Reply to comments by Marcus Sarofim

We thank Marcus Sarofim for his insightful comments. Please find our responses to his comments below. The reviewer's comments are copied into the document for ease of reading and appear in blue. Our responses are given in black.

> 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.

We agree on that this discussion is central in the paper, and will include the following sentences about this in the conclusion.

"We also find a rather general relationship between IGTP and GWP. The ratio IGTP to GWP (for a time horizon of say a few years to several hundred years) is slightly higher than one for gases with short life times (say methane), and lower than one for gases with long life times (say SF_6). However, if one consider time horizons for the analysis that stretch from just a few years to several thousand years, it turns out that both methane and SF_6 exhibit similar features. The ratio IGPT to GWP first drops (for both gases to just below unity) and then increase (to at most 10 percent) above unity, and then approach unity asymptotically. For gases with life times around 100 years, like nitrous oxide, this pattern holds too. The reason for this first-dive-and-then-emerge-above-unity pattern has to do with the fact that the life time of CO_2 cannot be captured in a single time constant. "

> 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.

We will change the sentence in the abstract so that it is clear that we have used "standard" assumptions. The sentence will read: "It is shown that GWP and IGTP are asymptotically equal when the time horizon approaches infinity and when standard assumptions about a constant background atmosphere are assumed".

> 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".

Agreed.

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. . .".

We prefer to keep this as it is since Fisher only looked at halocarbons.

p. 116, line 6: "standard" becomes somewhat better defined later in the paper, butperhaps 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.

Agreed, we will now phrase this as "standard assumptions of constant radiative forcing and adjustment time for each unit emission when calculating GWP values."

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.

Thanks for pointing out this reference to us. We will add the following sentence to the paper. "Sarofim (2011) analysed SGTP values for methane under the assumption of varying background concentrations for both methane and CO2. "

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"

Anonymous #2 found the sentence unclear and since it does not add anything to the analysis we will remove it from the text.

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.

It is of course also of interest to analyse what happens with changing background concentrations. As has been shown by Caldeira and Kastings, the AGWP values or CO2 does not change much due to the fact that higher concentrations of CO2 will lead to both lower radiative forcing (because of the logarithmic "nature" of the radiative forcing and a higher fraction of CO2 staying in the atmosphere. Coincidentally these two factors cancel out, and the AGWP value of CO2 remains roughly constant. See Caldeira, K. and J.F. Kasting, Insensitivity of global warming potentials to carbon dioxide emission scenarios, Nature 366, 251–253, 1993. (Contrasting Sarofim's paper with the work by Caldeira and Kasting would thus be interesting). More recently Reisinger et al also did study the impact of bakground scenario fo GWP and GTP value (Reisinger, A., M. Meinshausen, and M. Manning (2011), Future changes in global warming potentials under representative concentration pathways, Environmental Research Letters, 6(2), 024020.)

However, for the case of a gas such as N2O , the AGWP value will be lower under the assumption of increasing N2O concentrations since the marginal forcing in the future will be somewhat smaller. The reason for this is that the radiative forcing of N2O is roughly proportional to the square root of the concentration.

Discussing which background concentration to choose is beyond the scope of the current paper, and there is of course no single correct answer (in part because we do not know where future concentrations will end up).

> line 18: typo: “to a give”: delete the “a”.

Agreed, will do.

> 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.

In this paper we use an upwelling diffusion model and not a three box model, hence the calibration of a tree box model is not of an issue in this paper. (This is different from Johansson 2011 where a three box model is calibrated emulate an upwelling diffusion model.) Anyhow, the issue of calibration of simplified models is interesting and important. One important feature of the models is that if the “complicated” model is linear the fitted parameters in the “simple” model should be the same independent on the forcing scenario used in the calibration process. However, if the “complicated” model is far from being linear the fitted parameters in the “simple” model would be dependent of the forcing scenario (being a pulse, a step or any other forcing scenario). In our case the upwelling diffusion model is linear so in principle it should have been relatively straightforward to fit a simpler model to it.

> 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?

We rewrite line 16-19 to “In order to estimate IGTP numerically, we use an Upwelling Diffusion Energy Balance Model (UDEBM) developed in Johansson (2011).” And we remove the sentence “The model is parameterized to give response in line with more complex climate models” in order to avoid any misunderstanding.

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

Yes, this is now fixed.

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.

We have rewritten this in the following way "assumed to increase by only a fifth of the increase in the global average… "

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

Agreed, text changed as suggested.

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.

Agreed we will write "We have here multiplied the concentration level with λ so as to generate the equilibrium temperature change for a given radiative forcing level (the equilibrium temperature change is given as $T_{eq}=\lambda C_{pX}F_X$ for gas X and correspondingly for CO_2).

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)

Agree, this will be added to the footnote

Footnote 2: rephrase "parts of the CO2": perhaps, "a fraction of the increase in atmospheric concentration of CO2 will remain on longer timescales than the SF6 lifetime".

Agreed

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

Agreed

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")

Agreed, changed to singular.

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.

Yes, the sentence "In their paper, it is not explicitly stated in what sense SGTP and GWP are "near-equivalent", and why." will be removed. With this change plus some minor revisions in this paragraph in our paper we will more accurately reflect the important work by Shine et al (2005).

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.

Yes, we agree. We have now written this as "we assume that radiative forcing and adjustment time for each additional unit greenhouse gas are constant,…", and have moved the statement about temperature into a separate sentence. Thanks for making this observation.

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

Will be fixed. Thanks!

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

This will be fixed.

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)

Thanks for pointing this out, we did a mistake here, the end year is 2005 and there was a half year mismatch between the data. This is now fixed.

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. . .

Agreed, will be fixed.

p. 138: caption should refer to CH_4, N_2O, and SF_6, not just CH_4.

Thanks, will be fixed.

> 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).

Agreed.

> 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.

Agreed, and fixed.