Earth Syst. Dynam. Discuss., 3, C56–C60, 2012
www.earth-syst-dynam-discuss.net/3/C56/2012/

Earth System Dynamics Discussions

Interactive comment on "On the relationship between metrics to compare greenhouse gases – the case of IGTP, GWP and SGTP" *by* C. Azar and D. J. A. Johansson

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Received and published: 9 March 2012

I agree with the previous reviewer (A. Reisinger) that this is a good, useful paper which clearly explains and illustrates the similarities and differences between the GWP, the IGTP, and the SGTP. I also agree that the reasoning regarding the relationship between gas lifetime and the CO2 decay rate controlling whether the IGTP is larger than or smaller than the GWP is one of the interesting results of the paper (and one that it appears that Peters et al. missed), and is therefore worth bringing into the paper's conclusions.

I do have a number of minor comments and suggestions – those regarding language

choice in particular are optional, as they may reflect my own idiosyncrasies:

P. 114, Line 4-5: Please add the caveat that the asymptotic equal nature is true under "standard" assumptions but may not be under some non-standard assumptions: this was addressed well in the text, and should be noted in the abstract.

Line 24: Language choice: I suggest rephrasing: Perhaps, "See Forster et al. (2007) for the estimates of GWP values developed for the IPCC AR4 assessment".

p. 115, line 27: note that the exponential decay rate is in contrast to CO2 in specific (as every major non-CO2 GHG can also be modeled as an exponential decay rate, not just HFCs): perhaps a parenthetical "since halocarbons (like most non-CO2 gases) have exponential decay rates. . .".

p. 116, line 6: "standard" becomes somewhat better defined later in the paper, but perhaps could use a footnote or explanatory phrase here: perhaps something like, "standard assumptions of linearity in key processes," though I think there are probably better ways to summarize it.

line 9: A reference to Sarofim (2011) comparing SGTP and GWP for CH4 in MAGICC and the MIT IGSM would be appropriate here: the Sarofim paper does not keep a constant background concentration which limits comparability to this study, but does begin to investigate some of the non-linear effects which could make IGTP and SGTP different and is therefore relevant: MC Sarofim, "The GTP of Methane: Modeling Analysis of Temperature Impacts of Methane and Carbon Dioxide Reductions", Environmental Modeling and Assessment, pp. 1-9, August 2011.

line 10: Language choice: This seems to me to be a slightly odd use of the word "warranted": why IPCC "requested", perhaps? Or "why IPCC found that further investigations were warranted"? I think it is okay to say that "New evidence warranted further investigation", but it sounds odd to say that "The detective warranted further investigation"

p. 117, line 15: I agree that holding background concentration is standard. I'd be curious regarding the opinion of the authors regarding whether this is a good choice given that the future concentrations of most gases are likely to rise, but I recognize that this would not be a point of central relevance to this paper.

line 18: typo: "to a give": delete the "a".

line 18: While I do not request a change for this manuscript, I do note that Johansson 2011 (and many similar studies) use calibrations based on a step change in forcing: however, the IGTP and GTP calculations in this paper are based on pulse responses. It would be an interesting sensitivity test to analyze the difference between a calibration based on a pulse rather than a step change. Also, Johansson 2011 appears to be a calibration of a three-box EBM to a UDEBM: Appendix 1 mentions that UDEBMs "have been reported to successfully emulate the global average surface temperature response of AOGCMs" but does not actually give any details on parameterization that are not covered in the paragraphs on pg. 117 and 118 on parameter choices. Perhaps remove the citation, or rephrase the sentence to avoid giving the impression that Johansson et al. shows comparisons to more complex models, or cite another paper?

p. 118, line 3, 5, 10, 17, 20: Meinshausen references should be 2011a.

line 6: language choice: "The warming of the water that downwells in the polar regions is assumed to be increased by a fifth of the increase. . ." Does this mean that total warming is 120% of X, or 20% of X? I presume the former, but please clarify text.

Pg. 119, line 22: language choice: might read more cleanly as ". . .results in the temperature increasing. . ."

p. 121, line 5-6: It might help the reader if the text explicitly notes the inclusion of lambda (eg, climate sensitivity) over lambda in equation 4, in order to convert forcing to equilibrium temperature for the purposes of the discussion which follows.

p. 122, footnote 1: perhaps state explicitly the time period of the shortest time constant

here – eg, 1.186 years if using the IPCC AR4 estimate. (I do note that Johansson 2011 found a shortest time constant of closer to 3 years, so I suppose that might be a rationale for avoiding excess precision here)

Footnote 2: rephrase "parts of the CO_2": perhaps, "a fraction of the increase in atmospheric concentration of CO_2 will remain on longer timescales than the SF_6 lifetime".

p. 124, line 1: typo: perhaps rephrase to "The values differ by at most some 10% in the cases studied here."

Line 17-25: I agree with the sentiments in this paragraph, and am glad that the community appears to be in agreement on many of the key issues regarding this choice (as reflected in IPCC 2009 and some of the discussion in Peters 2011 as well)

Line 19: typo: metric, singular ("a snapshot metric")

p. 125, line 8: "In their paper, it is not explicitly stated in what sense SGTP and GWP are "near-equivalent", and why." Pg. 293 of Shine et al. 2005 has a discussion starting with considering the asymptotic limits of the various equations. What Shine et al. did not do was examine how the SGTP and GWP were _different_, which is what this paper does: but it did show at least one explanation for why they would be expected to have similar values. I suggest deleting or reworking this sentence.

p. 126, line 1: linearity in gas cycles and radiative forcing is standard in estimating GWPs, but linearity in the temperature model is not (because it is not used in the GWP estimation). Separate those into two clauses. Also. . . I think I understand what you mean in this case by "linearity", but because forcing has a square root or logarithmic relationship, this concept should possibly be clarified.

Line 19: typo: ". . .series of pulse emissions." not ". . .series a pulse. . ."

p. 127, line 5: 2011b for the Meinshausen reference.

Line 5: please specify the end-date: it isn't clear if it is 2005 (the end-date for historical radiative forcing data from Meinshausen) or 2006 (as it appears that is the last year used for the GISTEMP data in Fig. B1) (and make sure that the EBM and GISTEMP are appropriately aligned. . . I could see being off by 1 year here)

p. 129, line 10: the closing bracket is large and bold, but I think it should match the opening bracket in the previous line which is normal sized. . .

p. 138: caption should refer to CH4, N2O, and SF6, not just CH4.

p. 141: figure should note which source is used for the instrumental temperature record (Appendix B states that it is GISTEMP, but it would be good to note that within the figure itself).

Last two notes: while I appreciate the insight into the travails of research, I'm not sure that the aside about "just published as we typed the final words" is necessary, and similarly, while the introductory phrase "for the very interested reader" in footnote 1 made me chuckle – I was indeed interested – it may also be unnecessary. But that is a stylistic choice.

Interactive comment on Earth Syst. Dynam. Discuss., 3, 113, 2012.