smaller uncertainties. They tell us something about differences in study design but very little about uncertainties in the real world (which we assume the GTP applies to). So this particular conclusion needs substantial revision and simply needs to recognise that the limited sampling of uncertainties substantially limits the conclusions that can be drawn from this study about uncertainties. Reisinger et al (2010) also point out that one of the reasons why the uncertainties in GWPs are more limited is because some uncertainties in AGWPs of CO2 and CH4 cancel (but they don't cancel equally for AGTPs). My suspicion is that in this present study, the uncertainties in AGWPs don't cancel but aren't there in the first place, which leaves the apparent good agreement in uncertainties of GWPs between this study and Reisinger et al somewhat accidental.

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