

## ***Interactive comment on “Carbon dioxide and climate impulse response functions for the computation of greenhouse gas metrics: a multi-model analysis” by F. Joos et al.***

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This manuscript is a well written, important and new synthesis of the GWP and related metrics that has implications ranging from the scientists to the negotiators. It really must be published, but I hope that the authors will consider some revisions that correct from a misreading and that correct some seemingly longstanding misconceptions about time scales. Admittedly the references suggested here are primarily papers I have worked on, but some of these works are relevant to the discussion.

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Cross-cutting problems:

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1-Airborne fraction is discussed, but it is not clearly noted that the value is specific to a given time, and that it depends of course (as the authors have shown) on the historical record of emissions to date. Thus it needs to have a year assigned to it when used.

2-Non-linearity is casually thrown about here, but the examples to demonstrate that the results are not linear are ridiculous, viz, of course the difference between 100 Gt-C and 5000 Gt-C does not scale as the latter puts us into a totally different carbon world. What is needed here is a recognition that the perturbations can and should be linearized (and will scale) if we deal with C pulses that are relevant to the government decisions on mitigation being proposed (1 to 10 Gt-C). The paper presents inadvertently the misleading result that results are always non-linear. I know this is not true because we have the same issues with atmospheric chemistry which is inherently non-linear (bi-molecular reactions), but it is linear for CH<sub>4</sub> perturbations over a wide range (but not pulses of 1000 ppb).

In fact, if the AGWP is as non-linear as stated, then the AGWP reported here for 100 Gt-C is not that for pulses of <1 Gt-C. Did you somehow scale the non-linearity out to get to the linear regime. Since no one would calculate a GWP for CH<sub>4</sub> or N<sub>2</sub>O with a 100 Gt pulse (the chemistry of the atmosphere would be unrecognizable! as would the residence time of that pulse.) Thus what is needed is the AGWP for 0.1 Gt-C. Do you believe that the tabulated values in the paper are appropriate for this.

Abstract p.2 / line 18 Please give the percent for the ‘remainder’ as we are unsure if the 60% is absolute or just the fraction of the 76% taken up.

2/22 Do you not want to note that this depends on the future scenario. Both here and in the text, the mention that it depends very much on which scenario we follow is subdued.

2/25-26 Over what time scales? At the very long it seems like the different IPCC models were very different.

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2/28 Is this stating the obvious that larger pulses have larger integrated effects, or that it is non-linear in relative response. In which case this conclusion really needs to be modified to address the problem noted above.

4/1 “to change.” With what? Scenarios? New knowledge?

4/23 Explain what this (different years, different CO<sub>2</sub>) means to first-order in the value of the AGWP for example.

4/30 Why not give some obvious references here, e.g., [Prather and Hsu, 2010]

5/13 One of the few papers following this “causal chain” that several of us worked on is [Prather et al., 2009]

5/22 Lifetimes should more accurately be ‘time scales’ see later notes and discussion of such terms [Prather, 2007]

6/22-25 This simple-minded approximation should not continue to be propagated in this important paper. Please look at [Prather, 2007] for discussion of the different time scales. We have approximated the impact of long-lived gases as a single e-fold. It is not correct. For example, N<sub>2</sub>O emissions have no effective decay for 2-3 years until they get into the stratosphere. For all chemically reactive species there are indeed several times scales as there are for CO<sub>2</sub>. Unlike what is said in the CO<sub>2</sub> discussion, these are not parametric fits, but are the fundamental modes of that species in the atmosphere. Further the steady-state lifetime of a perturbation (does not work for CO<sub>2</sub> except over >106 yr) is the integrated impact of a pulse (see P2007). It can be approximated as a single e-fold but that is only approximate. Thus in truth all trace gas emissions follow the eqn 5.

7/6 The TAU used in this line for chemical gases should be the steady-state lifetime of a perturbation as this is the weighted sum of the different modes excited by the pulse.

7/9 CO<sub>2</sub> and N<sub>2</sub>O(?) - add another gas here.

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7/12 Add a restriction on your increasing time horizon (e.g., from 20 to 500 yr) This statement fails for very short times (<3 yr) when the CO<sub>2</sub> drops rapidly, but N<sub>2</sub>O and CH<sub>4</sub> decay only slowly!

7/16 SF<sub>6</sub> is an extreme example

7/22 “remains airborne AND in all reservoirs.”

7/25 This structure applies to all gases, not just CO<sub>2</sub>

In this section there is no talk about emission of fossil C that will be converted to CO<sub>2</sub> in the atmosphere (CO, CH<sub>4</sub>, C<sub>2</sub>H<sub>6</sub>). It should be mentioned somewhere.

8/4 Disagree, these are the eigenvalues of the system, not just some fitting. You sort of say this below in line 14

8/24 I remain unconvinced that this statement is true for small pulses. IF you agree, please fix here and throughout.

9/4 By setup do you mean future scenarios? Please

9/23-27 This argument only works if you have evidence that the two results co-vary in a self-consistent model, otherwise there is nothing wrong with what has been done. Please make clear is this is so and you find it from the results here.

11/14 The number quoted 2.123 GtC/ppmv is wrong. For one, the units are ppm in mole fraction, they are not by “volume”. No one measures by volume, and then the virial corrections for CO<sub>2</sub> will change the mole fraction anyway – so no ‘v’. Two, the coefficient appears to be in error, see the recent publication [Prather et al., 2012] that clarifies the mistakes that have been made in converting thus. The dry-air mass of the atmosphere must be different, and one cannot assume that current CO<sub>2</sub> perturbations are well mixed into the stratosphere. The CO<sub>2</sub> number (if one takes 98.5% fill) is 2.086 Gt-C/ppm. Thus there is a 2% bias. The 98.5% is about right given the 2 ppm/yr increase and a 2.5-year lag to fill most of the stratosphere. If 100% it is 2.118.

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11/31 It would be good to know what range of non-CO2 forcing was included? e.g., CH4 lifetime?

15/25 Could the reason here be that the simple models just do not have the degrees of freedom that the physical models do?

16/13 Please give the year for the airborne fraction, it is not constant in general.

18/3-9 Why give the equilibrium response values, they are not relevant?

19/6 No, it could be done with more ensembles.

19/29 Recommend that you repeat the 100 Gt-C number here as a reminder of what the 20 Gt-C is with respect to.

21/8 Is not this 'historical', rather than background, since the history matters.

22/23 Basically the ocean uptake for 5000 Tg-C is flux limited.

23/13 Again, I think this is incorrect for useful ranges of C pulses

24/2 Is this correct? only one model? Then most of the discussion on this page is weak and maybe should be dropped.

24/24 The uncertainty you are talking about cannot be compared directly to the negotiating positions of governments (TH).

25/2 "with a single e-fold perturbation. . .

26/3 There may be other recent papers that followed this causal chain and quantified it, consider referencing [Prather et al., 2009]

26/11 So something other than "man-made" – it does not translate well into American English.

26/14 What do you mean by all greenhouse gases – this result is very dependent on what is included – aerosols, O3, CH4, CFCs. . . .

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I did not review the Appendix.

Prather, M. J. (2007), Lifetimes and time scales in atmospheric chemistry, *Philos T R Soc A*, 365(1856), 1705-1726. Prather, M. J., and J. Hsu (2010), Coupling of Nitrous Oxide and Methane by Global Atmospheric Chemistry, *Science*, 330(6006), 952-954. Prather, M. J., C. D. Holmes, and J. Hsu (2012), Reactive greenhouse gas scenarios: Systematic exploration of uncertainties and the role of atmospheric chemistry, *Geophys Res Lett*, 39, L09803. Prather, M.J., J. E. Penner, J. S. Fuglestedt, A. Kurosawa, J. A. Lowe, N. Höhne, A. K. Jain, N. Andronova, L. Pinguelli, C. Pires de Campos, S.C.B. Raper, R. B. Skeie, P. A. Stott, J. van Aardenne, F. Wagner (2009), Tracking uncertainties in the causal chain from human activities to climate change, *Geophys. Res. Lett.*, 36, L05707.

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 12, 19799, 2012.

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