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

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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.

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

   Will be done.

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.

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 this footnote index somewhere else.

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 a remark on this.

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

We will rewrite the sentence to make it clearer.

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 the wording to "defining equation".

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), 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 explanation preceding Eq. (12) and add a remark after Eq. (16) to make this point immediately clear.

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 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* $\widetilde{A}(p)$.

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 adjustments in the revised paper to address these two possibilities: concerning (1), we will work 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.*

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 on the text to bring out in particular points (1) and (3) more clearly.

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 to section 3 a table summarizing all experiments used to compute A(p). 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.

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.

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 remark on this issue in section 4.1 to make this clearer.

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.

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 a remark on this.

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 a remark in the revised paper to clarify this difference.

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.

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 mention this to better guide the interested reader.

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 introducing the prediction error in equation (A1).

With best regards,

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

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