Response to editor Anja Rammig on “Time-scale dependence of airborne fraction and underlying climate-carbon cycle feedbacks for weak perturbations in CMIP5 models”

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Dear Prof. Dr. Anja Rammig,

In the attached documents you find our revised paper along with a track-changes file highlighting all modifications made to the original preprint. Below we reproduce also the answers to the reviewers already published during the discussion phase, but with some reformulations and additional comments to reflect our actual changes in the paper (shown in *blue*; differences to the published answers were automatically marked by latexdiff). In particular we added for better orientation the line numbers where we have made the respective changes: line numbers in round brackets *(. . .)* refer to the final revised paper, while line numbers in square brackets *[. . .]* refer to the track-changes file.
Please note that the track-changes file shows changes in almost every equation – this doesn’t mean that the equations have changed mathematically, but, as a consequence of the request by reviewer Ian Enting to change in all equations the indices A, O, and L to roman font, they are now differently type-set.

With best regards,

Guilherme L. Torres Mendonça, Christian H. Reick and Julia Pongratz

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We thank the reviewer for his careful comments. We reproduce the reviewer’s remarks in bold, while setting our replies in italic.

This paper (hereafter denoted TM23) is suitable for publication. The authors may wish to consider the following comments. References cited below refer to those listed in the bibliography of TM23 or additional references listed below. Some of my points are, for convenience, illustrated by citing my own publications. This not in itself a suggestion that these papers should be cited, nor that they are the most appropriate references.

OVERVIEW

This paper considers feedbacks in the carbon cycle, expressing the gain 1/(1-f) from feedbacks in terms of the total feedback, f. A generic result for linear feedbacks is that the combined loop feedback from multiple feedbacks is the sum of the feedbacks so that linear feedback contributions can be partitioned or aggregated without restriction. The analysis is in terms of the alpha, beta, gamma description (Friedlingstein et al, 2003), where carbon cycle feedback, f, is partitioned into a concentration feedback, beta, and a radiative feedback alpha*gamma. Each of these is often further partitioned into Land and Ocean contributions (and may be further partitioned, cf Enting and Clisby 2019 appendix). The description by (Friedlingstein et al, 2003) is history-dependent. A scenario-independent form requires generalising each of alpha, beta, gamma to functions which can be combined by convolutions over time. These combinations take a simpler form as Laplace transforms. (In this regard, as in the study by Enting and Clisby (2019), Laplace transform expressions can be regarded as a compact expression for calculations that are actually performed in the time domain, analogous to the way that vector expressions for electromagnetic fields define expressions that are ultimately calculated using specific components). The paper uses results from the CMIP intercomparison to estimate the generalised alpha, beta, gamma (in each case partitioned into Land and Ocean) and combines them into a generalised airborne fraction.
We fully agree.

COMMENTS

Line 71. This notes characterising climate change in terms of a single global temperature change. This is the basis for "pattern scaling" which is often used in impact assessments (Mitchell 2003, which should be cited). However this is not what is done later (see comment on eqn 4,5).

We thank the reviewer for this literature hint. Indeed, the study by Timothy Mitchell is a careful analysis of the extent to which global temperature alone is characterizing climate. Nevertheless, the study refers only to climate physics (temperature, precipitation), while Friedlingstein et al. base their alpha-beta-gamma formalism on the even wider hypothesis that also in relation to the global carbon cycle it suffices to characterize climate by global temperature alone. This is indeed nontrivial because one might think that for the estimation of feedbacks involving the global carbon cycle a separate characterization of the water and nutrient resources of plants would be needed – but apparently at a global scale this seems not necessary. Interestingly, Mitchell also corroborates the linearity of the response and finds in particular a dependence of the linear relationship on the rate of temperature rise. This can be interpreted as dependence on the internal memory of the system, which once more indicates the need for a more general formulation of the response in terms of linear response functions. In the revised paper we ~~will~~ now refer to the Mitchell paper (L70–71),[L70–71].

At some point between lines 155 and 175, it could be worth combining all the terms into one equation and write down the original alpha, beta, gamma formalism for comparison Delta C [1+ beta +alpha * gamma] = integrated emissions.

Thank you for this suggestion. We also think that we could make the relation to the original alpha-beta-gamma formalism more obvious. We ~~will do~~ have done so in relation to Eqs. (7)~~-~~–(10), which have close analogues in the original formalism (L224–233),[L229–238].

Equations 2 and 3.

These have several issues created by working with separate land and ocean temperatures:

*** it contradicts the initial statement about using a single global temperature**

*** the numbers are going to differ from other work that has a single global alpha and absorbs the land-ocean differences into gamma**

Indeed we use separate values for ocean and land instead of a single global temperature. We feel that in this way processes are more adequately represented, because e.g. ocean carbon uptake depends on ocean temperatures alone, and not on global temperature that involves an admixture of land temperatures. Still, comparison of results with previous studies is possible by means of straightforward transformations where in addition to the sensitivities only the fractions of global area occupied by land and ocean are needed (see Appendix A below). We ~~will add~~ have added this to the revised text (Appendix G).

*** working with a single alpha simplifies the inclusion of other radiative forcing (CH4, volcanoes etc) which would be needed for possible future work. Even within the context of the present work, including these terms helps make the important point that the carbon-climate coupling amplifies these forcings by the same factor that applies to amplification of the CO2 response determined by the beta term (Gregory 2009, eqns 8a,b of Freidlingstein 2003).**

Maybe using a single global temperature would indeed simplify a future inclusion of other radiative forcings, but even in that case we would find it more adequate to introduce the practically more complicated but physically more meaningful formulation in terms of separate land and ocean temperatures. But to include other radiative forcings one would probably have to account *for further feedbacks that complicate the situation anyway: of particular importance are the enhancement of CH_4 emissions from permafrost regions and wetlands under enhanced temperatures, and the reduction in the lifetime of N_2O in a warmer climate induced by an accelerated Brewer-Dobson circulation (Kracher et al., 2016). This seems to be a whole project in itself that is beyond the scope of the present work.*

The results shown in panels c and f of figure 2, suggest that for most, but not all, models the results are consistent with the assumptions of pattern scaling, with the land response about twice the ocean response on all timescales. It would be interesting to analyse this in more detail than what can be gleaned from low resolution plots– eg a plot of the ratios of the two responses as a function of timescale.

Yes, ocean and land alpha sensitivities are closely linked: it is well known that by various mechanisms land temperatures rise faster than ocean temperatures by a factor of about 1.4-1.7 (Lee et al., 2021, section 4.5.1.1.1, and Fig. 1 below for CMIP5 and CMIP6 simulation data, and Eyring et al., 2021, Fig. 3.2b for observation data). Looking at the definition of the land and ocean alpha generalized sensitivities one sees that they must differ exactly by this factor. We ~~will add~~ have added a remark on this ~~in the revised paper~~(L505–510),[L528–533] and a plot of the ratio of the respective generalized sensitivities (see inset of Fig. 2f).

Eqn 6. Introducing CO2 concentrations (rather than CO2 content as originally done by Friedlingstein et al 2003) (and thus introducing the factor k) seems an unnecessary complication which hinders comparisons with other work. (Of course k is given by the mass of the atmosphere scaled by ratio of molecular weights).

Indeed the usage of k could be omitted by absorbing it in the definition of the sensitivities. But physical and biogeochemical carbon processes develop via CO2 concentration and not by atmospheric carbon mass. Therefore we prefer to stick to the physically more adequate distinction between CO2 concentration and atmospheric carbon mass in the definition of the involved quantities, even though the equations look mathematically less elegant. Moreover, we do not see why it should hinder comparison with previous studies: although Friedlingstein et al. (2003) initially worked with atmospheric carbon content when introducing their methodology, the units of their calculated sensitivities in this and subsequent publications (e.g. Friedlingstein et al., 2006; Arora et al., 2013; Canadell et al., 2021) are actually in agreement with our definition: beta is given in GtC/ppmv, gamma in GtC/K, and alpha in K/ppmv (Friedlingstein et al., 2003, Table 1).

Line 188 seems poor wording of the situation. You can work in the time domain (and in many cases do so), but you have to incorporate history and not just single times (which is what Oeschger and Heimann pointed out in 1983).

Thanks for the comment, indeed we could have been clearer – we meant that Laplace transforms make the whole formulation simpler, but the interpretation of resulting quantities harder. We ~~will reformulate~~ have reformulated the sentence accordingly (L191–192),[L195–196].

Circa line 295. At around this point it could be helpful to have one or two sentences summarising the key aspects of the RFI technique (and maybe a longer summary in the relevant appendix).

We agree and have added more details on the RFI method (L308–337),[L314–357].

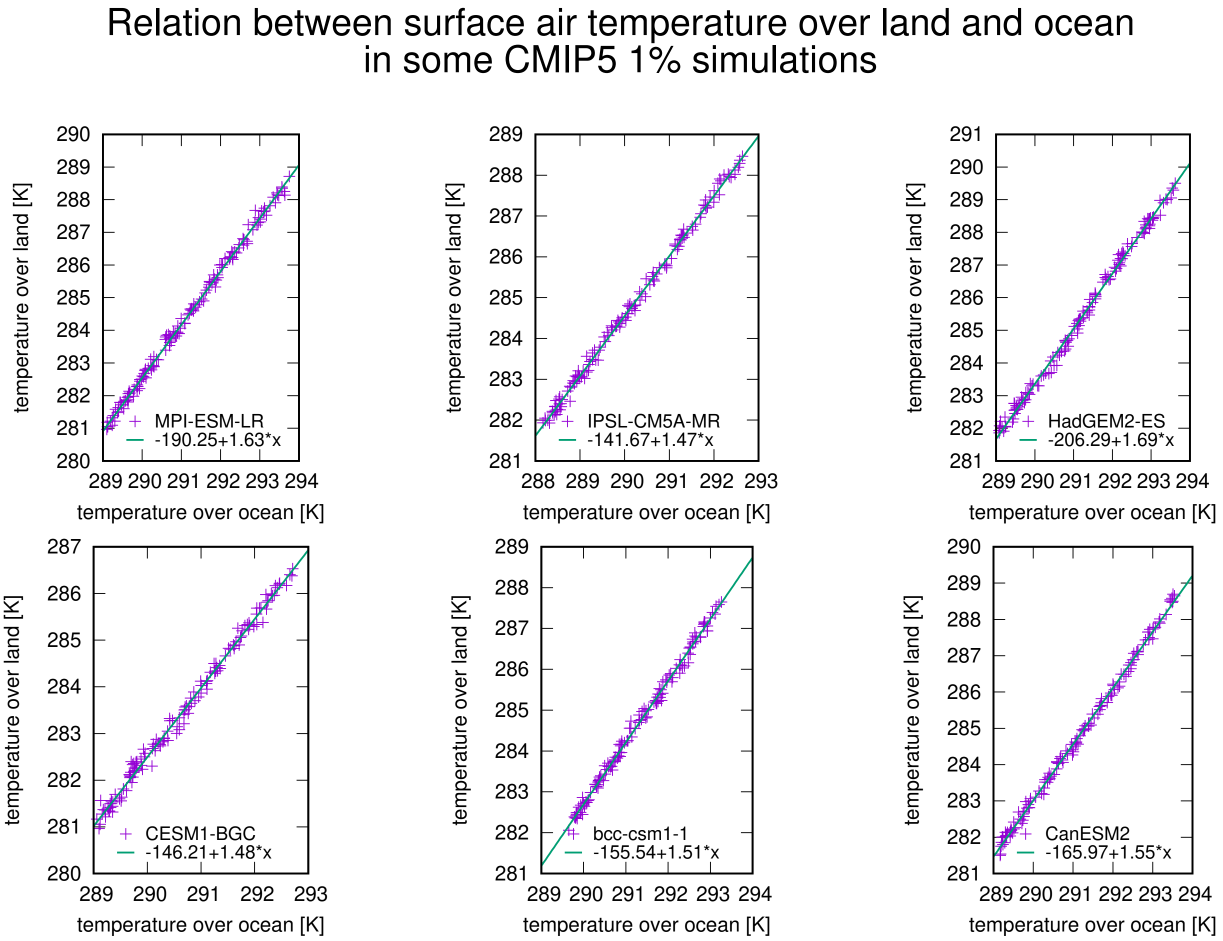

Figure 1. Annual mean land temperature as function of annual mean ocean temperature for various CMIP5 1% simulations. From the linear fits it is seen that the relation is rather linear and that land temperature rise is about a factor 1.5-1.7 times larger than ocean temperature rise. Noting that $\widetilde{\mathcal{X}}^{(\mathrm{O})}_{\alpha}(p) = \Delta\widetilde{T}_{\mathrm{O}}(p)/\Delta\widetilde{c}(p)$ and $\widetilde{\mathcal{X}}^{(\mathrm{L})}_{\alpha}(p) = \Delta\widetilde{T}_{\mathrm{L}}(p)/\Delta\widetilde{c}(p)$ these two generalized sensitivities differ by that factor.

Eqn 15, (and associated definitions). What this means is that chi is the CO2 impulse response function that is widely used in the definition of GWP and was the subject of an extensive intercomparisons by Joos et al (2013). This should be noted and Joos et al cited at this point.

Thank you for reminding us, we ~~will follow your advice~~ have followed your advice (footnote 3).

Eqn 16, goes back at least to Enting (1990), as a Laplace transform. As a relation for growth at single time-scale it is implicit in the results of Oeschger et al 1980.

We ~~will note~~ have noted this in the revised paper (L354),[L374].

The overall response for CO2 is given by 1/p/(1+beta(p) +alpha(p) * gamma(p)) Enting (2009) suggested that models could give similar fits to 20th century changes (p approx 0.02) whether or not the gamma feedback was included, simply

by changing beta (specific examples were given). While the CMIP data presented here do not give details of how the various models were calibrated, the results suggest that a similar inter-model flexibility appears here. Panels d and e of figure 2 suggest that on timescales of about 50 years, the models with smaller positive beta have smaller negative gamma. This interpretation is supported by figure 6a, where the difference between the true AF spread and that from eqn (22) (which assumes that the spreads are independent), suggests that the spreads of beta and gamma are not independent.

Following Arora et al. (2013), we explain in the paper the joint occurrence of a smaller positive beta together with a smaller negative gamma found for NorESM1-ME and CESM1-BGC by the presence of nitrogen limitation in these models: ".. the coupling between nitrogen and carbon cycle weakens not only the biogeochemical but also the radiative response, because temperature-driven nitrogen remineralization enhances plant productivity, which counteracts the parallel carbon loss from the enhanced soil respiration in the warmer climate (see also Melillo et al., 2002; Thornton et al., 2009)" (lines 463-467). But if we understand it correctly, the reviewer suggests that there may be in general some sort of compensation effect between beta and gamma in each model so that models with large beta have also large gamma and vice-versa, which implies that a large model spread in beta would then correspond to a large spread in gamma. While this may indeed affect the accuracy of approximation (22) of the overall spread in airborne fraction by a sum of individual spreads from the contributing components – which depends also on smallness of these individual spreads –, we feel that this rough approximation is sufficient to convey our main message from this analysis, namely that the land biogeochemical feedback is dominating the spread in the airborne fraction, which is further supported by Fig 6b.

*** POTENTIAL FUTURE EXTENSIONS**

An indication of the quality of the work is the extent that it suggests potential future studies. Some possibilities (realising that such work may already be in progress) are: 1: Calculate the airborne fraction, dC/dt/E, as a function of time for various cases of RCP-SSP (Representative Concentration Profiles - Shared Socio-economic Pathways). Non-linearity will limit the accuracy, but the least affected will be the ones that are of most interest as likely to have the greatest change from near-constant airborne fraction. 2: Using the formalism to analyse the CMIP historical runs. These two extensions would require extending eqn (4,5) to include non-CO2 radiative forcing.

Thank you for these suggestions and reminding us that our current work is limited to idealized simulations where the only changing anthropogenic greenhouse gas is CO2 –~~we will add~~ - we have added a note on this limitation in the discussion section of the paper (L724–727),[L748-751]. Indeed, application to historical simulations and RCP-SSPs would require an extension of the generalized alpha-beta-gamma formalism to include non-CO2 greenhouse gases. As pointed out above by the reviewer, one could follow here the works by Gregory et al. (2009) and Friedlingstein et al. (2003) to add those greenhouse gases as additional external forcing. But the real challenge would in our opinion be the inclusion of landuse change because in contrast to well-mixed greenhouse gases such as CO2 and methane, whose representation needs only global values, one had in the case of landuse change to account not only for its emissions, but also for its regional patterns that are different in different scenarios; these changes involve changes not only in regional surface properties (albedo, roughness), but also in the response characteristics of the regional biosphere whereby internal time scales are affected.

3: A more speculative possibility is the extension to response functions describing radiocarbon (C14). As noted by Enting and Clisby (2021) and Enting (2022), expressions for the responses to exponential forcing were described by Oeschger et al (1980). Oeschger et al also described corresponding responses for C14 perturbations and how these relate to total carbon (see also Enting 1990). Since such responses to exponential forcing are the Laplace transforms of the impulse response functions (eg see eqn 2.11 of Enting 2022), the Oeschger et al results suggest the possibility of defining generalised sensitivities for C14. This would be of most interest in the analysis of historical trends.

Thank you for this interesting suggestion.

MINOR POINTS

Line 182: the PgC/ppm CO_2 should be in upright font, since these are not symbols representing mathematical variables.

~~Will be changed~~Done (L185),[L187].

Similarly, throughout, the labels A, O, L should be in upright font.

~~Will be changed~~Done (throughout the paper).

In the figure captions, it may be helpful to the reader if the symbol (and maybe number of defining equation) was included after the text description.

~~We will consider this suggestion to improve the intelligibility~~Done (see new captions).

Note that actual (as opposed to CMIP model) human CO2 emissions include cement production and not just fuel use and also land use change and forestry.

Agreed ~~. We will revise the text to clarify this~~and corrected (L26),[L26].

With best regards,

Guilherme L. Torres Mendonça, Christian H. Reick and Julia Pongratz

Appendix A: Sensitivities transformations

In this appendix we show how from α and γ sensitivities defined using separate land and ocean temperatures one can compute their analogues defined by means of a single global temperature. Global temperature in a model is obtained by

$$\Delta T = \frac{\sum_i A_i \Delta T_i}{\sum_i A_i} = \frac{\sum_{i\in L} A_i \Delta T_i + \sum_{i\in O} A_i \Delta T_i}{\sum_{i\in L} A_i + \sum_{i\in O} A_i} = \frac{\sum_{i\in L} A_i \Delta T_i}{\sum_{i\in L} A_i}\frac{\sum_{i\in L} A_i}{\sum_i A_i} + \frac{\sum_{i\in O} A_i \Delta T_i}{\sum_{i\in O} A_i}\frac{\sum_{i\in O} A_i}{\sum_i A_i} =: \Delta T_L F_L + \Delta T_O F_O, \tag{A1}$$

where A_i and ΔT_i are the area and temperature of grid box i, $i \in L$ and $i \in O$ indicate sum over grid boxes on land/ocean, ΔT_L and ΔT_O are land/ocean temperatures, and F_L and F_O are the fractions of global area occupied by land and ocean.

Using a single global temperature, α is defined by

$$\Delta T = \alpha \Delta CO2. \tag{A2}$$

Using separate land and ocean temperatures one defines

$$\Delta T_L = \alpha_L \Delta CO2, \tag{A3}$$

$$\Delta T_O = \alpha_O \Delta CO2. \tag{A4}$$

Plugging (A1), (A3), (A4) into (A2) gives

$$\alpha = \alpha_L F_L + \alpha_O F_O. \tag{A5}$$

Taking a single global temperature, γ is defined by

$$\Delta C_X^{rad} = \gamma_X \Delta T, \tag{A6}$$

where X denotes the carbon response over land (L) or ocean (O).

Using separate land/ocean temperatures, γ can be defined by

$$\Delta C_X^{rad} = \gamma_X^* \Delta T_X. \tag{A7}$$

Inserting (A3)/(A4) and (A1) into (A7) gives

$$\gamma_X = \frac{\gamma_X^* \alpha_X}{F_L \alpha_L + F_O \alpha_O}. \tag{A8}$$

Since the Laplace-transformed formulation of the generalized framework is completely analogous to that of the original α-β-γ framework, (A5) and (A8) extend straightforwardly to the respective generalized sensitivities:

$$\widetilde{\chi}_\alpha = \widetilde{\chi}_\alpha^{(L)} F_L + \widetilde{\chi}_\alpha^{(O)} F_O, \tag{A9}$$

$$\widetilde{\chi}_\gamma^{(X)} = \frac{\widetilde{\chi}_\gamma^{(X,*)} \widetilde{\chi}_\alpha^{(X)}}{F_L \widetilde{\chi}_\alpha^{(L)} + F_O \widetilde{\chi}_\alpha^{(O)}}. \tag{A10}$$

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We thank the reviewer for the thorough review of our paper. Below we reproduce the reviewer’s comments in bold and write our answers in italic.

Authors present a new framework for representing the carbon feedbacks in the climate system that takes into account the history/memory of the system by using a convolution function based on Volterra series. This is indeed a new development that is welcome. The paper is written extremely well and should be published. I only have minor comments to improve the readability/clarity of the paper. I note the background of the first author in math. This may not be the case for a lot of carbon cycle folks, including myself. Hence a lot of math related questions in the following minor comments and my request to simplify/clarify things for a more general audience.

I also apologize for taking such a long time to review. This is a long paper. Unfortunately, I still haven’t made my way through the entire appendix, but I don’t want to hold this process on any longer.

We thank the reviewer for the appreciation. We will do our best to make the math-related issues as clear as possible to our intended audience.

Minor comments

1. **Recall that the carbon feedbacks framework can use results from any two of the three runs (COU, RAD, and BGC). Please note this in your manuscript and clarify that this manuscript uses the RAD and BGC runs.**

   ~~Will be done~~Done (footnote 5).

2. **Lines 28 and 29. Please changes “reaction” to “response”.**

   ~~Will be done~~Done (L28),[L29].

3. **Lines 6-80. This sentence is too long. Please also reword “the negative biogeochemical feedback is in terms of radiative forcing more than four times stronger than the positive radiative feedback” to make it more clear.**

~~Will be done~~Done (L6;L79–81),[L6;L79–81].

4. **Equation (1) – I think E(s)ds should be changed to E(t)dt for easy interpretation.**

   Sorry, but the variable "t" is already used for the integration range so that under the integral another variable name must be used to prevent confusion. Hence we ~~will~~ stay with $E(s)ds$.

5. **Line 255. Why is there is square in CAF(t)^2?**

   We indeed meant CAF, not CAF^2. The "2" was supposed to be a footnote index at the end of the sentence (see corresponding footnote in the same page 9). To avoid this misunderstanding we ~~will move~~ have moved this footnote index ~~somewhere else~~to "cumulative airborne fraction" (L267),[L273].

6. **Line 267. I am not a math expert but I didn't follow what the plus(+) sign in "lim(t->0+)" means.**

   The plus sign there indicates a one-sided limit, meaning that this is the limit of $\chi_\zeta(t)$ when t is approaching zero from the side of positive t-values. We ~~will add~~ have added a remark on this (L269–270),[275–277].

7. **Line 304 needs rewording – "Accordingly, when studying in the next section also these other CMIP5 models, we ..."**

   We ~~will rewrite the sentence~~ rewrote the second paragraph of section 3 to make it clearer (L323–337),[334–357].

8. **Line 319. Does "definition (12)" actually mean "equation (12)"?**

   Here we are indeed referring to Eq. (12), but we use "definition" to make it explicit that this is the equation that defines $A(t)$. To avoid confusion we ~~will change~~ have changed the wording to "defining equation" (L345),[L365].

9. **Why does the Laplace transform of equation (12) yields a p in the denominator in equation (13), and the Laplace transform of equation (15) doesn't (in equation 16).**

   This is essentially because the left-hand side of Eq. (12) is the time derivative of the left-hand side of Eq. (15). As briefly explained in the sentences introducing Eq. (~~12~~13), the Laplace transform of dC_A/dt is $p\Delta\widetilde{C}_A$ when assuming $\lim_{t\to 0^+}\Delta C_A(t) = 0$, which explains the p in the denominator of the right-hand side of Eq. (13). The left-hand side of Eq. (15), on the other hand, is not dC_A/dt but ΔC_A, whose Laplace transform is simply $\Delta\widetilde{C}_A$ – therefore no p shows up in the right-hand side of Eq. (16). This difference between the two equations ((12) with derivative, but (15) without) is the reason why in Eq. (17) $\widetilde{A}$ and $\widetilde{\chi}_\zeta$ are related by the factor p that makes the difference in the Laplace transforms of those equations. We ~~will extend the~~ have tried to make explanation preceding Eq. (~~12) and add a remark after~~ 13) (now (14)) clearer (see (L269–270),[275–276]) and added a footnote before Eq. (16) (now (17)) to make this point ~~immediately clear~~more readily understandable (see footnote 4).

10. **Lines 379-381 are somewhat difficult to follow. Can you simply say a delta CO2 of how many ppm is considered a linear regime?**

L379–381 summarizes what was more extensively explained in L304–309 in the introduction of section 3, namely that for the application to other CMIP5 models in section 4 we take (1) the same linear regime ranges found for the generalized sensitivities in MPI-ESM; (2) the same pre-processing procedures that gave best results in deriving the generalized sensitivities in MPI-ESM. All these technical issues are discussed in detail in Appendix A and have been compactly summarized in Table A2. But looking at this comment and also at comments 14 and 15, it is clear we need to make these issues more readily understandable. We ~~will therefore work~~ have therefore worked on the text to improve this ~~and also add a table in section 3 summarizing which experiments were used to obtain both the true and the predicted $\widetilde{A}(p)$~~(see (L308–337),[L314–357]) and extended the explanations in the caption of Fig. 1 instead of adding a table as we suggested in the original answer to the reviewer (striked out above).

11. The results in Figure 1 correspond to which scenario?

Depending on how to understand this question, we have two different answers:

(1) If the reviewer wants to know for which scenario the generalized airborne fraction showed in Fig. 6 ***is valid****, we feel an essential point of our study was not made sufficiently clear: key advancements of the generalized α-β-γ framework when compared to the standard Friedlingstein's framework are not only that – as noted by the reviewer – the memory of the system is now taken into account, but also that, as a consequence of considering this memory, the resulting quantities – e.g. generalized α-β-γ sensitivities, feedback functions and generalized airborne fraction –* ***are all invariant system properties and therefore scenario independent****, i.e. valid for any sufficiently weak perturbation scenario.*

(2) Alternatively, if the point above is clear but the reviewer is missing information on which experiment's data were used to compute the curves in Fig. 6, we fully agree that this information should be more readily accessible. But just to emphasize: since the generalized airborne fraction is scenario independent, the experiment's data from which it is derived is from a fundamental point of view irrelevant: in principle $\widetilde{A}(p)$ can be derived from any scenario experiment. The only difference the experiment's data make is that their signal-to-noise ratio and their level of nonlinearity is different for different experiments, which influences the quality of the derived $\widetilde{A}(p)$. In other words, if one successively derived $\widetilde{A}(p)$ from a series of experiments with an increasing signal-to-noise ratio and a decreasing level of nonlinearity, one would obtain a series of approximations of $\widetilde{A}(p)$ that are getting closer and closer to the "true" generalized airborne fraction of the system (details on these technical issues when recovering response functions such as the generalized airborne fraction can be found in Torres Mendonça et al., 2021a).

We ~~will make~~ have made adjustments in the revised paper to address these two possibilities: concerning (1), we ~~will work~~ have worked on the text to make clearer that the generalized airborne fraction and all derived quantities in the generalized framework are scenario independent ~~; and concerning (2), we will add in the caption of Fig. 6 a reference to the new table (see answer to comment 10) where all the technical details on the data used to derive $\widetilde{A}(p)$ will be found.~~((L145–146),[L146–147]; (L224–233),[L229–238]; (L432–435),[L454–457]; caption of Fig. 1); and concerning

(2), we have extended the explanations in the caption of Fig. 1 instead of adding a table as we suggested in the original answer to the reviewer (striked out above).

12. Equation (17) has a lot of meaning. It implies that airborne fraction in the frequency domain is not a function of emissions but rather a function of the feedbacks. Is this correct interpretation? If yes, please bring out this message more clearly.

If we understand the remark of the reviewer right, he is referring not to Eq. (17), but to Eq. (14), expressing the generalized airborne fraction fully by the feedback functions: $\widetilde{A}(p) = 1/(1-\widetilde{f}(p))$. *We agree almost completely with the reviewer's interpretation, except that it is not only valid in the "frequency domain", but also in the "time domain" – but maybe the reviewer has for the time domain not our generalized airborne fraction* $A(t)$ *in mind, but the standard airborne fraction* $AF(t)$. *To give a more complete answer, we address in the following three aspects of the reviewer's comment: the independence of the generalized airborne fraction from emissions, the circumstance that it is fully described by feedbacks, and the question on the validity of these properties in different domains.*

(1) That the generalized airborne fraction $A(t)$ *is independent of the emission scenario was already stated in our answer to comment 11. Note, however, that* $A(t)$ *differs from the standard airborne fraction* $AF(t)$ *(see definitions in Eqs. (11) and (12)) in that it is a generalized form of it (and also of* $CAF(t)$*) that by application of Eq. (12) can be used to predict the response of the atmospheric carbon accumulation rate for* **any** *given sufficiently weak emissions scenario. This scenario independence of* $A(t)$ *is exemplarily demonstrated in appendix F of our paper: in Fig. F1 of Appendix F, we show that given* $A(t)$*, one can successfully predict from it – within the linear regime – the standard* $AF(t)$ *of two different scenarios, although* $A(t)$ *was derived from the data of even other scenario experiments. From a more formal point of view, this scenario independence of A(t) arises because, by the defining equation (12),* $A(t)$ *can be understood as the functional derivative of the response* dC_A/dt *with respect to the perturbation* $E(t)$ *(Parr and Yang, 1989, Appendix A). Such a functional derivative (which is the kernel of the linear term of a Volterra expansion) reflects the internal sensitivity of the system when perturbed by emissions from a particular equilibrium state and has thus nothing to do with* $E(t)$ *itself. Its analogue for a system without memory is the linear coefficient of a Taylor expansion, which is also completely independent of the particular perturbation.*

(2) That a function such as this generalized airborne fraction can be fully described by feedbacks is well-known from the literature: from the viewpoint of the general theory of feedbacks, as developed for electronic amplifiers (see e.g. (Drosg and Steurer, 2014)) and in control theory (see e.g. en.wikipedia.org/wiki/Closed-loop_controller), the airborne fraction may be understood as a gain function relating the input (emissions) to the output (rate of atmospheric carbon change). For such gain functions it is well known that they can be fully expressed by feedback functions. This is well known in climate science (see e.g. Peixoto and Oort, 1992) since Hansen's et al. seminal paper (Hansen et al., 1984). In the context of the standard α-β-γ *formalism of carbon-climate feedbacks this gain property of airborne fraction was first recognized by Gregory et al. (2009) and was later on used in various studies (see e.g. Adloff et al., 2018; Jones and Friedlingstein, 2020). Insofar, we recover by Eq. (14) mostly well-established knowledge that needs in our opinion no special emphasis.*

The only thing new about Eq. (14) is that it is recovered in the generalized α-β-γ framework, for which (in contrast to the standard framework) the feedback functions appearing in Eq. (14) are time-scale dependent and scenario independent – which is indeed expected from the general theory of feedbacks that also accounts for memory.

(3) Finally, this scenario-independence and dependence on feedbacks holds not only in the time-scale domain ("frequency domain"), but also in the time domain, because the Laplace transform relating these two domains is a mere change of the mathematical representation that does not modify the physical meaning of the involved quantities.

Since in our view point (2) is well-known, we ~~will work~~ have worked on the text to bring out in particular points (1) and (3) more clearly ((L145–146),[L146–147]; (L224–233),[L229–238]; (L432–435), [L454–457]; caption of Fig. 1).

13. Equation (18) has a lot going on. In particular, what does the term between the last two "=" means physically?

$\chi_\zeta(t)$, defined by Eq. (15), is the response function that describes the response of atmospheric carbon to any weak emissions scenario. Physically it may be interpreted as the response of atmospheric carbon to a "pulse" in emissions (see Fig. B1 and related explanations in Appendix B). And the limit $t \to 0+$ means that only the value instantly after the pulse is requested. Because after the pulse all the added emissions are still in the atmosphere (land and ocean carbon uptake set in with a delay) it is by this interpretation obvious that one obtains in this limit "1" for the airborne fraction.

14. I felt, Section 3.3 needs more description of the experiments/simulations.

See our answer to the next comment.

15. Line 405. "In addition, because the two curves were obtained from very different simulations . . . ". Sorry, what simulations are being referred to here.

As anticipated in our response to comment 10, we ~~will add~~ have extended our explanations in the caption of Fig. 1 instead of adding a table to section 3 ~~a table summarizing all experiments used to compute A(p)~~as proposed in the original answer to the reviewer. See also our response to comment 11.

16. In the context of the airborne fraction, A, is it correct interpretation that A(t) depends on the emissions scenario while A(p) does not. If yes, again this is profound and should be brought out more clearly.

Please see our response to comment 12.

17. Please make it clear that you have assumed T*=0, i.e. the temperature change in the BGC simulation is ignored.

We ~~will do so~~found out that this was actually already done in the preprint version ((L381–385);[L402–406]); therefore no further changes were made in this respect.

18. Lines 511-512. "In contrast, for all models the predicted beta(O)(t) is for times larger than 15 years systematically too high, and . . . ". This sentence is unclear.

We ~~will reformulate it~~ have reformulated it (L545–547); [L568–570].

19. Note that typically we want the perturbation to be larger to enhance the response. In the usual carbon feedbacks analysis the feedback metrics are highly variable when c’ and especially T’ are small. Only when c’ and T’ have increased sufficiently then the feedback metrics settle down.

In contrast, your analysis requires c’ < 95 ppm to keep things in the linear regime.

Can these two statements be reconciled?

Thank you for this remark, this is certainly a good point that we should take up in the revised paper. Indeed, when computed at small perturbation strengths, the values of the α, β *and* γ *sensitivities can be highly variable – this is well seen in our Fig. 3, in particular for those sensitivities whose calculation involves temperature, as correctly pointed out by the reviewer.*

But in contrast to the typical α, β *and* γ *sensitivities, the generalized sensitivities are smooth even when computed from small-perturbation experiments because we explicitly account for the noise – i.e. internal variability – in the data when calculating them (although simulation data with better signal-to-noise ratio do improve the results and we do take advantage of this in our calculations; see details in Torres Mendonça et al., 2021a, b, and Appendix A). That the thereby derived generalized sensitivities are indeed robust may be seen e.g. in our paper (Torres Mendonça et al., 2021b, Figs. 5, 8) and in Appendix A of the present paper (Figs. A1, A2, A3, A4), where we demonstrate that with a single generalized sensitivity one can predict e.g. the “bgc” or the “rad” response of the model for different perturbation scenarios (which is well-known to fail for Friedlingstein’s framework, as shown e.g. by Gregory et al., 2009).*

And since the generalized sensitivities can be used to predict the response of the model in different scenarios, we show in Fig. 3 that they can also be used to predict the values of the typical α, β *and* γ*, but with an important difference: while the values calculated directly from the data can, as already mentioned, be highly variable, the values predicted from the generalized sensitivities are well-defined. The reason, as explained in the discussion of Fig. 3, is that the generalized sensitivities are predicting the response of the model not in individual noisy realizations, but in the ensemble mean (i.e. the mean of an ensemble starting from many different initial conditions).*

We ~~will expand our~~ added a specific remark on this issue in section 4.1 to make this clearer (L557–559); [L580–582]).

20. First sentence of section 4.2 – “Before in the next section finally the main question of this study on the role of feedbacks . . . ” needs rewording.

We ~~will rewrite it~~ rewrote it ((L563–564), [L586–587]).

21. In Figure 5b what is the y-axis unit for “Feedback function”?

As can be seen from their implicit definition in Eq. (14), the feedback functions are dimensionless. We ~~will make~~ made a remark on this (caption of Fig. 5).

22. Lines 594-595, “These results are in particular at short time scales in contrast with previous estimates (Gregory et al., 2009; Arora et al., 2013) using Friedlingstein’s framework, which suggested that the biogeochemical feedback is about 4 times larger than the radiative feedback”.

Note that the 4 times number was in the context of C units (Pg C). Hence my question in bullet 21 (what are the y-axis units in Figure 5b).

We are not completely sure we understand this comment. If the reviewer is referring to the fact that β and γ have different units and are therefore not directly comparable, the definition of feedback functions does account for that: as explained above, these functions are dimensionless, so that the magnitudes of f_β and $f_{\gamma\alpha}$ can indeed be compared.

But if the reviewer is pointing out instead that our and previous estimates are not entirely comparable, we fully agree. While previous estimates were made for a particular scenario, our estimate is more general in the sense that it is valid for any (weak) perturbation scenario. We still think mentioning those previous results is helpful to emphasize our finding, ~~but we will add~~ so we have just added a remark in the revised paper to clarify this difference ((L632–634); [L655–657]).

23. Lines 676-679. This last sentence of this paragraph is unclear. Please consider rewording.

This sentence summarizes one of our main conclusions and thus should indeed be formulated such that it can be readily understood. ~~Will be done~~Done ((L714–717), [L737–741]).

24. Line 813. I do not follow how $\chi^{(O)}_{\beta,\ln}(t) = \chi^{(O)}_{\beta}(t)$.

As mentioned in the text, this is explained in (Torres Mendonça et al., 2021b, section 4): starting from Eq. (A3)

$$\Delta C_O^{bgc}(t) = \int_0^t \chi^{(O)}_{\beta,\ln}(t-s) c_{PI} \ln\left(\frac{c(s)}{c_{PI}}\right) ds, \tag{a}$$

and expanding the perturbation term $c_{PI}\ln\left(\frac{c(t)}{c_{PI}}\right)$ into c, one obtains

$$\Delta C_O^{bgc}(t) = \int_0^t \chi^{(O)}_{\beta,\ln}(t-s) \Delta c(s) ds + \mathcal{O}((\Delta c)^2). \tag{b}$$

Taking now Δc sufficiently small and comparing the result to Eq. (A2) finally gives

$$\chi^{(O)}_{\beta}(t) = \chi^{(O)}_{\beta,\ln}(t). \tag{c}$$

All this is explained in particular in subsection 4.1, Eqs. (16), (18) and (19) of our paper – we ~~will~~ now mention this to better guide the interested reader ((L857), [L881]).

25. What are the units of prediction error in Figure A1a on y-axis?

It is dimensionless. We ~~will add a note on this after~~ have added a remark on this before introducing the prediction error in equation (A1) ((L827),[L851]).

With best regards,

Guilherme L. Torres Mendonça, Christian H. Reick and Julia Pongratz

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