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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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Like the two previous reviewers, I think the paper is useful and should eventually be published. The figures and uncertainty analysis were interesting, and they raise the interesting issue of complexity versus simplicity! However, at this stage I believe the manuscript leaves a few too many things unsaid. I believe my concerns can be rectified and will lead, I believe, to a much stronger manuscript.

The following are informal thoughts and comments, in rough chronological order:

1. Equations (1), (2), (3): The use of t was a little confusing at first. The variable t is the starting time and not the time horizon (TH). I suggest to put AGWP(t,TH) and explain clearly what t and TH are. Otherwise, this is a nice way of expressing the metrics, as

some parameters like the radiative efficiency is a function of t, a=a(t), and also the IRFs are a function of t, IRF=IRF(t,TH); that is, the IRF would be different if using a different background. It could be worth emphasising this, and I will return to these issues later.

2. Equation (3) and Line 15 on page 7. I think the GDP is put on a different footing to the GWP and GTP from the start; consistent assumptions are not used. I am not (yet) convinced that the GDP is (unintentionally) constructed in a way that has various desirable properties. The GDP is allowed to have a varying background (ΔT), while the GWP and GTP are assumed to have a constant background. Even though if the damage function was linear the ΔT would cancel (making the assumption not matter), it is also the case that the GWP and GTP would have different radiative efficiencies and IRFs if the background changed. I know this is discussed on page 12 in relation to the GWP, but this is a subtly different issue. From my understanding, Reisinger et al (2011) run the model to various years, fix the concentration, infer emissions, rerun with a pulse emission, and derive an IRF (using the IPCC approach) based on a constant background. That is, the starting background, hence radiative efficiency and IRF are different in each run, but the background remains constant. They compute new GWP values based on this process. Having a varying background is different! In that case, you would allow the model to follow a scenario (e.g., RCP), then place a pulse on top of the scenario to derive the IRF and radiative efficiencies (which would then be time varying). Caldiera and Kasting (1993), Enting et al, and Wuebbles et al have all touched upon this. My point of all this, is that a comparison between the GWP, GTP, and GDP really needs to have the same background scenario otherwise you are comparing apples and oranges. I need to be convinced that the GWP and GTP would not increase over time if they also had the background varying as in the GDP.

a. Enting, I.G., Wigley, T.M.L., Heimann, M., 1994. Future Emissions and Concentrations of Carbon Dioxide: Key Ocean/Atmosphere/Land Analyses. CSIRO Division of Atmospheric Research Technical Paper no. 31.

b. Wuebbles, D.J., Jain, A.K., Patten, K.O., Grant, K.E., 1995. Sensitivity of direct

global warming potentials to key uncertainties. Climatic Change 29, 265-297.

3. Extending on the previous point, I am not sure we have the IRFs available to do the analysis. An extension of Reisinger et al (2011) might be needed to achieve a fair comparison. The literature on metrics, unfortunately, does not emphasise the importance of the IRF with different background scenarios. In the case of IRFs for T, there are a really, really small sample to choose from! Would the Boucher and Reddy (2008) IRF be different if the experiment was started with different climate states (1%increase based on pre-industrial, or based on 2000 conditions), or based on a different experiment (not a 1% increase, but following historic emissions, step increase, etc)? I think yes, results will change. There is a larger literature on the CO2 IRF depending on the background and experimental design (Enting et al, Wuebbles et al). I am not suggesting this analysis be done for this paper, but it is an underlying issue in making a comparison with varying backgrounds that should be mentioned.

4. Page 7, line 15 and Page 10, Discounting: There is no reason why a physical metric, GWP and GTP, cannot be discounted (e.g., Lashof and Ahuja, 1990). The TH can be thought of a discount rate (Fuglestvedt et al 2003; Forster et al 2007); where the discount function is a step function which is unity before TH and zero after. It is also possible to back calculate the equivalent exponential discount rate applied for the GWP (Fuglestvedt et al 2003, Table IV) and this paradoxically varies with gas! Fuglestvedt et al (2003) discuss the GDP in some detail and some of this discussion may be relevant here. On the discount rate and exponent, see Figure 5 and text in Fuglestvedt et al 2003, which shows that at 2% and 2 in the GDP approximate the GWP100.

5. Section 2.5: I am not sure I am convinced of this, and some of these points are mentioned in the text. Forster et al 2007 (page 210) argues that the GWP and GTP are special cases of the GDP. If you take the impact (or damages) as RF and linear, and use a discount function that is a step function changing from 1 to 0 at t=TH, then you have the GWP. If you take the impact as temperature and use a Dirac delta function as the discount function, then you get GTP. Following on from Peters et al 2011, I would

also argue that the GWP and iGTP (similar to GDP) are quite similar. I would also argue the GTP is quite similar, as the IRF acts as a discount function too (see you equation 6). If the IRF in equation 6 is equal to one (no physical discounting) then you end at GWP is a subset of GTP. I really think there are lots of connections between GWP, GTP, iGTP, and GDP. They are just not thought about in that way. Peters et al 2011 explores some of these issues in more detail, and in fact has an equation linking, c.AGTP + T in deeper layers = AGWP-iAGTP/lambda.

6. Page 13, lines 5+: There is a common theme along the lines “This is a clear advantage of a GDP-like metric over the GWP and GTP metrics whose values can only be increased systematically by an ad-hoc shortening of the time horizon”. The GTP (and GWP) can be made time varying if it moves to a target year, AGTP(TH-t), which replicates the properties of some economic approaches (Manne and Richels, Shine et al 2007). This is no more, or no less, ad hoc then assuming a discount rate, background temperature scenario, quadratic damage function, etc. While this is mentioned, I think it should be emphasised as the current text does not seem to fully appreciate other ways of having a time varying metric with the same properties that requires less assumptions (and hence less ad-hoc?) then the GDP! For example, the GTP time varying only requires one additional assumption to the standard GTP and that is the inclusion of a target year (e.g., 2050 or 2100) while the GDP requires three extra (Table 1). A time varying GWP would have one additional assumption compared to seven additional assumptions for GDP (Table 1). So, I do not see this property of the GDP as a “clear advantaged”, and I think it could be argued that it is a weakness as it is much less ad-hoc to get this property with the GWP and GTP?

7. Section 4, I think one always has to be careful with pulse versus sustained as they are connected by integration for linear responses. The instantaneous response to a sustained emissions is equivalent to the integrated response of a pulse emission (this is a property of convolutions, see Peters et al 2011). You mention that the same conclusion would be reached “even if a different climate metric had been used”. Thus,

if you show this for a pulse and integrated temperature (or GDP), then it follows that it must hold for a sustained emissions and instantaneous temp as the two equal! So I am not sure that these results are to do with pulse versus sustained, or some other property? Having said that, I did not study this section in detail.

8. Conclusions:

a. As O'Neill 2000 (Climatic Change, "The jury is still out. . .") points out, the simple nature of the GWP may serve a purpose and it is unfair to critique the GWP for something it was not designed to do. I think the same could be said for the GTP. It is worth revisiting this paper as it has many useful points on critiquing metrics. First, the paper finds GDP and GWP are basically the same, but the GDP has more uncertainty due to the additional assumptions. If so, then the GWP would be a "better" metric (same answer, less uncertainty)? Second, as I mentioned at the start, some of the time-varying properties can be put into the GWP and GTP with one additional assumption. Without emphasising this, you are critiquing the GWP and GTP for not behaving the same way that another metric was specifically designed to behave!

b. Based on Peters et al 2011 (and the cited literature there such as O'Neill 2000, Derwent 1990, Wuebbles 1989, etc), it is no surprise that GWP approximates GDP as GWP is basically iGTP which is basically GDP. They are slight variations of the same thing. I would argue GWP$\sim$iGTP$\sim$GDP.

c. On the assumptions, my biggest one would be the varying background in the GDP and not in the GTP and GWP. It may be that the GTP and GWP have the same behaviour as the GDP if a time-varying background is used. I may be wrong on this point, but I think it needs to be shown that the GTP and GWP will not behave in the same way with the same assumptions.

9. Figure 4. Does this really imply that the GWP is an approximation of the GDP? (Just like the GWP is an approximation of the iGTP, Peters et al). If so, then given the simplicity of the GWP in relation to the GDP, wouldn't a policy maker be better off

with the GWP100. I think this figure quite nicely demonstrates the issues of complexity versus simplicity, which I would think is a conclusion of the paper.

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