1

We thank both reviewers for their time and effort and their helpful and constructive comments. The original comments by the reviewers are in color. Our reply is in black.

There was apparently a problem with the page and line numbering in the review by Michael Prather. Now, both the page and line numbers indicated in the original review and the page and line numbers corresponding to the numbering of the published manuscript in ACPD are given. We thank the reviewer for revising his page and line numbers.

In addition to the changes requested by the reviewers, we have made some further changes. Two new tables were added. Table 7 shows the response in time-integrated global mean surface air temperature to an emission pulse. Table 5 provides the coefficient to represent the multi-model mean responses by sums of exponentials. The responses as simulated by individual models were also fitted. The coefficients are tabulated in the supplementary information for all models and for the responses in CO_2. Results of the fits are compared with the model output in a complementary figure provided in the supplementary.   Figure 5 (new figure 6) was adjusted; the results for the 5000 GtC pulse were scaled by a factor 1/50. This allows the reader to better compare the results for the 100 GtC versus the 5000 GtC pulse. A few groups slightly revised their output; all numerical values in the MS were checked again and revised where necessary. Revisions in numerical values are minor and do not affect our conclusions.

The revised manuscript is attached (see supplement). All changes in the text are highlighted in color using the "track change mode" of the text editor program.

Review by S. E. Schwarzt

Overview

This manuscript compares the radiative forcings and temperature changes attributable to emissions of CO2, 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 CO2 integrated over a 100-year time horizon is reported as $92.7 \times 10{-}15$ yr W m-2 per kg CO2, with very likely (5-95%) confidence range $(70$ to $115) \times 10{-}15$ yr W m-2 per kg CO2.

My principal concern with the manuscript is that the models that participated in the intercomparison aremore 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 CO2 in the atmosphere attributable to a pulse of emitted CO2 in excess of the natural (preindustrial) amount decreases with a continually decreasing fractional rate, with a substantial fraction of this excess CO2 (ca 20%) remaining in the atmosphere for

46 millennia. This treatment of the decrease of excess CO2 is dominant in the current literature. However
47 it is based entirely on models that assume a more or less static carbon cycle, with the long-time excess
48 fraction being governed mainly by the amount of emitted CO2 not taken up by ocean inorganic
49 chemistry following equilibration with the gas phase and complete mixing in the ocean.

51 We disagree with the reviewer. Unlike stated by the reviewer, most of the models applied in this study
52 are fundamentally different from the early generation carbon models used in the first two IPCC
53 assessments. The exceptions are MAGGICC, ACC2 and Bern-SAR that apply the perturbation
54 approach (Oeschger et al., Tellus, 1975, Joos et al, Tellus, 1996) to describe excess carbon uptake by
55 the ocean. The other models rely on dynamic ocean circulation models and dynamic global
56 vegetation models and include the marine biological cycle and the natural cycling of carbon within the
57 ocean and land as detailed in Table 3, in the appendix, and the underlying literature.

59 Simpler representations of the impulse response function of CO2 as a single exponential decay that are
60 based on the observed rate of uptake of CO2 by the oceans and terrestrial biosphere (difference
61 between emissions and increase of atmospheric CO2) are not included in the intercomparison.
62 The single time scale approach is in conflict with observations. Please see our reply regarding the
63 single life time model below.

65 Also excluded is a recent model study that shows much greater shortterm (100-year) persistence of
66 atmospheric CO2 than is exhibited in the models examined.
67 We did not include this model. However, unlike stated by the reviewer, the persistence of atm. CO_2
68 after a pulse input is not greater than by the models examined here. Please see our detailed response
69 below.

71 The restriction of the intercomparison to this subset of representations of the carbon cycle greatly
72 narrows the range of expected long-term forcing commitment by emitted CO2 and the range of
73 outcomes of alternative future CO2 emission scenarios. I elaborate on this concern below. For these
74 reasons I think that the uncertainty in present knowledge of the fate of excess CO2 in the atmosphere
75 is much greater than would be inferred from the present manuscript. In my judgment this situation has
76 major implications on the publishability of the manuscript as it stands.

78 I have numerous additional specific concerns with the manuscript, detailed below.
79 I also raise some issues of terminology and nomenclature pertinent to the present paper but that go
80 well beyond the present paper and offer suggestions which, if adopted, would greatly enhance the
81 present paper and the field more generally.

83 Please see our point-by-point reply.

86 **Major concern**
87 To my thinking the key results of the study are presented in Figure 1a, which shows the impulse
88 response function (IRF, fraction of a pulse of emitted CO2 present in the atmosphere as a function of
89 time subsequent to emission) for a CO2 emission pulse of 100 GtC added to an atmospheric
90 background concentration of 389 ppm for a range of Earth system models, Earth system models of
91 intermediate complexity, and so-called reduced-form models. For time horizon 100 years, a useful
92 time horizon for policy purposes, the fraction of the emitted pulse remaining in the atmosphere is 0.41.
93 The uncertainty on this number from the spread of the model results does not seem to be explicitly
94 stated (in contrast, the uncertainty *is* given for the time integrated IRF). I would encourage the authors
95 to give it also for the IRF at 100 years. Looking at the figure it seems to be ± 0.1 or so.

97 **Done.** Value and uncertainty in IRF for t=100 is 0.41±0.13 as stated at the beginning of section 4.1

This statement is not true. The ensembles by the GENIE and Bern3D-LPJ models are observationally driven. The parameters in these models were determined such as to match a set of target observations within uncertainties. Further, as noted by the reviewer further below, we were also open to include a linear programming approach that is also explicitly constrained by observations.

In the case of the Bern3D-LPJ ensemble, the parameter distribution within a sample of 5000 simulations is constrained by the following broad set of observations in a probabilistic approach.

- Seasonal CO2 (GLOBALVIEW-CO2) Average seasonal cycle at nine sites.
- fAPARa (SeaWiFS) Seasonal climatology (2-D field)
- NPP (EMDI class A) Estimates from about 80 sites worldwide.
- NPP (FLUXNET) Estimates from about 140 sites worldwide.
- Soil C Low/mid-latitude soil carbon content 2-D field south of 50N
- High latitude soil carbon content 2-D field North America, north of 50N
- Global soil carbon content
- Vegetation carbon. Estimates from about 140 sites worldwide.
- Global vegetation carbon inventory 550±200 GtC
- WOA Temperature (T) Surface (2-D) and 3-D climatological fields
- WOA Salinity (S) Surface (2-D) and 3-D climatological fields
- WOA Phosphate (PO4) Surface (2-D) and 3-D climatological fields
- GLODAP Alkalinity (Alk) Surface (2-D) and 3-D fields (1995)
- GLODAP CFC-11 Surface (2-D) and 3-D fields (1995)
- GLODAP Dissolved inorganic carbon (DIC) Surface (2-D) and 3-D fields (preindustrial)
- GLODAP 14C Surface (2-D) and 3-D fields (preindustrial)
- CO2 Atm. record from ice-core analysis; Time series 1850-1958
- Direct atm. pCO2 measurements, Time series 1959-2010
- Net ocean carbon uptake rates; Global mean 1959-2006, 1990-1990, and 2000-2006
- Net land carbon uptake rates; Global mean 1959-2006, 1990-1990, and 2000-2006 86
- SAT anomaly; Northern hemisphere SAT (HadCRUT3) Annual mean time series 1850-2010
- Southern hemisphere SAT (HadCRUT3) Annual mean time series 1850-2010 87
- Ocean heat Ocean heat content anomaly, Global mean time series 1955-2011 (0-700 m)
- Ocean heat content anomaly; Global mean time series 1993-2008 (0-700 m)
- Ocean heat uptake; Global mean 2005-2010

144　Obviously, this approach includes much more observational constraints than considered by the study
145　of Jacobson 2005; Jacobson uses the recent rise in atmospheric CO_2 as a single constraint.

146　In this respect the following quotations from the recent review by Archer et al. (2009), the
147　authorship of which exhibits considerable overlap with the authorship of this manuscript, are apt:

148　　If fossil fuel CO2 in the atmosphere was expected to diminish according to linear kinetics, then it
149　　would be possible to calculate the lifetime simply using the present-day excess CO2 concentration
150　　in the atmosphere (~100 ppm or 200 Pg C) and the natural uptake rate, currently ~2 Pg C yr−1
151　　(petagrams of carbon per year) each into the oceans and into the land biosphere. Dividing the
152　　inventory by the flux yields an apparent lifetime of 50 to 100 years, depending on whether the
153　　terrestrial uptake is counted in addition to the oceanic uptake. This type of calculation has been
154　　most recently presented by Jacobson (2005), who determined an atmospheric lifetime of 30 to 95
155　　years. For the nonlinear CO2 uptake kinetics, as predicted by carbon cycle models, however, this
156　　apparent lifetime would increase with time after the CO2 is released. Some CO2 from the release
157　　would remain in the atmosphere thousands of years into the future, and the atmospheric lifetime
158　　of the CO2, calculated at that time, would be thousands of years.

159　　The extent and longevity of the climate impact from CO2 release will also depend on transient
160　　uptake by the terrestrial biosphere, which takes up 2 Pg C year−1 today and shortens the
161　　apparent lifetime of CO2, but which could become saturated in the coming decades, leaving pCO2
162　　to follow the slower uptake kinetics of the ocean (Moore & Braswell 1994). If the terrestrial
163　　biosphere, including soil carbon, turned into a new source of CO2 to the atmosphere at some
164　　point in the future (Friedlingstein et al. 2006), then it would act to prolong the apparent lifetime
165　　of CO2. In addition, high plant primary productivity in a high-CO2 world may also act to enhance
166　　the rate of weathering of soil minerals and bedrock, leading to an acceleration of CO2 uptake by
167　　silicate weathering, the slowest and ultimate sink for fossil fuel CO2 (Lenton & Britton 2006).

168　Note that the arguments against the rapid, observationally derived removal rates are theoretical, "as
169　predicted by carbon cycle models" or hypothetical "If the terrestrial biosphere, including soil carbon,
170　turned into a new source of CO2 to the atmosphere at some point in the future," the latter with a
171　citation to Friedlingstein et al. (2006), which paper also has substantial overlap in authorship with the
172　manuscript under review here. A 50 year lifetime of excess CO2 would result in an IRF at year 100 of
173　0.14, well below the range represented in Figure 1 of the manuscript.
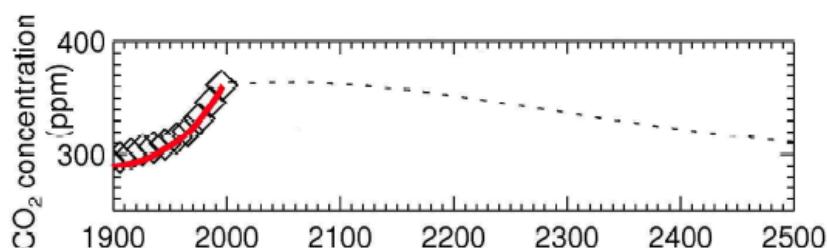
Portion of Figure S3b in the supplementary material of Allen et al. (2009), showing (dashed line) the
mixing ratio of atmospheric CO_2 following abrupt cessation of emissions in year 2000.

174

175　In the other direction, I call attention to a study by Allen et al. (2009), again with significant overlap in
176　authorship with the manuscript under review here, in which a box-diffusion carbon cycle model was
177　used to calculate CO2 mixing ratio following abrupt cessation of emissions in year 2000. The finding
178　of that study, shown in the figure below, is that there is essentially no decline in CO2 mixing ratio

179   over the first hundred years, in great contrast with the results reported in this manuscript. If this
180   model had been included in this study, the range of uncertainty would have been greatly extended
181   upwards. It would thus seem that restricting the analysis of the future course of atmospheric CO2
182   presented in this manuscript to carbon cycle models that met certain criteria, and then inferring the
183   uncertainty from the spread of the results from the models examined, was a recipe for severely
184   limiting the uncertainty.

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

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

195   Independent from the choice of emission metric, the long life time of the anthropogenic CO2
196   perturbation implies that anthropogenic emissions of CO2 must be reduced if greenhouse gas
197   forcing and anthropogenic climate change are to be stabilized.

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

201   In fairness to the authors, the discussion of constraining the *integrated* IRF from estimates of the
202   airborne fraction of emitted CO2 over the twentieth century (structural uncertainty) and its
203   implications, **pages 19817-19818**, is a strong complement to the assessment of uncertainty based on
204   the range of models that participated in the intercomparison exercise. One can do this calculation on
205   the back of an envelope. Assume an exponential decay for excess CO2 and assume a lifetime. Ask
206   what is the integrated IRF for 100 years (or equivalently the AGWP (which is the product of
207   integrated IRF and normalized forcing A_x). The average integrated IRF (for values of lifetime of excess
208   CO2 40, 50, ... 100 yr that are consistent with the airborne fraction of CO2 in twentieth century
209   observations that served as the basis of the Moore-Braswell and Jacobson studies cited above) is 52
210   yrs ± 18% (1-σ) virtually identical with the 52.4 yr the multimodel mean in Table 4. However although
211   the *integrated* IRF is highly constrained by these considerations, the IRF itself, which is perhaps more
212   policy-relevant than the integrated IRF (or AGWP), is not well constrained, with values at $t = 100$
213   years ranging (for the above set of lifetimes) from 0.08 to 0.37 (average 0.23 ± 45%, 1-σ) and
214   exhibiting a much lower central value and greater range than the model results shown in Figure 1a.

215   With respect to the publishability of this manuscript, restricted scope of the paper presents a
216   quandary. It certainly seems worthwhile to publish the results of the intercomparison study, but it
217   would seem essential that the authors strongly qualify their findings by noting that the design of the
218   study effectively limited the range of uncertainty that was found and point out that other approaches
219   to the determination of the rate of decrease of atmospheric CO2 yield results that differ greatly from
220   those reported in the present study.

221 We disagree with the view of the reviewer that the uncertainty ranges given in this study are not
222 appropriate.

223 We agree with the reviewer that it is important to estimate uncertainties ranges. This should be done
224 with care and by using available information and observations to the extent possible. Providing too
225 small uncertainty ranges is misleading. Equally misleading is to provide too large uncertainty
226 estimates.

227 First, we discuss the model presented by Allen et al., Nature, 2009. The following statement and thus
228 the conclusions drawn by the reviewer are wrong: In the other direction, I call attention to a study by
229 Allen et al. (2009), …. The finding of that study, shown in the figure below, is that there is essentially
230 no decline in CO2 mixing ratio over the first hundred years, in great contrast with the results
231 reported in this manuscript.

232 Unlike suggested, it is not appropriate to compare the results for an experiment where emissions
233 increase over the industrial period and are stopped at 2000 AD with the results of an impulse
234 experiment in the case of the model used by Allen et al.

235 As detailed in the Supplementary Information of Allen et al., their "very simple climate model"
236 assumes that

237   1) 15% of emissions are removed instantaneously from the atmosphere
238   2) 25% of emissions are removed with an e-folding time scale of about 200 years
239   3) 50% of emissions are removed by diffusion
240   4) 10% of emissions stay airborne for millennia

241 Term 1 and 2 alone bring the fraction of the initial CO_2 pulse remaining airborne to 0.75 within 100
242 year and to 0.62 within 500 year. Term 3 reduces the airborne fraction further.

243 As stated by Allen et al. "With appropriate choices of parameters this model can reproduce the
244 response of more complex ESMs to a pulse injection of CO_2"

245

246 Next, we discuss the single life time model of Jacobson. The model of Jacobson (2005) is in conflict
247 with observations and not adequate to represent a perturbation of the global carbon cycle.

248   Jacobson represents the carbon cycle by assuming that CO_2 decays in the atmosphere according to a
249 first order reaction:

250 $$\frac{d\mathrm{CO2}(t)}{dt} = E(t) - \frac{CO2(t)}{\tau}$$

251 Here, CO_2 is the atmospheric concentration, E anthropogenic carbon emissions, and τ a timescale. It
252 is no surprise that this single equation works to represent the atmospheric CO_2 increase over the
253 industrial period. This increase is driven by approximately exponentially increasing emissions. It is
254 basic calculus that many systems with a wide range of intrinsic time scales respond to an exponential
255 forcing exponentially and with a single "apparent" time scale.

256   The agreement of this model with the 20th century atmospheric CO_2 record is not a sufficient
257   justification for the single time scale model. To illustrate this we construct a model that is as simple
258   as possible and constrained by atmospheric CO_2 and get a "constant airborne fraction model":

259   $$\Delta CO_{2,atm}(t) = c \cdot I_{Emission}(t)$$

260   Where $\Delta CO_{2,atm}$ is the perturbation in the atmospheric CO_2 inventory, c is a constant (the airborne
261   fraction of the recent decades, about 0.5) and $I_{Emission}$ the time-integrated anthropogenic carbon
262   emissions. Both the constant airborne fraction model and the single time scale model are able to
263   approximately match the observed CO_2 record for the carbon emission history from fossil and land
264   use sources. However, both models are purely diagnostic, applicable to approximately exponential
265   forcing only, and do not represent the functioning of the global carbon cycle.

266   The single life time model is in conflict with observations.

267   a) It is known that CO_2 is chemically stable under current environmental conditions and not
268   oxidized as other chemical species (e.g. CH_4). Thus, CO_2 does not undergo a first order decay.
269
270   Anthropogenic carbon is redistributed among the major carbon reservoirs in the Earth
271   System. These are the ocean with an inventory of 38000 GtC, the atmosphere (280 ppm =590
272   GtC; 390 ppm=828 GtC), vegetation on land (~600 GtC) and soils (~3000 GtC). The marine
273   biosphere (3 GtC) is too small and exchange with ocean sediments and the geosphere occur
274   on multi-millennial time scales.
275
276   b) CO_2 is exchanged between the atmosphere and the mixed-layer of the ocean where it reacts
277   with water to form carbonate and bicarbonate ions. The air-sea exchange rate is
278   approximately known from experiments and observations (e.g., Wanninkhof, JGR, 1992) and
279   the typical time scale to equilibrate the surface layer with an atmospheric perturbation is
280   about 1 year. The carbonate chemistry in the ocean is well known (Zeebe and Wolf-Gladrow,
281   2001) and observational data firmly establish the relationship between pCO_2 (or fCO_2, the
282   fugacity), dissolved CO_2, bicarbonate and carbonate ions . (Revelle and Suess, 1957) showed
283   that the Revelle factor (or buffer factor) which is defined by the relative change in pCO_2
284   divided by the relative change in dissolved inorganic carbon (Delta-pCO_2/$pCO_{2,0}$)/(Delta-
285   DIC/DIC0) is about 10. This means that the relative change in dissolved inorganic carbon is
286   ten times smaller than the relative change in pCO_2 in a water parcel. Accordingly, the relative
287   perturbation in the oceanic concentration is ten times smaller than the perturbation in pCO_2.
288   (We note that the Revelle factor increases with increasing CO_2).
289
290   Any observation-informed model considers these text-book relationships (Dickson et al.,
291   2007;Stumm and Morgan, 1996) between pCO_2, dissolved CO_2, bicarbonate and carbonate
292   ions and the equilibration between atmospheric CO_2 and the dissolved species in the ocean.
293
294   The assumption of the single life-time model of Jacobson that the atmospheric concentration
295   will simply relax back to the preindustrial concentration with a multi-decadal time scale is not
296   justified and not in accordance with basic chemistry data and the limited volume of the
297   ocean.
298

Can transport processes within the ocean be approximated with a single time scale? There is the GLODAP data base on CDIAC which includes CFCs and radiocarbon data sampled over the past decades (Key et al., 2004). These data show that mixing time scales for the upper thermocline are decadal and mixing time scales for the deep ocean are multi-centennial. The lowest radiocarbon ratios of dissolved inorganic carbon are found in the deep Pacific with values that are about 240 permil or so lower than the atmosphere or about 200 permil lower compared to the surface ocean. This corresponds to a water age of roughly 1800 years.

The assumption of the Jacobson model that the CO_2 sink follows a single time scale is in conflict with oceanic tracer data (e.g., CFCs and radiocarbon).

The implicit assumption of the Jacobson model that the deep ocean is ventilated with a time scale of order 50 years is in conflict with radiocarbon observations.

c) Carbon uptake by the land occurs through photosynthesis and the related conversion of CO_2 to organic carbon. Carbon release occurs through oxidation (autotrophic,heterotrophic, fire) of organic material back to CO_2. Carbon sink processes in the land biosphere include forest regrowth and woody encroachment, fertilization of plant growth by increased availability of fixed nitrogen and higher CO_2 concentrations. Manipulative experiments indicate the potential magnitude of C and N fertilization (e.g.,(Norby et al., 2010)). Similarly as for the ocean, radiocarbon data indicate that there are multiple time scales for soil overturning that are in the range from years to centuries (e.g., (Gaudinski et al., 2000)).

In summary, the single life time model is in conflict with radiocarbon data (and other tracer data) and with first order principles of chemistry.

We recall that we applied 15 different carbon cycle-climate models spanning the full model hierarchy from reduced form models to state-of-the-art Earth System Models as well as a linear programming approach to estimate time integrated IRF and AGWP. Unlike stated by the reviewer, most of these models are fundamentally different from the early generation carbon models used in the first two IPCC assessments.

We recall that the GENIE and Bern3D-LPJ ensembles and the linear programming approach were explicitly constrained by observations.

In conclusion, the uncertainty range presented in this manuscript is appropriate and reflects our observational-based understanding of the carbon cycle. We do not include the erroneous single time scale model of Jacobson and the very simple model of Allen et al approximately falls within the uncertainty limits provided in the manuscript.

335

Specific comments

Page 19805, eq (2). In the definition of AGWP the authors choose to leave the quantity Ax, 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 Ax 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 Ax outside the integral right away.

$$\mathrm{AGWP_x(TH)} = \int_0^{TH} \mathrm{RF_x}(t)\,dt = \int_0^{TH} A_x\ \mathrm{IRF_x}(t)\,dt = A_x \int_0^{TH} \mathrm{IRF_x}(t)\,dt$$

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 Ax 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 theydenote "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 RFx (and for that matter Ax) is W m-2 kg-1 consistent with the unit ofAGWP being W yr m-2 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 CO2, $ACO2$, 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 19805, eq (2). : Partly **done:** We did not change equation 2, but we state at the end of the following paragraph:

"For sufficiently small emissions and approximately constant background conditions the radiative efficiency, A_x, can be approximated as time-invariant."

The approximation of a constant A_x does not hold for time varying backgrounds or large pulses as for example documented in figure 5 and 6 of the revised manuscript. Thus, we keep equation 2 unchanged.

371

The relation for CO_2 radiative forcing is introduced here as requested by the reviewer:

"The radiative forcing by a perturbation in the atmospheric burden of CO_2, ΔN_{CO2}, relative to a
reference burden, $N_{CO2,0}$, is parameterized following (Myhre et al., 1998):

$$RF_{CO2}(\Delta N_{CO2}) = 5.35 \text{ W m}^{-2} \ln\left(\frac{N_{CO2,0} + \Delta N_{CO2}}{N_{CO2,0}}\right)$$

(3)

This yields for small perturbations:

$$RF_{CO2}(\Delta CO_2(t)) = 5.35 \text{ W m}^{-2} \frac{\Delta N_{CO2}}{N_{CO2,0}} = A_{CO2} \cdot \Delta N_{CO2}$$

for $\Delta N_{CO2} \to 0$ (4)

Thus in the limit of a small perturbation, the radiative efficiency of CO_2 is 5.35 W m^{-2} divided by the
constant reference burden and thus itself a constant. "

Page 19808, line 16. It appears that the authors meant to say that CO2($t0$) [not CO2(t)] is the is the
atmospheric CO2 inventory at a time when the system was in (approximate) equilibrium.

P 19808, line 16: Done. thank you, corrected

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.

P 19808, line 17: Done. "equilibrium changed to "steady state" everywhere where appropriate

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-1]; 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-2 kg-1]; cf. Eq (2) of the manuscript, discussed above;
the integral is over time [yr]; so the quantity R(TH - t) must have dimension temperature per forcing
per time [K (W m-2)-1yr-1]. 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-2 yr.

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

403   **P 19809, line 20: Done.** Text clarified to read: **"**where $R(t)$ is the temporally displaced response in T

404   to a δ-function change in radiative forcing at time t=0.

405   **Page 19810, line 3**. The authors try to draw a distinction between the AGTP (absolute global

406   temperature potential), which they denote an "instantaneous (end-point) metric," and the

407   AGWP(absolute global warming potential), which they denote an "integrated metric." I do not see

408   any difference in the nature of the two quantities. The AGWP is the integral of the forcing from the

409   time of emission of 1 kg of material to the time horizon, or equivalently the amount of energy taken

410   up by the planet (per square meter) as a consequence of the emission of 1 kg of material. The AGTP

411   is the change in global mean surface temperature from the time of emission of 1 kg of material to the

412   time horizon as a consequence of the emission of 1 kg of material. I suggest either the authors delete

413   the point or offer a more persuasive explanation of the difference.

414   **P 19810, line 3: Done.** Text deleted as suggested

415   **Page 19819, line 7**. The expression for A_{CO2} given here depends on the Myhre (1998) expression for

416   forcing given later in the page (line 23), but actually in a new subsection that deals with response in

417   surface air temperature, ocean heat uptake and steric sea level, not forcing. Given the central

418   importance of both these expressions I would recommend that they be broken out instead of inline.

419   The inline equation is correct but cumbersome. Actually, as the Myhre expression for CO2 forcing

420   was known prior to the present study (indeed, well prior), it is strange that this expression is given

421   here, in the results section. I recommend it be given much earlier in the paper, back at Eq (2) where

422   the quantity A_x is introduced.

423   **P 19819, line 7: Done.** The radiative forcing equation for CO_2 developed by Myhre et al is introduced

424   now in section 2.1 and given by the new equation 3 and 4 in the limit of small perturbations (see

425   response above to "Page 19805, eq (2)").

426   The text here has been extended to include a reference to these two new equations: "Here, A_{CO2} is

427   computed for an atmospheric background of 389 ppm and in the limit of a small perturbation by

428   using the derivative of the simplified radiative forcing expression of (Myhre et al., 1998) (Equation (3)

429   and (4) and converting ppm into kg- CO_2): A_{CO2} =5.35 W m^{-2} (389 ppm)$^{-1}$ × (2.123×10^{12} kg-C/ppm)$^{-1}$ ×

430   (12 kg-C / 44 kg-CO_2) = 1.77 10^{-15} W m^{-2} kg-$CO_2$$^{-1}$."

431   **Page 19819, line 9-14**. Here and in Table 4 it would seem that the authors need to pay much

432   closerattention to specifying the meanings of the uncertainties. The IPCC AR4 ± 10% uncertainty in

433   radiative forcing of CO2 (Forster et al., 2007 p. 131), which denotes the 5-95% range (± 1.64 σ) of the

434   probability distribution function for the forcing, would seem to be the source of the 0.1 that is given

435   in the radical at line 14 along with the 49% (0.49) that is attributed to the uncertainty in integrated

436   impulse response function. Table 4 gives for the 100 yr time horizon the best estimate for time

437   integrated airborne fraction as 52.4 yr and gives the "average of ranges in % of the multi-model

438   mean" as 48.8%, which I take to be the source of the 0.49 in the radical. However the table states

439   that the 5-95% confidence range in the best estimate for time-integrated radiative forcing of CO2 is

440   (39.6-65.2) yr. Likewise the abstract states that the normalized forcing by CO2 integrated over a 100-

441   year time horizon is 92.7 × 10−15 yr W m−2 per kg CO2, with very likely (5-95%) confidence range (70

442   to 115) × 10−15 yr W m−2 per kg CO2.

443 These uncertainty ranges correspond to ± 24.4%, not ± 48.8%, so there would seem to be some
444 inconsistency here that needs to be resolved. This uncertainty is discussed also at **page 19818, lines**
445 **20-26**, where it is stated that the average uncertainty range (for IRF at TH = 100 yr) is 26 yr, or
446 49%relative to the best estimate 52.4 yr. The manuscript states:

447 [T]hese approaches yield an average uncertainty range of 26 yr or of 49% for the 100-yr integrated
448 response (Table 4). We assume that this average range represents approximately a 5–95%
449 confidence range and that it is symmetrically distributed around the 25 multi-model mean to arrive
450 at our best estimates for the mean and 5–95% confidence range for the time-integrated IRFCO2 .

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

454 However perhaps more important is the ± 10% uncertainty associated with CO_2 forcing which the
455 authors take at face value from AR4 (Forster et al., 2007). Although this uncertainty is hoary with age
456 and burnished by repetition, it hardly seems an accurate estimate of the uncertainty that actually
457 attaches to present knowledge of this forcing. Recently Andrews et al. (2012) compared CO_2 forcings
458 and climate response of 15 atmosphere-ocean general circulation models (GCMs) that participated in
459 the CMIP-5 model intercomparison. Forcing and temperature response coefficient were inferred
460 from the output of the model runs respectively as intercept and slope of a graph of net top-of-
461 atmosphere energy flux versus global mean temperature anomaly subsequent to a step-function
462 quadrupling of atmospheric CO_2. (Because the model experiments examined response to a
463 quadrupling of CO_2, rather than a doubling, the intercept had to be divided by 2 to obtain the forcing
464 pertinent to doubled CO_2). The forcing is interpreted as an "adjusted forcing" that includes rapid
465 adjustments, mainly of atmospheric structure, that modify the TOA radiative flux on time scales
466 shorter than a year or so. A key finding of the Andrews et al. study is the spread of values of forcing
467 exhibited by the different GCMs, 16%, 1-σ. The spread in forcing is a consequence of differing
468 treatments of the radiation transfer in the several models as well as different treatments of clouds
469 that interact with radiation. As the forcing inferred from the analysis of Andrews et al. is an adjusted
470 forcing, it appropriately reflects differences among the models in rapid (1 yr) response of
471 atmospheric structure to the imposed forcing. This spread in forcings inferred from the climate
472 model runs is substantially greater than the uncertainty specified in the IPCC AR4, which gives a 5-
473 95% confidence range (± 1.64 σ) of ± 10%; i.e., 1-σ uncertainty 6.1%.

474 That there is such a range of forcing as inferred from GCM runs should not come as much of a
475 surprise. For example, although the Radiative Transfer Model Intercomparison Project (Collins et al.,
476 2006) reported a 1-σ spread in longwave forcing at 200 hPa among the GCMs compared of only 8.5%,
477 that study was restricted to cloud-free atmospheres, with the reason given that "the introduction of
478 clouds would greatly complicate the intercomparison exercise," from which one infers that the
479 spread of forcing in a model with clouds would greatly exceed that in an idealized cloud-free model.
480 Hence the finding of a 1-σ spread of some 16% in the forcings (i.e., 5-95% range ± 26%, much greater
481 than the ± 10% reported by AR4) is likely as accurate an assessment of the maximum level of
482 confidence that can be placed in this quantity at present. It would thus seem that it is this
483 uncertainty that should be combined (in quadrature) with the uncertainty in impulse response
484 function to get an accurate measure of the uncertainty in integrated forcing.

485 **P 19819, line 9-14: part a) Done.** Thank you for pointing out this inconsistency. The text has been
486 changed to read: "The uncertainty in the radiative efficiency of CO_2 is given as ±10% in the IPCC TAR
487 and AR4 (90% confidence interval; see page 140 of (Forster et al., 2007)) and guided by the spread in
488 published estimates. An uncertainty of ±10% translates to an uncertainty range of 20%. The overall
489 uncertainty in $AGWP_{CO2}$ is only slightly larger than that for IRF_{CO2} as the uncertainty in A_{CO2} is much
490 smaller than that of the time integrated $IRF_{CO2.}$ Assuming quadratic error propagation, the
491 uncertainty range in $AGWP_{CO2}(TH=100$ yr) is 53% (sqrt($0.49^2+0.2^2$)=0.53) compared to 49% of the
492 integrated IRF_{CO2}.

493 **Part b) magnitude of uncertainty range:**

494 We disagree with the reviewer that our uncertainty estimate for the radiative forcing
495 parameterization is too small.
496
497 The uncertainty in the radiative forcing parameterization applied here (RF=5.35 W/m2 ln $CO_2/CO_{2,0}$)
498 is distinct from the spread in radiative forcing exhibited by the different GCMs. The radiative forcing
499 parameterization is derived from detailed line-by-line models, whereas GCMs apply continuum
500 formulations. Line-by-line codes are investigated in many studies and their forcings generally agree
501 within a few percent (e.g., Collins et al., 2006; (Iacono et al., 2008)) and compare well to observed
502 radiative fluxes under controlled situations (Oreopoulos et al., 2012) including clouds.
503
504 The conclusion of the reviewer that the spread in CO_2 radiative forcing in models with and without
505 cloud is much different is not justified. Clouds can greatly reduce the magnitude of radiative forcing
506 due to greenhouse gases, but this does not necessarily increase the spread in radiative forcing
507 calculations and recent studies included clouds (Iacano et al., 2008, Oreopoulos et al., 2012).
508
509 Uncertainties in radiative forcing in GCMs are often larger than the uncertainty assumed here and in
510 IPCC AR4 for the radiative forcing parameterization of Myhre and colleagues. However, the point is
511 that we are computing the radiative forcing from the Myhre et al parameterization (equation 2 and
512 3). What matters is thus the uncertainty in this parameterization and not the uncertainty of the
513 radiative forcing as implemented in GCMs. Foster et al., 2007, page 140 states: "Collins et al. (2006)
514 performed a comparison of five detailed line-by-line models and 20 GCM radiation schemes. The
515 spread of line-by-line model results were consistent with the ±10% uncertainty estimate for the
516 LLGHG RFs adopted in Ramaswamy et al. (2001) and a similar ±10% for the 90% confidence interval is
517 adopted here.
518 However, it is also important to note that these relatively small uncertainties are not always
519 achievable when incorporating the LLGHG forcings into GCMs. For example, both Collins et al. (2006)
520 and Forster and Taylor (2006) found that GCM radiation schemes could have inaccuracies of around
521 20% in their total LLGHG RF (see also Sections 2.3.2 and 10.2)."
522

523 **Page 19819, line 23**. As noted above, the forcing equation given here should be given earlier and
524 given its central importance might be broken out instead of in-line. It might also be motivated better.
525 The 47.1 in the equation comes from 100 G ton corresponding to 47.1 ppm, but that is not stated.
526 The 389 in the equation is particular to the starting point of the calculation. This equation is central
527 to the derivation of the ACO2 given earlier in the page (in a prior subsection). As noted earlier, I think
528 all of this material should be moved up toward Eq (2).

529

530

P 19819, line 23: Done. Equations are given in line in section 2.1 and the text here is modified to read: "The response in radiative forcing to the 100 GtC pulse (equivalent to 47.1 ppm) corresponds to a step increase by 0.61 W m^{-2} at year 0, followed by a decrease to 0.26 W m^{-2} at year 100 and to 0.15 W m^{-2} at year 1000. These values are computed from the multi-model mean IRF_{CO2} with the help of equation (3) and for a reference mixing ratio of 389 ppm ($RF(t)$=5.35 W m^{-2} ln((389 ppm + $IRF_{CO2}(t)$ × 47.1 ppm)/389 ppm))."

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 Fuglestvedt (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).

P 19819, line 23: Done. a) The evolution of temperature is simulated by the models and eq. 10 is not used to compute AGWP. This is already clearly stated below equation 10: "It is also possible to estimate $AGTP_{CO2}$ and $IRF_{T,CO2}$ directly from a climate-carbon cycle model in response to a pulse emission. This is done in this study with the suite of carbon-cycle climate models."

b) equilibrium SAT response: The text has been clarified to read: "What magnitude in the SAT response is to be expected from this forcing? The equilibrium response in global mean surface air temperature (SAT) to these forcing values are 0.49^{o}C (year 0), 0.21^{o}C (year 100) and 0.13^{o}C (year 1000) when assuming for illustrative purposes a typical mid-range climate sensitivity of 3°C for a nominal doubling of CO_2."

c) SAT variability: The following sentence was added to the MS for clarification:" We note that the three Earth System Models were run over the first 100 years only."

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 CO2 and nitrogen input on net primary productivity and carbon stocks." This is an important point.

571 **P 19822, line 26: Done**. no action requested.

572 **Page 19826, lines 8-9.** "However, the most important factor that determines the time-integrated
573 IRFCO2 and AGWP is the choice of time horizon." This is of course no surprise. It is a consequence of
574 the definition. But it is surprising that it is the concluding sentence of the results section. I refer the
575 authors to my discussion of average IRF vs integrated IRF, below.

576 **P 19822, line 8-9:** We agree that this is no surprise, but we feel it remains important to spell this out
577 for a wide audience.

578 **Page 19827, line 18-21**. "The subjective choice of the time horizon dominates uncertainties in the
579 absolute global warming potential of CO2 and related uncertainties in global warming potential 20 of
580 most other agents. The uncertainty in AGWP (in units of yr Wm−2 per kg CO2) can be mapped to an
581 uncertainty in the time horizon (in units of year)." Again, the time horizon that is used is up to the
582 user; the "subjective choice" of time horizon is not a source of uncertainty in the usual sense that the
583 term is employed in science.

584 **P 19822, line 18-21: Done.** Text clarified to read: "The subjective choice of the time horizon has a
585 much larger influence on the range in absolute global warming potential of CO_2 and in the global
586 warming potential of most other agents than uncertainties associated with the computation of these
587 values for a given time horizon. The uncertainty in AGWP (in units of yr W m^{-2} per kg-CO_2) can be
588 mapped to a range in the time horizon (in units of year)."

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

590 **P 19827, line 26: Done.** Reference added as requested: "in the IPCC report (Table 2.14, page 212 in
591 (Forster et al., 2007)) GWP are tabulated"

592 **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
593 included, I would certainly recommend that the *Ax* values of the several gases be presented as these,
594 together with the IRFs are the fundamentally relevant quantities. Likewise the AGWPs (or better the
595 integrated and/or average IRFs) are more fundamental than the GWPs. Elsewhere (Schwartz, 2012) I
596 have argued that the GWP concept be abandoned in favor of AGWPs, as changing the denominator
597 because of improved understanding of forcing or IRF of CO2 has the effect of changing the GWPs of
598 gases other than CO2 even though there has been no change in the understanding of the properties
599 of those gases. The present revision of GWPs is just another instance of the consequences of
600 expressing scientific quantities in non-standard units (i.e., multiples of the AGWP of CO2). This point
601 is underscored by Figure 7.

602 In Figures 1-3 gray shading is used to show the uncertainty range of quantities of interest. It would be
603 of great value to include this gray shading in Figure 7, as I have done in the figure below, from which
604 it is seen that the range of impulse response profiles of the models examined by Joos et al.
605 encompasses and indeed well exceeds the range of all the impulse response profiles from the prior
606 IPCC reports. So even with the limited subset of carbon cycle models examined in the current study,
607 the uncertainty in the carbon cycle dominates the uncertainty in the GWPs of all non-CO2 gases. This
608 finding underscores the

609 shifting sands that constitute the foundation on which the GWP edifice is built. The discussion (**lines
610 10-20**) of the differences between the several IPCC IRFs and that of the multi-model mean of the

P 19827, line 27, table 7: a) Table 7 shows the ratio of GWP(TH) to the GWP(TH=100). The radiative efficiency A_x does not enter this calculation. Thus, we do not add Ax to this table as it is irrelevant.

The point of this table is to underscore that the choice of the time horizon is important when comparing GHG emissions in a basket approach.

b) Done. We do report AGWP in table 4 and we added the following text in section 2.1 to underscore the disadvantage of reporting GWP:

"Forster et al., 2007 (page Table 2.14, page 212) report the GWP of many gases and for different time horizons. A problem related to reporting GWP only is that each update in $AGWP_{CO_2}$ affects the reported GWP values of all other gases. This could be easily avoided by reporting Absolute Global Warming Potentials in addition to GWP.."

Figure 7: The reason for the differences in the different IRFs are described in the submitted version (and kept in the revised version): "The responses published in the SAR and the AR4 are lower than the multi-model model mean response of this study. This is predominantly due to the smaller pulse size and lower CO_2 background in the SAR and AR4 setup. The time-integrated IRF_{CO_2} for the AR4 (Bern2.5D-LPJ) and SAR (Bern-SAR) models under the setup of this study (Table 4) are with 49 and 51 yr only slightly lower than the multi-model mean of 52 yr at year 100. We do not find indications that there are systematic differences in IRF_{CO_2} between models of different complexities such as EMICs and comprehensive Earth System Models."

This discussion is not moot as it demonstrates that the reason for the update in the IRF form SAR to AR4 is mainly related to differences in the setup of the IRF experiment and a consequence of the rapidly rising atmospheric CO_2 and not due to the choice of models. As the purpose of figure 7 is to illustrate the revision in the IRF compared to previous assessment we do not see the point to add the grey uncertainty range. In our opinion, this range would distract from the intended message of the figure.

Relation to prior work

Relation to prior work: Done. Text in section 3 reads**:** "5000 GtC is of the same order as available conventional (coal, oil, gas) fossil carbon resources and has been used in past pulse experiments (e.g. Archer et al., 2009;Eby et al., 2009). This experiment is thus indicative of the long-term consequences for burning all conventional fossil resources."

Text in discussion sections reads: "An emission of 5000 GtC is an extreme case in the context of Global Warming Potential (GWP), though within reach when burning all fossil resources. Such large pulses are also used in other studies to assess the evolution in the CO_2 perturbation over several

16

650   centuries and up to 10 000 years (Archer et al., 2009;Eby et al., 2009). These studies also find a long-
651   lasting perturbation in atmospheric CO_2. "

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.

Figures, tables, appendix: no changes applied.

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 W yr m-2 kg-1, the integral of forcing (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 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.

Definitions and terminology: paragraph 1 and 2: It is beyond the scope of this paper to redefine the terminology used by different scientific communities.

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 $t0 + t'$ due to an impulse at time $t0$ 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 $t0$ that remains in the atmosphere at time $t0 + t'$, a function of t' and perhaps weakly dependent on $t0$ 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 $t0$, again normalized to the amount of emitted material, not just to the

690   fraction of emitted material itself. Such use of the term is also made in the manuscript, I think
691   appropriately, for example the impulse response of global mean temperature normalized to a unit
692   emission of a given substance, page 19809, line 13.

693   The IRF of the amount of material in the atmospheric reservoir plays a special role. This IRF is
694   dimensionless, the fraction of material emitted at time $t0$ that is present in the atmosphere at time t'
695   subsequent to emission.

696   **Definitions and terminology: paragraph 3 and 4:** reviewer agrees with current manuscript – no
697   changes applied as definitions, explanations, and units are already included in the MS.

698   The time integral of an impulse response function over a period of time, which has dimension time
699   and unit year, is referred to in the present manuscript somewhat cumbersomely as "time-integrated
700   airborne fraction" or "time-integrated IRF." The end point of the integration is denoted in this
701   community as"time horizon." The time integrated IRF is strongly dependent on the time horizon.
702   Initially this integral scales linearly with the time horizon. As the material is depleted from the
703   atmosphere the integrated IRF is no longer proportional to the time horizon, but falls off from that
704   proportionality. Ultimately, if and when the material is completely depleted from the atmosphere,
705   the integrated IRF becomes a constant. For this reason, at least over the period for which the IRF is
706   non-zero and the integral is increasing, it might make sense to divide the integrated IRF by the time
707   horizon to obtain the *average* IRF over the time horizon. This would make sense especially in the case
708   of CO2, for which in the models examinedhere, the IRF does not reach zero for time horizons of
709   interest, extending out to millennia.

710   The integrated IRF scales roughly with time and thus exhibits a strong dependence on time horizon,
711   as shown in the excerpt from Table 4 of the manuscript, below, and as commented on at page 19826,
712   lines 8-9, "However, the most important factor that determines the time-integrated IRFCO2 and
713   AGWP is the choice of time horizon." This dependence is wholly a consequence of definition. In
714   contrast to the integrated IRF, the average IRF, the average fraction of emitted material that remains
715   in the atmosphere over the time horizon, is a much more constrained quantity, bounded between 0
716   and 1 and decreasing with time horizon, and much less strongly a function of time horizon than the
717   integrated IRF. This property of the IRF readily allows comparisons within the table both down and
718   across, obviating the need for mentally normalizing to the time horizon. For this reason I would
719   advocate the quantity being reported and tabulated be the average IRF, not the integrated IRF.

720   **Definitions and terminology: paragraph 5 and 6: average IRF:** We appreciate that the average IRF is
721   more constrained and easier to compare in the case of CO_2. However, we like to keep the time-
722   integrated IRF as (i) this quantity forms the basis for the computation of AGWP, (2) the averaged IRF
723   values might be more easily confused with the instantaneous values of the IRF.

724

725   **Nomenclature**

726   As a reviewer I must express distress over the use of multi-letter symbols (acronyms) such as GWP,
727   RF, TH to denote quantities in algebraic equations in the manuscript under review. This usage flies
728   against long accepted practice in the scientific community. The authoritative document published by
729   the International Union of Pure and Applied Physics, "Symbols, Units, Nomenclature and

750   **Nomenclatura:**

751   As this paper is not only intended for physicists, we keep the conventional multi-letter notation. This

752   notation is also used in other Copernicus publications (see e.g. Boucher, O.: Comparison of

753   physically- and economically-based CO_2-equivalences for methane, Earth Syst. Dynam., 3, 49–61,

754   doi:10.5194/esd-3-49-2012, 2012.) .

755   Thank you. We use now an upright font for the symbol x.

756   Thank you. We removed this typo and use now ppm throughout.

757

758   **Selected references**

759   Archer, D., Eby, M., Brovkin, V., Ridgwell, A., Cao, L., Mikolajewicz, U., Caldeira, K., Matsumoto, K.,
760   Munhoven, G., Montenegro, A., and Tokos, K.: Atmospheric Lifetime of Fossil Fuel Carbon Dioxide,
761   Annu. Rev. Earth Planet. Sci., 37, 117-134, 10.1146/annurev.earth.031208.100206, 2009.
762   Dickson, A. G., Sabine, C. L., and Christian, J. R.: Handbook of methods for the analysis of the various
763   parameters of the carbon dioxide system in sea water, 2007.
764   Eby, M., Zickfeld, K., Montenegro, A., Archer, D., Meissner, K. J., and Weaver, A. J.: Lifetime of
765   Anthropogenic Climate Change: Millennial Time Scales of Potential CO2 and Surface Temperature
766   Perturbations, Journal of Climate, 22, 2501-2511, 10.1175/2008jcli2554.1, 2009.
767   Forster, P., Ramaswamy, V., Artaxo, P., Berntsen, T., Betts, R., Fahey, D. W., Haywood, J., Lean, J.,
768   Lowe, D. C., Myhre, G., Nganga, J., Prinn, R., Raga, G., Schulz, M., and Van Doorland, R.: Changes in
769   Atmospheric Constitutents and in Radiative Forcing, in: Climate Change 2007: The Physical Science
770   Basis. Contribution of Working Group I to Fourth Assessment Report of the Intergovernmental Panel

771   on Climate Change, edited by: Solomon, S., Qin, D., Manning, M., Chen, Z., Marquis, M., Averyt, K. B.,
772   Tignor, M., and Miller, H. L., Cambridge United Kingdom and New York, NY, USA, New York, NY, USA,
773   129-234, 2007.
774   Gaudinski, J. B., Trumobore, S. E., Davidson, E. A., and Shuhui, Z.: Soil carbon cycling in a temperate
775   forest: radiocarbon-based estimates of residence times, sequestration rates and partitioning of
776   fluxes, Biogeochemistry, 51, 33-69, 2000.
777   Iacono, M. J., Delamere, J. S., Mlawer, E. J., Shephard, M. W., Clough, S. A., and Collins, W. D.:
778   Radiative forcing by long-lived greenhouse gases: Calculations with the AER radiative transfer
779   models, J. Geophys. Res., 113, D13103, 10.1029/2008jd009944, 2008.
780   Key, R. M., Kozyr, A., Sabine, C. L., Lee, K., Wanninkhof, R., Bullister, J. L., Feely, R. A., Millero, F. J.,
781   Mordy, C., and Peng, T. H.: A global ocean carbon climatology: Results from Global Data Analysis
782   Project (GLODAP), Global Biogeochem. Cycles, 18, GB4031, 10.1029/2004GB002247, 2004.
783   Myhre, G., Highwood, E. J., Shine, K. P., and Stordal, F.: New estimates of radiative forcing due to well
784   mixed greenhouse gases, Geophysical Research Letters, 25, 2715-1718, 1998.
785   Norby, R. J., Warren, J. M., Iversen, C. M., Medlyn, B. E., and McMurtrie, R. E.: CO2 enhancement of
786   forest productivity constrained by limited nitrogen availability, Proceedings of the National Academy
787   of Sciences, 107, 19368-19373, 2010.
788   Oreopoulos, L., Mlawer, E., Delamere, J., Shippert, T., Cole, J., Fomin, B., Iacono, M., Jin, Z., Li, J.,
789   Manners, J., Räisänen, P., Rose, F., Zhang, Y., Wilson, M. J., and Rossow, W. B.: The Continual
790   Intercomparison of Radiation Codes: Results from Phase I, J. Geophys. Res., 117, D06118,
791   10.1029/2011jd016821, 2012.
792   Revelle, R., and Suess, H. E.: Carbon dioxide exchange between atmosphere and ocean and the
793   question of an increase of atmospheric CO_2 during the past decades, Tellus, 9, 18-27, 1957.
794   Stumm, W., and Morgan, J. J.: Aquatic Chemistry, Chemical Equilibria and Rates in Natural Waters,
795   3rd ed. John Wiley & Sons, Inc., New York, 1022 pp., 1996.
796   Zeebe, R. E., and Wolf-Gladrow, D.: CO_2 in seawater: equilibrium, kinetics, isotopes, Elsevier
797   Oceanography Series, 2001.

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