

## ***Interactive comment on***

### **“Carbon dioxide and climate impulse response functions for the computation of greenhouse gas metrics: a multi-model analysis”**

**By F. Joos, R. Roth, J. S. Fuglestedt, G. P. Peters, I. G. Enting, W. von Bloh, V. Brovkin, E. J. Burke, M. Eby, N. R. Edwards, T. Friedrich<sup>1</sup>, T. L. Frölicher, P. R. Halloran, P. B. Holden, C. Jones, T. Kleinen, F. Mackenzie, K. Matsumoto, M. Meinshausen, G.-K. Plattner, A. Reisinger, J. Segschneider, G. Shaffer, M. Steinacher K. Strassmann, K. Tanaka, A. Timmermann, and A. J. Weaver**

***For Atmospheric Chemistry and Physics***

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#### **Overview**

This manuscript compares the radiative forcings and temperature changes attributable to emissions of CO<sub>2</sub>, normalized to emissions, as evaluated by multiple groups that participated in a model intercomparison using carbon-cycle models and coupled carbon-climate models.

Results are presented as so-called global warming potentials, global temperature change potentials, and the like. For example the normalized forcing (absolute global warming potential) of CO<sub>2</sub> integrated over a 100-year time horizon is reported as  $92.7 \times 10^{-15}$  yr W m<sup>-2</sup> per kg CO<sub>2</sub>, with very likely (5-95%) confidence range  $(70 \text{ to } 115) \times 10^{-15}$  yr W m<sup>-2</sup> per kg CO<sub>2</sub>.

My principal concern with the manuscript is that the models that participated in the intercomparison are more or less similar, for the most part descendents or variants of the so-called Bern model of Siegenthaler, Oeschger, Joos, and colleagues (Joos et al., 1996). A common feature of these models is that the amount of CO<sub>2</sub> in the atmosphere attributable to a pulse of emitted CO<sub>2</sub> in excess of the natural (preindustrial) amount decreases with a continually decreasing fractional rate, with a substantial fraction of this excess CO<sub>2</sub> (ca 20%) remaining in the atmosphere for millennia. This treatment of the decrease of excess CO<sub>2</sub> is dominant in the current literature. However it is based entirely on models that assume a more or less static carbon cycle, with the long-time excess fraction being governed mainly by the amount of emitted CO<sub>2</sub> not taken up by ocean inorganic chemistry following equilibration with the gas

phase and complete mixing in the ocean. Simpler representations of the impulse response function of CO<sub>2</sub> as a single exponential decay that are based on the observed rate of uptake of CO<sub>2</sub> by the oceans and terrestrial biosphere (difference between emissions and increase of atmospheric CO<sub>2</sub>) are not included in the intercomparison. Also excluded is a recent model study that shows much greater short-term (100-year) persistence of atmospheric CO<sub>2</sub> than is exhibited in the models examined. The restriction of the intercomparison to this subset of representations of the carbon cycle greatly narrows the range of expected long-term forcing commitment by emitted CO<sub>2</sub> and the range of outcomes of alternative future CO<sub>2</sub> emission scenarios. I elaborate on this concern below.

For these reasons I think that the uncertainty in present knowledge of the fate of excess CO<sub>2</sub> in the atmosphere is much greater than would be inferred from the present manuscript. In my judgment this situation has major implications on the publishability of the manuscript as it stands.

I have numerous additional specific concerns with the manuscript, detailed below.

I also raise some issues of terminology and nomenclature pertinent to the present paper but that go well beyond the present paper and offer suggestions which, if adopted, would greatly enhance the present paper and the field more generally.

### **Major concern**

To my thinking the key results of the study are presented in Figure 1a, which shows the impulse response function (IRF, fraction of a pulse of emitted CO<sub>2</sub> present in the atmosphere as a function of time subsequent to emission) for a CO<sub>2</sub> emission pulse of 100 GtC added to an atmospheric background concentration of 389 ppm for a range of Earth system models, Earth system models of intermediate complexity, and so-called reduced-form models. For time horizon 100 years, a useful time horizon for policy purposes, the fraction of the emitted pulse remaining in the atmosphere is 0.41. The uncertainty on this number from the spread of the model results does not seem to be explicitly stated (in contrast, the uncertainty *is* given for the time integrated IRF). I would encourage the authors to give it also for the IRF at 100 years. Looking at the figure it seems to be  $\pm 0.1$  or so.

So is the cup half empty or half full? If this were the whole story, I would say that the cup is definitely half full, maybe even more than half, in the sense that the research community could report to the policy community that if emissions of CO<sub>2</sub> were halted today or at some point in the near future, one could

expect something like 60% of the excess CO<sub>2</sub> and associated forcing to disappear within 100 years. (I recognize that the models tell us that the decrease of the present excess is not exactly the same as the decrease of a pulse, but I would argue that that difference is of second order because most of the excess CO<sub>2</sub> is fairly new).

However, I do not think that the whole story is represented by Figure 1. My major concern with the figure and the manuscript in general is the limited set of models examined, all of which are multi-compartment models, as specified in the protocol for the intercomparison, which is usefully included in the supplementary material, as follows:

The model must be able to compute the redistribution of anthropogenic carbon among the principal carbon reservoirs atmosphere, land biosphere, and ocean.

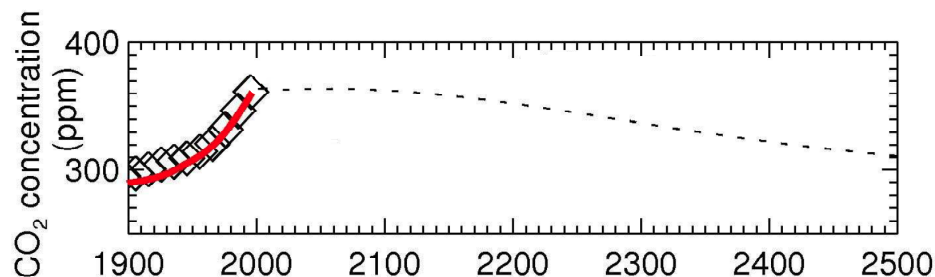
This requirement effectively precludes observationally driven models such as those of Moore and Braswell (1994) and Jacobson (2005). In this respect the following quotations from the recent review by Archer et al. (2009), the authorship of which exhibits considerable overlap with the authorship of this manuscript, are apt:

If fossil fuel CO<sub>2</sub> in the atmosphere was expected to diminish according to linear kinetics, then it would be possible to calculate the lifetime simply using the present-day excess CO<sub>2</sub> concentration in the atmosphere (~100 ppm or 200 Pg C) and the natural uptake rate, currently ~2 Pg C yr<sup>-1</sup> (petagrams of carbon per year) each into the oceans and into the land biosphere. Dividing the inventory by the flux yields an apparent lifetime of 50 to 100 years, depending on whether the terrestrial uptake is counted in addition to the oceanic uptake. This type of calculation has been most recently presented by Jacobson (2005), who determined an atmospheric lifetime of 30 to 95 years. For the nonlinear CO<sub>2</sub> uptake kinetics, as predicted by carbon cycle models, however, this apparent lifetime would increase with time after the CO<sub>2</sub> is released. Some CO<sub>2</sub> from the release would remain in the atmosphere thousands of years into the future, and the atmospheric lifetime of the CO<sub>2</sub>, calculated at that time, would be thousands of years.

The extent and longevity of the climate impact from CO<sub>2</sub> release will also depend on transient uptake by the terrestrial biosphere, which takes up 2 Pg C year<sup>-1</sup> today and shortens the apparent lifetime of CO<sub>2</sub>, but which could become saturated in the coming decades, leaving pCO<sub>2</sub> to

follow the slower uptake kinetics of the ocean (Moore & Braswell 1994). If the terrestrial biosphere, including soil carbon, turned into a new source of CO<sub>2</sub> to the atmosphere at some point in the future (Friedlingstein et al. 2006), then it would act to prolong the apparent lifetime of CO<sub>2</sub>. In addition, high plant primary productivity in a high-CO<sub>2</sub> world may also act to enhance the rate of weathering of soil minerals and bedrock, leading to an acceleration of CO<sub>2</sub> uptake by silicate weathering, the slowest and ultimate sink for fossil fuel CO<sub>2</sub> (Lenton & Britton 2006).

Note that the arguments against the rapid, observationally derived removal rates are theoretical, "as predicted by carbon cycle models" or hypothetical "If the terrestrial biosphere, including soil carbon, turned into a new source of CO<sub>2</sub> to the atmosphere at some point in the future," the latter with a citation to Friedlingstein et al. (2006), which paper also has substantial overlap in authorship with the manuscript under review here. A 50 year lifetime of excess CO<sub>2</sub> would result in an IRF at year 100 of 0.14, well below the range represented in Figure 1 of the manuscript. In the other direction, I call attention to a study by Allen et al. (2009), again with significant overlap in authorship with the manuscript under review here, in which a box-diffusion carbon cycle model was used to calculate CO<sub>2</sub> mixing ratio following abrupt cessation of emissions in year 2000. The finding of that study, shown in the figure below, is that there is essentially no decline in CO<sub>2</sub> mixing ratio over the first hundred years, in great contrast with the results reported in this manuscript. If this model had been included in this study, the range of uncertainty would have been greatly extended upwards. It would thus seem that restricting the analysis of the future course of atmospheric CO<sub>2</sub> presented in this manuscript to carbon cycle models that met certain criteria, and then inferring the uncertainty from the spread of the results from the models examined, was a recipe for severely limiting the uncertainty.



Portion of Figure S3b in the supplementary material of Allen et al. (2009), showing (dashed line) the mixing ratio of atmospheric CO<sub>2</sub> following abrupt cessation of emissions in year 2000.

My point in noting overlapping authorship between the present manuscript and the several studies mentioned is to stress that the authors of the present manuscript should be well aware of these alternative approaches to determining the fate of excess carbon dioxide in the atmosphere. Hence if the present article is to be an assessment of the state of understanding of this important scientific issue rather than a report about an exercise with a fairly constrained set of models, the authors have an obligation to broaden the scope of the models examined or alternatively to inform the reader that they have deliberately restricted the set of models and to detail the consequences of that restriction with respect to the inferences that can be drawn from the study.

This consideration has implications for example even on the concluding statement of the article (page 19829, line 17):

Independent from the choice of emission metric, the long life time of the anthropogenic CO<sub>2</sub> perturbation implies that anthropogenic emissions of CO<sub>2</sub> must be reduced if greenhouse gas forcing and anthropogenic climate change are to be stabilized.

This statement and its position at the conclusion of the manuscript certainly implies that the authors intend for their paper to be of greater significance than a report of a comparison of a restricted subset of models.

In fairness to the authors, the discussion of constraining the *integrated* IRF from estimates of the airborne fraction of emitted CO<sub>2</sub> over the twentieth century (structural uncertainty) and its implications, **pages 19817-19818**, is a strong complement to the assessment of uncertainty based on the range of models that participated in the intercomparison exercise. One can do this calculation on the back of an envelope. Assume an exponential decay for excess CO<sub>2</sub> and assume a lifetime. Ask what is the integrated IRF for 100 years (or equivalently the AGWP (which is the product of integrated IRF and normalized forcing  $A_x$ )). The average integrated IRF (for values of lifetime of excess CO<sub>2</sub> 40, 50, ... 100 yr that are consistent with the airborne fraction of CO<sub>2</sub> in twentieth century observations that served as the basis of the Moore-Braswell and Jacobson studies cited above) is 52 yrs  $\pm$  18% (1- $\sigma$ ) virtually identical with the 52.4 yr the multimodel mean in Table 4. However although the *integrated* IRF is highly constrained by these considerations, the IRF itself, which is perhaps more policy-relevant than the integrated IRF (or AGWP), is not well constrained, with values at  $t = 100$  years ranging (for the above set of lifetimes) from 0.08 to 0.37 (average 0.23  $\pm$  45%, 1- $\sigma$ ) and exhibiting a much lower central value and greater range than the model results shown in Figure 1a.

With respect to the publishability of this manuscript, restricted scope of the paper presents a quandary. It certainly seems worthwhile to publish the results of the intercomparison study, but it would seem essential that the authors strongly qualify their findings by noting that the design of the study effectively limited the range of uncertainty that was found and point out that other approaches to the determination of the rate of decrease of atmospheric CO<sub>2</sub> yield results that differ greatly from those reported in the present study.

### Specific comments

**Page 19805, eq (2).** In the definition of AGWP the authors choose to leave the quantity  $A_x$ , which they denote as the radiative forcing per kg increase in atmospheric abundance of gas x, inside the time integral. In principle this is correct. This normalized forcing might depend on the amount of gas x or other gases in the atmosphere (through overlap of lines) on global mean temperature (through interaction of radiation with clouds) and many other variables that are dependent on secular time. In practice these dependences are not well known if known at all, and even if they were known it would be arbitrary or hard to sort them out and apportion them to the different forcing agents. Hence  $A_x$  is generally treated as a constant, as the authors do later on in the manuscript at page 19806, line 12. It would thus seem that the authors might just as well discuss this matter here and bring the  $A_x$  outside the integral right away.

$$\text{AGWP}_x(\text{TH}) = \int_0^{\text{TH}} \text{RF}_x(t) dt = \int_0^{\text{TH}} A_x \text{IRF}_x(t) dt = A_x \int_0^{\text{TH}} \text{IRF}_x(t) dt$$

The last equality explicitly shows the AGWP for a given time horizon to be the product of the radiative forcing associated with an increment of abundance of the gas and the impulse response function integrated over the time horizon, readily displaying the two contributions to the AGWP and allowing the contributions to the uncertainty in the AGWP to be readily identified.

Pulling the  $A_x$  out of the integral at this stage would make the definition of AGWP much more transparent and might also help the authors better draw the distinction between AGWP (which they denote "an integrated metric") and AGTP (which they denote "an instantaneous (end-point) metric") made at page 19810, which I discuss later.

One might note that the unit of  $\text{RF}_x$  (and for that matter  $A_x$ ) is  $\text{W m}^{-2} \text{kg}^{-1}$  consistent with the unit of AGWP being  $\text{W yr m}^{-2} \text{kg}^{-1}$ . At this point I would only note that the AGWP as defined in eq (2) of the

manuscript is an intensive property of a gas, that is a property of the gas itself and not dependent on the amount of gas introduced into the atmosphere, at least to first order. Definition of such a property (which goes back at least to Shine et al., 1990) is very useful for comparing different gases.

As noted below I recommend also that the expression for the specific forcing for CO<sub>2</sub>,  $A_{CO_2}$ , that is used later in evaluations of the AGWP be given here and not in the results section.

See also below my objection to the use of multi-letter symbols for quantities in algebraic equations.

**Page 19808, line 16.** It appears that the authors meant to say that CO<sub>2</sub>( $t_0$ ) [not CO<sub>2</sub>( $t$ )] is the atmospheric CO<sub>2</sub> inventory at a time when the system was in (approximate) equilibrium.

**Page 19808, line 17** and elsewhere: The term "equilibrium" formally denotes a state in which the requirement of detailed balance is met, namely that all net fluxes on all paths are zero, a given flux from a particular reservoir negated by an equal flux in the opposite direction. The authors certainly mean "steady state", although the term "equilibrium" is widely used in this context. If the authors prefer to use the customary "equilibrium", I suggest that they at least qualify its meaning at first use.

**Page 19809, line 20.** The definition of the quantity  $R(t)$ , denoted by the authors as "response in  $T$  to a unit change in radiative forcing" is erroneous, incomplete, and confusing. First, it is the change in (global mean surface) temperature at time  $t$  that results from a forcing applied at time  $t = 0$ , that is to say, it is a *temporally displaced* response; this property of the quantity is not mentioned in the definition. Second, the quantity  $R(t)$  must be the response (at time  $t$ ) not to a unit forcing (as stated in the manuscript) at time  $t = 0$  but to a *delta function* forcing applied at time  $t = 0$ . Formally, this can be seen from examination of the dimension of eq (10). The dimension of the LHS is temperature change per emitted mass [unit, K kg<sup>-1</sup>]; on the RHS the quantity RF( $t$ ) is forcing at time  $t$  due to emission of 1 kg of material at time  $t = 0$ , [W m<sup>-2</sup> kg<sup>-1</sup>]; cf. Eq (2) of the manuscript, discussed above; the integral is over time [yr]; so the quantity  $R(t)$  must have dimension temperature per forcing per time [K (W m<sup>-2</sup>)<sup>-1</sup> yr<sup>-1</sup>]. Hence  $R$  is not a response to a unit forcing; it is a response (at time  $t$ ) to a short forcing (at time  $t = 0$ ) such that the integral of the forcing over time is 1 W m<sup>-2</sup> yr.

That said, the AGTP defined in eq (10) is, like the AGWP, an intensive property of a given gas.

**Page 19810, line 3.** The authors try to draw a distinction between the AGTP (absolute global temperature potential), which they denote an "instantaneous (end-point) metric," and the AGWP

(absolute global warming potential), which they denote an "integrated metric." I do not see any difference in the nature of the two quantities. The AGWP is the integral of the forcing from the time of emission of 1 kg of material to the time horizon, or equivalently the amount of energy taken up by the planet (per square meter) as a consequence of the emission of 1 kg of material. The AGTP is the change in global mean surface temperature from the time of emission of 1 kg of material to the time horizon as a consequence of the emission of 1 kg of material. I suggest either the authors delete the point or offer a more persuasive explanation of the difference.

**Page 19819, line 7.** The expression for  $A_{CO_2}$  given here depends on the Myhre (1998) expression for forcing given later in the page (line 23), but actually in a new subsection that deals with response in surface air temperature, ocean heat uptake and steric sea level, not forcing. Given the central importance of both these expressions I would recommend that they be broken out instead of inline. The inline equation is correct but cumbersome. Actually, as the Myhre expression for  $CO_2$  forcing was known prior to the present study (indeed, well prior), it is strange that this expression is given here, in the results section. I recommend it be given much earlier in the paper, back at Eq (2) where the quantity  $A_x$  is introduced.

**Page 19819, line 9-14.** Here and in Table 4 it would seem that the authors need to pay much closer attention to specifying the meanings of the uncertainties. The IPCC AR4  $\pm 10\%$  uncertainty in radiative forcing of  $CO_2$  (Forster et al., 2007 p. 131), which denotes the 5-95% range ( $\pm 1.64 \sigma$ ) of the probability distribution function for the forcing, would seem to be the source of the 0.1 that is given in the radical at line 14 along with the 49% (0.49) that is attributed to the uncertainty in integrated impulse response function. Table 4 gives for the 100 yr time horizon the best estimate for time integrated airborne fraction as 52.4 yr and gives the "average of ranges in % of the multi-model mean" as 48.8%, which I take to be the source of the 0.49 in the radical. However the table states that the 5-95% confidence range in the best estimate for time-integrated radiative forcing of  $CO_2$  is (39.6-65.2) yr. Likewise the abstract states that the normalized forcing by  $CO_2$  integrated over a 100-year time horizon is  $92.7 \times 10^{-15}$  yr  $W m^{-2}$  per kg  $CO_2$ , with very likely (5-95%) confidence range (70 to 115)  $\times 10^{-15}$  yr  $W m^{-2}$  per kg  $CO_2$ . These uncertainty ranges correspond to  $\pm 24.4\%$ , not  $\pm 48.8\%$ , so there would seem to be some inconsistency here that needs to be resolved. This uncertainty is discussed also at **page 19818, lines 20-26**, where it is stated that the average uncertainty range (for IRF at TH = 100 yr) is 26 yr, or 49% relative to the best estimate 52.4 yr. The manuscript states:



[T]hese approaches yield an average uncertainty range of 26 yr or of 49% for the 100-yr integrated response (Table 4). We assume that this average range represents approximately a 5–95% confidence range and that it is symmetrically distributed around the 25 multi-model mean to arrive at our best estimates for the mean and 5–95% confidence range for the time-integrated  $\text{IRF}_{\text{CO}_2}$ .

This may be the source of the problem. If the authors are expressing the uncertainty as a  $\pm$  to combine with the similarly expressed uncertainty in the forcing, then it would seem that the 49% range must be divided by 2.

However perhaps more important is the  $\pm 10\%$  uncertainty associated with  $\text{CO}_2$  forcing which the authors take at face value from AR4 (Forster et al., 2007). Although this uncertainty is hoary with age and burnished by repetition, it hardly seems an accurate estimate of the uncertainty that actually attaches to present knowledge of this forcing. Recently Andrews et al. (2012) compared  $\text{CO}_2$  forcings and climate response of 15 atmosphere-ocean general circulation models (GCMs) that participated in the CMIP-5 model intercomparison. Forcing and temperature response coefficient were inferred from the output of the model runs respectively as intercept and slope of a graph of net top-of-atmosphere energy flux versus global mean temperature anomaly subsequent to a step-function quadrupling of atmospheric  $\text{CO}_2$ . (Because the model experiments examined response to a quadrupling of  $\text{CO}_2$ , rather than a doubling, the intercept had to be divided by 2 to obtain the forcing pertinent to doubled  $\text{CO}_2$ ). The forcing is interpreted as an "adjusted forcing" that includes rapid adjustments, mainly of atmospheric structure, that modify the TOA radiative flux on time scales shorter than a year or so. A key finding of the Andrews et al. study is the spread of values of forcing exhibited by the different GCMs, 16%, 1- $\sigma$ . The spread in forcing is a consequence of differing treatments of the radiation transfer in the several models as well as different treatments of clouds that interact with radiation. As the forcing inferred from the analysis of Andrews et al. is an adjusted forcing, it appropriately reflects differences among the models in rapid ( $\lesssim 1$  yr) response of atmospheric structure to the imposed forcing. This spread in forcings inferred from the climate model runs is substantially greater than the uncertainty specified in the IPCC AR4, which gives a 5-95% confidence range ( $\pm 1.64 \sigma$ ) of  $\pm 10\%$ ; i.e., 1- $\sigma$  uncertainty 6.1%. That there is such a range of forcing as inferred from GCM runs should not come as much of a surprise. For example, although the Radiative Transfer Model Intercomparison Project (Collins et al., 2006) reported a 1- $\sigma$  spread in longwave forcing at 200 hPa among the GCMs compared of only 8.5%, that study was restricted to cloud-free atmospheres, with the reason given that "the introduction of clouds

would greatly complicate the intercomparison exercise," from which one infers that the spread of forcing in a model with clouds would greatly exceed that in an idealized cloud-free model. Hence the finding of a 1- $\sigma$  spread of some 16% in the forcings (i.e., 5-95% range  $\pm$  26%, much greater than the  $\pm$  10% reported by AR4) is likely as accurate an assessment of the maximum level of confidence that can be placed in this quantity at present. It would thus seem that it is this uncertainty that should be combined (in quadrature) with the uncertainty in impulse response function to get an accurate measure of the uncertainty in integrated forcing.

**Page 19819, line 23.** As noted above, the forcing equation given here should be given earlier and given its central importance might be broken out instead of in-line. It might also be motivated better. The 47.1 in the equation comes from 100 G ton corresponding to 47.1 ppm, but that is not stated. The 389 in the equation is particular to the starting point of the calculation. This equation is central to the derivation of the  $A_{CO_2}$  given earlier in the page (in a prior subsection). As noted earlier, I think all of this material should be moved up toward Eq (2).

**Page 19820.** Did any of the results presented in Figure 2a use the analytical method involving the  $R$  function of Eq (10)? It is stated that Fuglestedt (2010) used this approach. If that approach was used here, it would seem essential that the  $R$  function be presented, discussed, and justified. If not, perhaps Eq (10) could be omitted and the concern over the definition of  $R$  (see comments above re p. 18906) thereby be finessed.

It is not clear why the "equilibrium" temperature response to these forcings is presented. I guess it is for the comparison with the transient results. But then it would seem more relevant to present the "equilibrium" temperature increase for the several times and forcings not for the "mid-range climate sensitivity of 3 °C" as at line 1 but for the sensitivities of the individual models, perhaps as symbols on Figure 2, so that one might assess how close a given model is to its own "equilibrium" temperature change for that forcing.

The point about fluctuations in temperature in several of the models is important, namely that these fluctuations are characteristic of the control runs. As I examined the figure I had been trying to understand the possible reasons for the fluctuations. That said, I am puzzled why the fluctuations seem to be concentrated at the beginning of the run with little high frequency variation beyond 100 years (where, with the compression of the time axis, they would be expected to appear with much higher frequency on the graph).

**Page 19822, line 26 to page 19823, line 1.** "The response of the land biosphere carbon inventory is associated with considerable uncertainties. It is currently not clear whether the land will continue to act as a strong carbon sink or whether climate change will lead to a loss of land carbon that overwhelms the potentially positive influence of elevated atmospheric CO<sub>2</sub> and nitrogen input on net primary productivity and carbon stocks." This is an important point.

**Page 19826, lines 8-9.** "However, the most important factor that determines the time-integrated IRF<sub>CO<sub>2</sub></sub> and AGWP is the choice of time horizon." This is of course no surprise. It is a consequence of the definition. But it is surprising that it is the concluding sentence of the results section. I refer the authors to my discussion of average IRF vs integrated IRF, below.

**Page 19827, line 18-21.** "The subjective choice of the time horizon dominates uncertainties in the absolute global warming potential of CO<sub>2</sub> and related uncertainties in global warming potential 20 of most other agents. The uncertainty in AGWP (in units of yr Wm<sup>-2</sup> per kg CO<sub>2</sub>) can be mapped to an uncertainty in the time horizon (in units of year)." Again, the time horizon that is used is up to the user; the "subjective choice" of time horizon is not a source of uncertainty in the usual sense that the term is employed in science.

**Page 18927, line 26.** "in the IPCC report"; suggest give year, chapter; even better table number.

**Page 18927, line 27 and Table 7.** I am not sure this table belongs in this paper at all. But if it is to be included, I would certainly recommend that the  $A_x$  values of the several gases be presented as these, together with the IRFs are the fundamentally relevant quantities. Likewise the AGWPs (or better the integrated and/or average IRFs) are more fundamental than the GWPs. Elsewhere (Schwartz, 2012) I have argued that the GWP concept be abandoned in favor of AGWPs, as changing the denominator because of improved understanding of forcing or IRF of CO<sub>2</sub> has the effect of changing the GWPs of gases other than CO<sub>2</sub> even though there has been no change in the understanding of the properties of those gases. The present revision of GWPs is just another instance of the consequences of expressing scientific quantities in non-standard units (i.e., multiples of the AGWP of CO<sub>2</sub>). This point is underscored by Figure 7.

In Figures 1-3 gray shading is used to show the uncertainty range of quantities of interest. It would be of great value to include this gray shading in Figure 7, as I have done in the figure below, from which it is seen that the range of impulse response profiles of the models examined by Joos et al. encompasses and

indeed well exceeds the range of all the impulse response profiles from the prior IPCC reports. So even with the limited subset of carbon cycle models examined in the current study, the uncertainty in the carbon cycle dominates the uncertainty in the GWPs of all non-CO<sub>2</sub> gases. This finding underscores the shifting sands that constitute the foundation on which the GWP edifice is built. The discussion (**lines 10-20**) of the differences between the several IPCC IRFs and that of the multi-model mean of the present study is of course rendered moot because of the large spread in IRFs associated with the models that contribute to the multi-model mean.

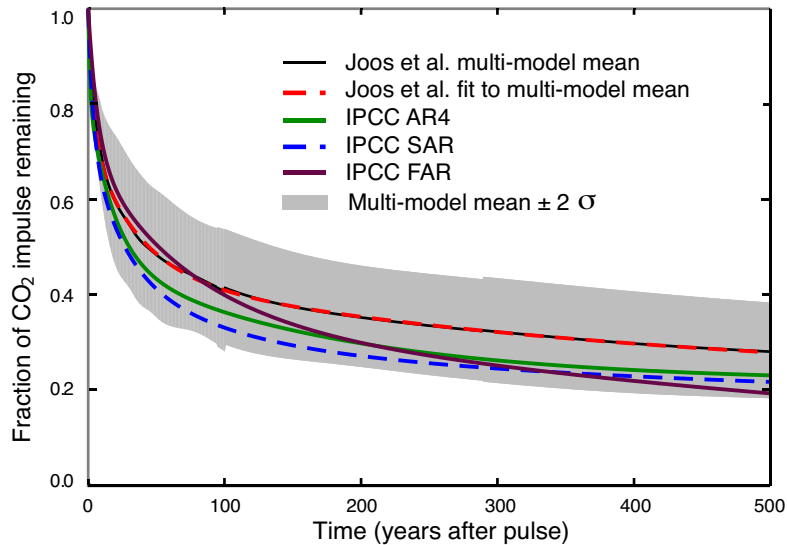


Figure 7 of Joos et al with  $\pm$  two standard deviation range of the multi-model mean from Figure 1 shown by the gray shading.

### Relation to prior work

A key prior model intercomparison is that reported in Archer et al. (2009), which included some of the same models (as well as some of the same authors) of the present study. It would seem appropriate in the introduction to distinguish the two intercomparisons and in the discussion to compare and contrast results. Perhaps there are other intercomparison studies that should be similarly distinguished and compared.

## Figures, tables, appendix

The **figures** are well drawn. The **tables** are informative. It is useful to have the descriptions of all the models (given in the **Appendix**) in one place, but perhaps these (and associated citations) might be relegated to the Supplementary Material rather than take up (magnetic) space in the article itself.

## Definitions and terminology

The terms "global warming potential" and "absolute global warming potential" as defined and used in the present manuscript and in the literature in general are of course misnomers. They do not quantify warming, an increase in temperature. They quantify forcing over a period of time. Thus, despite the widespread use of these terms in the literature, as a reviewer of the present manuscript I feel an obligation to note that these terms are misnomers and encourage the authors to use more appropriate terminology, and the editor to insist on this.

As is made clear in the manuscript under review, the quantity denoted as the "absolute global warming potential" AGWP is the integrated forcing by a gas over a specified time period (horizon), normalized to the amount of emitted gas. Thus it denotes and quantifies integrated forcing. It has units  $\text{W yr m}^{-2} \text{ kg}^{-1}$ , the integral of forcing ( $\text{W m}^{-2}$ ) over time (yr), per emitted material (kg). I suggest that the authors and the community more broadly denote the quantity as "normalized integrated forcing" or "specific integrated forcing," the qualifying adjective "normalized" or "specific" denoting that the integrated forcing is per mass of emitted material. This is an intensive property of a substance; that is, it does not scale with the amount of material (although, as discussed in the manuscript, it may be weakly dependent on the amount of material, for example because of saturation of absorption lines or dependence of the rate constants of chemical or biochemical processes on the concentration of material). One can then speak of a committed integrated forcing that would result from the emission of a given mass of the substance, evaluated as the normalized integrated forcing times the mass of emitted material, units  $\text{W yr m}^{-2}$ , an extensive property of the substance because it scales with the amount of material. Both of these quantities would be functions of the upper limit of the time of integration.

The manuscript under review and the literature in general (and well beyond atmospheric science, into electronics and the like) use the term "impulse response function" IRF to refer to the signal at time  $t_0 + t'$  due to an impulse at time  $t_0$  and normalized to the strength of the impulse. With specific reference to emission of substances into the atmosphere the IRF denotes the fraction of material emitted at time  $t_0$

that remains in the atmosphere at time  $t_0 + t'$ , a function of  $t'$  and perhaps weakly dependent on  $t_0$  and on the amount of material emitted. The use of this terminology in atmospheric science seems appropriate. The term impulse response function might similarly be used to refer to other time-dependent quantities such as forcing and temperature change that result from a pulse emission of material at time  $t_0$ , again normalized to the amount of emitted material, not just to the fraction of emitted material itself. Such use of the term is also made in the manuscript, I think appropriately, for example the impulse response of global mean temperature normalized to a unit emission of a given substance, page 19809, line 13.

The IRF of the amount of material in the atmospheric reservoir plays a special role. This IRF is dimensionless, the fraction of material emitted at time  $t_0$  that is present in the atmosphere at time  $t'$  subsequent to emission.

The time integral of an impulse response function over a period of time, which has dimension time and unit year, is referred to in the present manuscript somewhat clumsily as "time-integrated airborne fraction" or "time-integrated IRF." The end point of the integration is denoted in this community as "time horizon." The time integrated IRF is strongly dependent on the time horizon. Initially this integral scales linearly with the time horizon. As the material is depleted from the atmosphere the integrated IRF is no longer proportional to the time horizon, but falls off from that proportionality. Ultimately, if and when the material is completely depleted from the atmosphere, the integrated IRF becomes a constant. For this reason, at least over the period for which the IRF is non-zero and the integral is increasing, it might make sense to divide the integrated IRF by the time horizon to obtain the *average* IRF over the time horizon. This would make sense especially in the case of CO<sub>2</sub>, for which in the models examined here, the IRF does not reach zero for time horizons of interest, extending out to millennia.

The integrated IRF scales roughly with time and thus exhibits a strong dependence on time horizon, as shown in the excerpt from Table 4 of the manuscript, below, and as commented on at page 19826, lines 8-9, "However, the most important factor that determines the time-integrated IRF<sub>CO2</sub> and AGWP is the choice of time horizon." This dependence is wholly a consequence of definition. In contrast to the integrated IRF, the average IRF, the average fraction of emitted material that remains in the atmosphere over the time horizon, is a much more constrained quantity, bounded between 0 and 1 and decreasing with time horizon, and much less strongly a function of time horizon than the integrated IRF. This property of the IRF readily allows comparisons within the table both down and across, obviating the

need for mentally normalizing to the time horizon. For this reason I would advocate the quantity being reported and tabulated be the average IRF, not the integrated IRF.

Integrated IRF for fraction of emitted CO <sub>2</sub> in atmosphere, yr						
MODEL	time horizon, yr →	20	50	100	500	1000
Bern2.5D-LPJ		13.9	29.7	51.1	163	283
CLIMBER2-LPJ		13.0	26.8	49.2	181	305
DCESS		14.6	31.8	56.3	199	329
GENIE	ensemble	13.6	28.9	50.5	173	n/a

Average IRF for fraction of emitted CO <sub>2</sub> in atmosphere						
MODEL	time horizon, yr →	20	50	100	500	1000
Bern2.5D-LPJ		0.695	0.594	0.511	0.326	0.283
CLIMBER2-LPJ		0.650	0.536	0.492	0.362	0.305
DCESS		0.730	0.636	0.563	0.398	0.329
GENIE	ensemble	0.680	0.578	0.505	0.346	n/a

## Nomenclature

As a reviewer I must express distress over the use of multi-letter symbols (acronyms) such as GWP, RF, TH to denote quantities in algebraic equations in the manuscript under review. This usage flies against long accepted practice in the scientific community. The authoritative document published by the International Union of Pure and Applied Physics, "Symbols, Units, Nomenclature and Fundamental Constants in Physics" (<http://metrology.wordpress.com/measurement-process-index/iupap-red-book/index-iupap-red-book/>) states:

"Symbols for physical quantities should be single letters of the Latin or Greek alphabet with or without modifying signs (subscripts, superscripts, primes, etc.). The two-letter symbols used to represent dimensionless combinations of physical quantities [e.g., Reynolds number,  $Re$ ] are an exception to this rule."

This practice is a long established convention in physical science that leads to improved clarity and communication. As a reviewer I would encourage the authors to adhere to this convention (despite the widespread practice otherwise in the GWP community), and if they choose not to, I would encourage the Editor to insist that they do so.

This notation can get confusing, as the authors themselves seem to concede at page 19809, line 20 where, with reference to their equation 10,

$$AGWP_x(TH) = \int_0^{TH} RF_x(t) R(TH-t) dt$$

they state: "where  $R(t)$  is the response in  $T$  to a unit change in radiative forcing and not to be confused with  $IRF_{T,x}$ ."

While they are at it, I would encourage the authors to use an upright (roman) symbol  $x$  to denote the name of the gas; why upright? because slant (italic) is conventionally used to denote a quantity that has numerical value; upright is used as an identifier. Later on in Eq 9 the upright symbol is used, appropriately, for  $CO_2$ .

Finally, on quantities and units, the relevant quantity for expression of the amount of  $CO_2$  in the atmosphere is molar mixing ratio in dry air (not concentration, which is amount of material per volume), with unit part/part, e.g.,  $\mu\text{mol}(CO_2)/\text{mol}(\text{air})$ , commonly denoted ppm (no v, erroneously for volume) (Keeling et al., 1976; Schwartz and Warneck, 1995).

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