Our responses are in black, marked as **[Response]**, and the comments of the Reviewers are in purple, marked as **[Comment]**. In our responses, we mark the changes in the manuscript with shading and separate comments using "***********".

Reviewer #1 (Remarks to the Author):

The authors investigate carbon cycle feedbacks under CO2 and non-CO2 GHG forcings. Since non-CO2 GHG lead to warming only, the CO2 concentration induced component of the carbon cycle feedbacks is missing for this forcing. This motivates the authors to investigate what has been termed "non-linearity of carbon cycle feedbacks" in previous studies, but with a focus on non-CO2 forcings. The authors use an impressive set of idealized model experiments to separate the different feedback components. The manuscript is generally well written, well structured, and the methods are sound and well described, although some parts of the manuscript could be improved in clarity and I found some of the results difficult to understand (see my comments below). There are only very few studies dealing with the interactions of non-CO2 GHG forcing and the carbon cycle, even though non-CO2 GHG reduction will be an important climate mitigation measure in pathways that limit global warming to below 2 degrees. Although the main results do not seem to be very surprising, I believe this study is a valuable contribution to this field and I would recommend publication in Earth System Dynamics after my comments listed below have been addressed.

We thank Dr. Schwinger for taking the time to read the manuscript and provide detailed and insightful comments and suggestions that helped to improve the manuscript.

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General comments

[Comment 1]

The topic of this study is complicated and not easy to grasp for a reader without specific knowledge of the carbon-cycle feedback literature. I would therefore encourage the authors to critically review their introduction and provide more explanation of the basic concepts and how they are related to the main topic of the study, the differences between CO2 and non-CO2 GHG forcings. More specifically, I think a link between the non-linearity of carbon cycle feedbacks and the feedbacks due to non-CO2 GHG needs to be made, given that this topic is discussed quite extensively later in the manuscript. It would be a good idea to add **a paragraph to the introduction that deals with the fact (and the causes for) that**

temperature mediated feedbacks can be different under rising or constant CO2, and that this is the main difference between CO2 and non-CO2 GHG mediated feedbacks. Here it would be also pertinent to cite the two (to my knowledge) studies that have investigated the topic of non-linearity previously (Zickfeld et al. 2011 and Schwinger et al. 2014, both studies did not deal with non-CO2 forcings). Also, in the Methods and Table 1, there are some sources of confusion, which should be addressed (see my specific comments below).

On a related note, why do the authors not go a step further and introduce a new symbol for the cross term? A clear definition of the "non-linear" or "cross-term" has been hampered by the fact that in the first studies using the beta/gamma framework (Friedlingstein et al. 2003, 2006), gamma was defined by [CO2]-[CO2bgc]. For this reason, also later studies that actually had a [CO2rad] simulation available continued using the term gamma for both climate carbon feedbacks [CO2rad] and [CO2]-[CO2bgc], as the authors mention themselves. This study might be a good opportunity to clean up with this "notational mess"?

[Response]

We are grateful for this comment and for the insight around the "notational mess". As suggested, in Introduction we added a paragraph that introduces the nonlinearity concept with the citations on the suggested studies.

The weakening of land and ocean carbon sinks due to non-CO_2 GHGs underscores the importance of understanding the differences in carbon cycle feedbacks between CO_2 and non-CO_2 GHGs. Only the changes in CO_2 concentrations are associated with the carbon-concentration (β) feedback, that is the response of the land and ocean carbon uptake to the changes in CO_2 concentration, mainly via the stimulation of photosynthesis through CO_2 fertilisation effect over land and the solubility pump over the ocean. The changes in both CO_2 and non-CO_2 concentrations are associated with the carbon-climate feedback (γ), that is the response of the land and ocean carbon uptake to climate change, mainly via the increased plant and soil respiration over land and reduction of the CO_2 solubility in the ocean with warming (Arora et al., 2013; Schwinger et al., 2014; Zickfeld et al., 2011). Under changing CO_2 concentrations, land and ocean carbon storages are simultaneously exposed to the carbon-concentration and carbon-climate feedbacks. However, the interaction between these feedbacks can introduce a non-linearity in the system, whereby the combined effect is not simply the sum of individual feedbacks. Thus, temperature-mediated feedback can differ under changing versus constant CO_2 levels, an important distinction when comparing CO_2 and non-CO_2 GHG feedback mechanisms. Here, it is also important to acknowledge that other factors, such as time lags and potential irreversibilities in the climate system, may also contribute to these differences (Boucher et al., 2012; Chimuka et al., 2023; Schwinger et al., 2014).

Previous studies investigated the nonlinearity in the carbon cycle feedback and revealed that the nonlinearity, or the cross term, may be comparable in size with γ (Schwinger et al., 2014; Zickfeld et al., 2011). They attributed the nonlinearity to the different responses of the land biosphere to the temperature changes, depending on the presence or absence of the CO_2 fertilisation effect, as well as the weakening of ocean circulation and mixing between water masses of different temperatures. However, these studies did not consider non-CO_2 GHGs.

We also fixed the mix-up in Table 1.

Following the Reviewer's suggestion, we introduced a symbol χ for the cross-term. We modified the Methods section to clarify the reasoning behind the need for the new symbol.

Zickfeld et al. (2011) and Schwinger et al. (2014) demonstrated that Eq. (4) includes the residual term ε, which can be derived from the difference between [CO_2] – [CO_2bgc] and [CO_2rad] - [piControl], using Eq. (5):

$$\varepsilon = \Delta U_{COU} - \Delta U_{BGC} - \Delta U_{RAD}. \quad (5)$$

These studies revealed that the residual 'nonlinearity' term depends on both CO_2 concentration and climate change, and it can be of the same order of magnitude as the γ term. Here, we propose attributing the residual nonlinearity to a cross term, associated with the nonlinearity feedback χ. Although many recent studies continued to attribute χ to the γ feedback—partly due to the absence of the [CO_2rad] experiment in some experimental designs, and also because this approach has been widely established in earlier research (Friedlingstein et al., 2003, 2006)—we show that these metrics become less well-defined when examining the effects of both CO_2 and non-CO_2 GHGs on the carbon cycle.

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[Comment 2]

In the section on the physical climate (section 3.1), the strongest warming is found in [CO2rad], but it is not explained why. [CO2rad] is warmer, particularly in the Arctic, than both [CO2] and [nonCO2], if I am not mistaken. Results show no very strong CO2 physiological warming in [CO2bgc], but nevertheless the CO2 physiological warming is used to explain the differences in simulations several times (e.g. lines 221-222), and it remains completely unclear to me why then [CO2rad] is the warmest simulation? **In previous studies, the strongest CO2 physiological warming was found in the Arctic region for CMIP5 ESMs (Park et al. 2020),**

with significant regional SAT contributions. This study, which includes the predecessor ESM IPSL-CM5A-LR, could be mentioned in the context of the CO2 physiological warming. In the present study, the authors find the CO2 induced total warming smaller than the radiative warming alone in high latitudes (line 224, Fig. S5e), which is opposite from the results of the Park et al. study. This needs at least to be mentioned and if possible some explanation should be provided (the authors mention differences in snow albedo as an explanation, but this is rather a consequence than a cause of the different surface temperatures?).

[Response]

Indeed, we completely missed this point in the original manuscript. In the revised version, we added discussion on the larger warming in the [CO_2rad] compared to the [CO_2] experiment. We added some discussion, including a comparison with the findings of Park et al. (2020). We also revised Fig. S3 to include the [CO_2rad] - [CO_2] combination.

We tested several potential mechanisms that could lead to a larger warming in [CO_2rad] compared to the [CO_2] experiment. Particularly, Park et al. (2020) describe two contrasting effects of CO_2 fertilisation: (1) CO_2 leads to reduction in the stomatal conductance, which in its turn decreases evaporative cooling, and (2) CO_2 leads to higher leaf area index, which (i) increases evaporative cooling and (ii) decreases albedo, which also leads to warming. We cannot approve either of these mechanisms because land evaporation is slightly higher in the [CO_2rad] experiment (Fig S5).

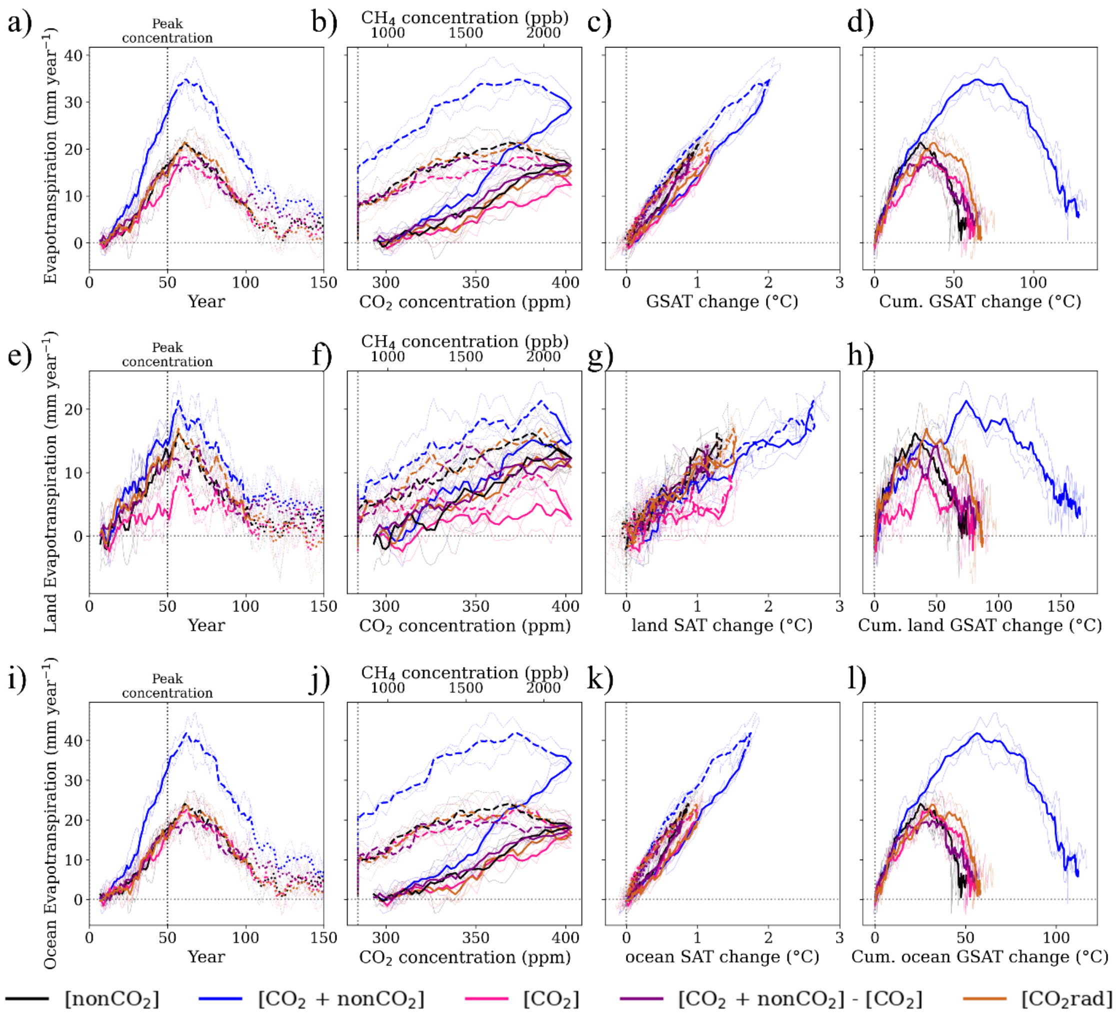

Figure S5. (a–d) Global, (e–h) land and (i–l) ocean annually-averaged changes in evapotranspiration (mm year^{-1}) as a function of (a, e, i) time (year), (b, f, j) CO_2 concentration (ppm) / CH_4 concentration (ppb, only for [nonCO$_2$]), (c, g, k) GSAT (ºC) and (d, h, l) cumulative GSAT (ºC).

The behaviour of the IPSL-CM6A-LR model remains the same in other similar experiments. Comparison of CMIP6 1pctCO2 (fully-coupled experiment with 1% CO_2 increase per year) and 1pctCO2-rad (same but radiatively-coupled) shows that, in agreement with our results, the fully coupled experiment produces a slightly higher surface air temperature increase, especially in the northern high latitudes, at moderate CO_2 levels (Figure R1). Similar behaviour can be seen in the GFDL-ESM4 simulations but is absent in the NorESM2-LM. As noted by the Reviewer, the ensemble size in our study is small and the effects of the model's internal variability should be considerable.

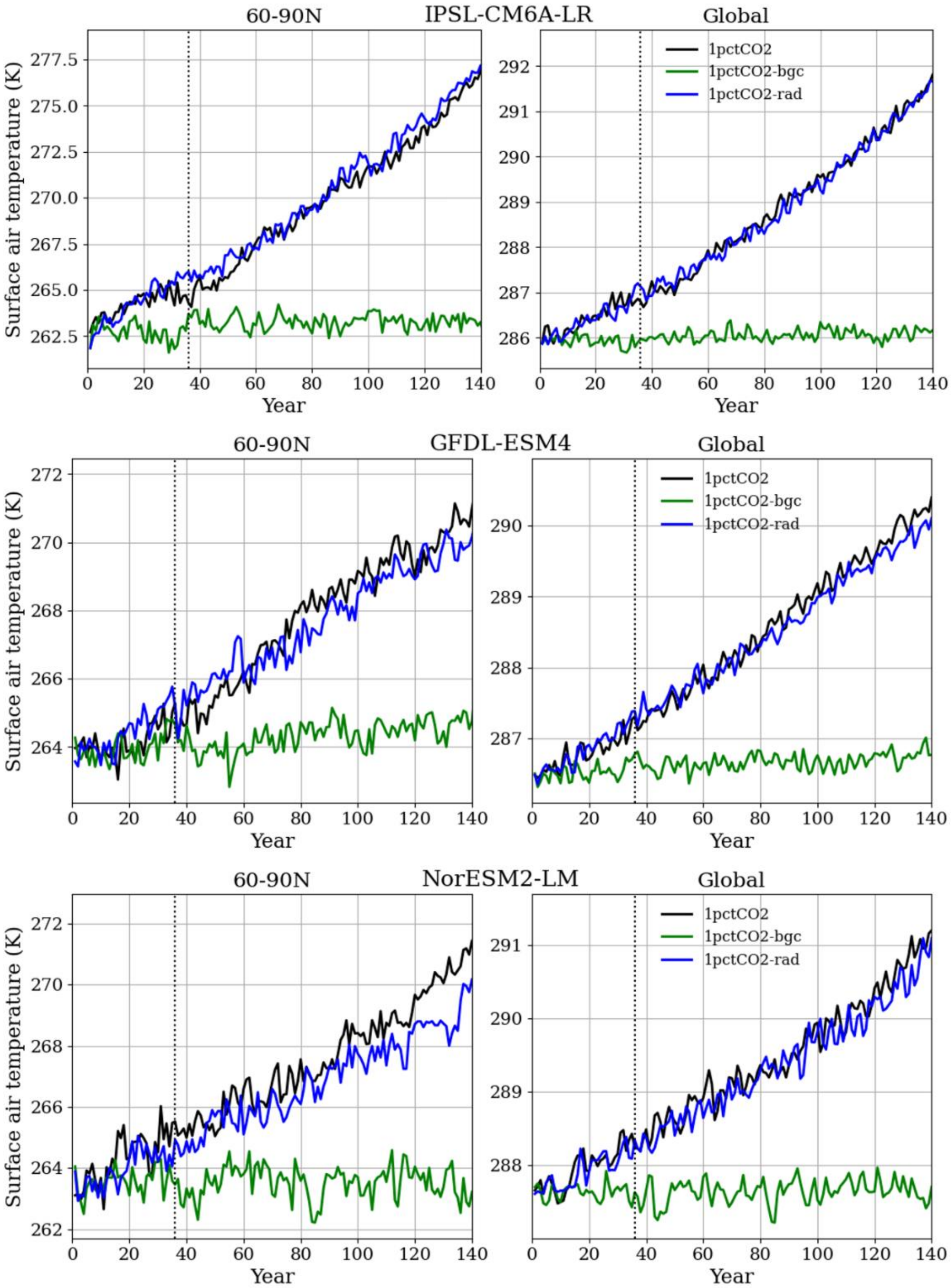

Figure R1. Time series of (left) northern high-latitude (>60º N) and (right) global surface air temperature increase (K) in the radiatively-, biogeochemically- and fully-coupled 1pctCO2 experiments by selected CMIP6 ESMs. The vertical dotted line indicates year, when the experiment's CO_2 concentration is nearly equal to the maximum CO_2 concentration (403 ppm) of this study.

We have added the following discussion of the differences between our results and those of Park et al. (2020).

The combined effects of CO_2 physiological and radiative forcing do not lead to more warming, as the radiative forcing alone ([CO_2rad] experiment) leads to a slightly higher global temperature increase compared to the coupled [CO_2] experiment (Fig. 2a, b). This temperature difference is particularly evident in the Arctic region (Fig. S3a). Our findings differ from those of a CMIP5 intercomparison study, which reported that CO_2 physiological warming amplifies the Arctic warming (Park et al., 2020). The study showed that the CO_2 physiological effect contributes to high-latitude warming by reducing evaporative cooling due to stomatal closure under elevated CO_2 levels. In contrast, we observe higher evapotranspiration in the [CO_2rad] compared to the [CO_2] experiment (Fig. S5), which is probably a consequence of the lower warming in the [CO_2] experiment. In our study, the greater warming in the [CO_2rad] experiment may be driven by increased surface albedo, especially over the Arctic Ocean (Fig. S3b). While the underlying causes remain unclear, this pattern appears consistent in other experiments conducted with IPSL-CM6A-LR under moderate CO_2 levels (not shown). Because the ensemble size in our study is limited and the effects of the model's internal variability should be considerable, future research should validate the robustness of our findings with larger ensemble simulations.

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[Comment 3]

Table 1 is somewhat confusing. Column 4 refers only to beta and gamma such that both experiments [CO2rad] and [CO2]-[CO2bgc] appear to be the same (they include the carbon cycle feedback "CO2 gamma"), but it is not mentioned that the cross-term is present in [CO2]-[CO2bgc]. The same is true for [nonCO2] and [CO2bgc+nonCO2]-[CO2bgc]. Also, in the 5th column the only term listed for [CO2]-[CO2bgc] is the cross term, while the actual gamma-term is missing. Again, the same is true for [CO2bgc+nonCO2]-[CO2bgc]. In the footnotes, the terms $\Delta U_{\gamma,CO2physiological}$ and $\Delta U_{\beta\gamma,CO2physilogical}$ are not defined anywhere. I would suggest to just say that the warming from the physiological CO2 forcing is assumed to be negligible.

[Response]

Columns 4 ("Included carbon cycle feedback") and 5 ("Included carbon cycle terms from Eq.2") in the original manuscript included such information, and apparently column 4 adds more confusion than clarity to the description of the experimental design. Thus, in the revised manuscript we delete it.

As the Reviewer pointed out, the terms in column 5 had errors on the included terms, which is now corrected. We also remove the original explanation on the $\Delta U_{\gamma,CO2physiological}$ and $\Delta U_{\beta\gamma,CO2physilogical}$ terms and added the following instead:

*according to equations by Etminan et al. (2016), warming from the physiological CO_2 forcing is assumed to be negligible.

****

[Comment 4]

In the abstract (line 21-22), even if Arora et al 2020 and Schwinger et al. 2014, did not use the term “cross-term” but “non-linearity”, the results are consistent with these studies. So I would suggest adding “consistent with previous studies that considered CO2 forcing only”.

[Response]

We agree with the suggestion and revised the abstract accordingly.

We introduce a framework, consistent with previous studies that focused exclusively on CO_2 forcing, to separate the carbon-climate feedback into the temperature and cross terms. Our findings reveal that these feedback terms are comparable in magnitude for the global ocean. This underscores the importance of considering both terms in carbon cycle feedback framework and climate change mitigation strategies.

****

Specific comments

[Comment 1]

Equation 2: It might be pertinent to cite Schwinger et al. 2014 here, who used the Taylor expansion to define “nonlinearity” of carbon cycle feedbacks. Please double check the factor 1/2 in the cross-term (also in Equation 5), which is wrong I believe (only the quadratic terms have the factor of 1/2).

[Response]

As suggested, we added the citation, changing text to:

Following Schwinger et al. (2014) the formulation can be expanded to a Taylor series…

Besides we agree that the factor of 1/2 is wrong here, removed.

****

[Comment 2]

Equations 3-4: Why are the quadratic terms included here? They cannot be quantified, so they belong to the residual term in the context of this study.

[Response]

We respectfully disagree, because via our analysis, we conclude that second-order terms (quadratic terms) cannot be neglected. We think it is necessary to show them consistently with the quadratic term that is needed to define the cross term (ΔU_χ), as shown below.

$$\Delta U_\beta = \frac{\partial U}{\partial C_{CO2}} \Delta C_{CO2} + \frac{1}{2}\frac{\partial^2 U}{\partial {C_{CO2}}^2}(\Delta C_{co2})^2 + Res., \quad (7)$$

$$\Delta U_\gamma = \frac{\partial U}{\partial T}\Delta T + \frac{1}{2}\frac{\partial^2 U}{\partial T^2}\Delta T^2 + Res., \quad (8)$$

$$\Delta U_\chi = \frac{\partial^2 U}{\partial C_{CO2}\,\partial T}\Delta T \Delta C_{CO2} + Res.. \quad (9)$$

****

[Comment 3]

Line 19-20: Please double check the sentence: Shouldn't this be the other way around – "Non-CO2 forcing primarily affects temperature driven feedbacks…" or did I misunderstand something here?

[Response]

This indeed should be the other way around, now corrected.

CO_2 forcing affects both carbon-climate and carbon-concentration feedbacks, whereas non-CO_2 gases influence only the carbon-climate feedback.

****

[Comment 4]

Line 22: It is a bit unclear what "both components" refers to. Also, non-CO2 forcing are usually considered in Earth system modelling, e.g., in SSP scenarios. Please reword this sentence to make the main conclusion of this paper clearer.

[Response]

We changed the wording to “feedback terms” to be consistent with the previous sentence. We further reworded the last sentence of the abstract by rewording “considered in Earth system modelling” to “considered in carbon cycle feedback framework”. Now it reads as follows:

Our findings reveal that these feedback terms are comparable in magnitude for the global ocean. This underscores the importance of considering both terms in carbon cycle feedback framework and climate change mitigation strategies.

****

[Comment 5]

Line 75-81: “like many contemporary models” could be made more specific by saying “like all other ESMs participating in CMIP6” or similar. Generally, I think this paragraph is not necessary here. These are idealized concentration-driven experiments, so why discuss the lack of CH4 and N2O emission-driven capability in the Introduction? Particularly, since a section on “limitations” exists at the end of the manuscript. I would suggest deleting this paragraph and move parts of the text to Section 4.

[Response]

We followed the Reviewer’s suggestion by deleting this paragraph and moving part of it to the section on study limitations as follows:

However for this study, the use of the model is justified because current changes in CH_4 and N_2O concentrations are primarily driven by anthropogenic sources, suggesting that the absence of interactive modules of natural sink/source processes does not significantly affect the representation of natural variability trends for the CH_4 and N_2O concentration (Nakazawa, 2020; Palazzo Corner et al., 2023; Zhu et al., 2013).

****

[Comment 6]

Line 210-214: Although the physiological warming might be “significant” it is still quite small. Also, I would suggest being more careful here (and elsewhere in the manuscript), since the ensemble size is small and decadal scale variability can still be present in the ensemble mean. For example, the “significant CO2 physiological warming” in [CO2bgc] over “the high latitudes of land and ocean during stabilization period” could very well be an effect of AMOC, which happens to be significantly stronger over much of the stabilization period of [CO2bgc] compared to [piControl] in two of three ensemble members (Fig. S1a).

[Response]

We agree and, thus, changed the paragraph to include a more careful statement.

The CO_2 physiological warming that can be quantified by comparing [CO_2bgc] with [piControl] is small (green line in Fig. 2). Spatially, some differences are ubiquitous over land, e.g., CO_2 physiological warming persists over Eurasia during the ramp-up period, and over the high latitudes of both land and ocean during the stabilisation period (Fig. S3a). A larger ensemble size of model simulations would be required to investigate these differences more thoroughly. In our following analysis on carbon cycle feedbacks, we assume the CO_2 physiological warming to be negligible.

****

[Comment 7]

Line 219: “… the higher sensitivity to non-CO2 forcing compared to CO2 forcing”. This should be the other way round (SAT is higher under CO2 forcing)?

[Response]

Indeed, this should be the other way round, corrected.

****

[Comment 8]

Line 220-221: “The combined effect of CO2 physiological and radiative forcing leads to more warming in the coupled [CO2] experiment compared to both the [CO2rad] experiment.” I guess the “both” should be deleted? Also, I cannot see this in Fig 2a, here [CO2rad] shows a stronger warming than [CO2]. This is consistent with the figures in the supplementary, which also show that [CO2rad] seems to be warmer than both [CO2] and [nonCO2], particularly in the high latitudes (Fig S3a). What is the reason for this? Also, as mentioned above, this is different from the CMIP5 study of Park et al. 2020.

[Response]

We deleted the unnecessary “both” term. We agree with the comment and revised the paragraph, as described in our response to General comment 2.

****

[Comment 9]

Line 223-224: “…the CO2-induced total surface warming is larger than CO2-induced radiative warming almost everywhere, except for the high northern latitudes over the land and ocean (Fig. S3).” I can’t see this from Fig S3, because [CO2]-[CO2rad] is not shown there. Again, the most striking difference is that [CO2rad] is warmer than [nonCO2], particularly in high latitudes (and by comparison with the next column also warmer than [CO2] in the high latitudes. What is the reason for this difference?

[Response]

We added the [CO2]-[CO2rad] experiment to Fig. S3 in the revised manuscript version. We also revised the discussion, as described in our response to General comment 2.

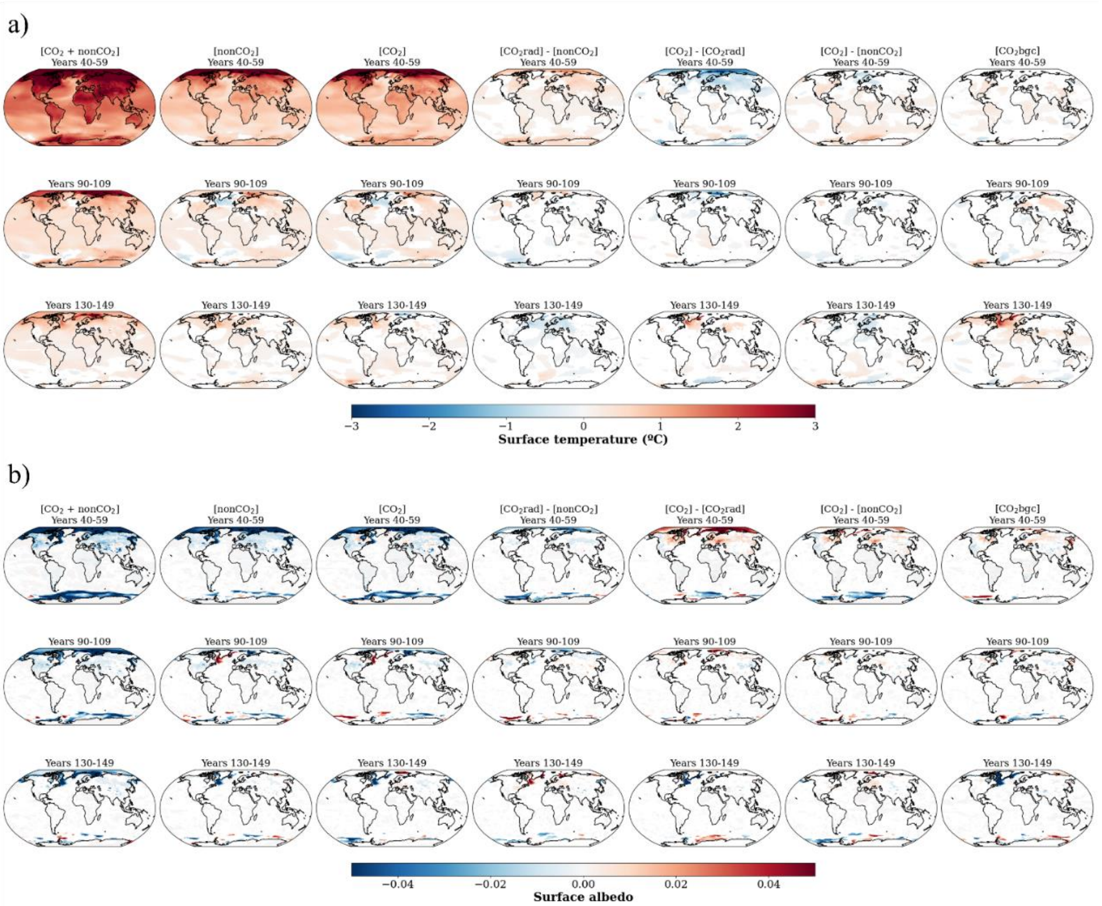

Figure S3. Spatial variation of three-member-ensemble mean changes in (a) surface temperature (ºC) and (b) surface albedo averaged over 20 years at the end of (first rows)

ramp-up, (middle rows) ramp-down, and (bottom rows) stabilisation phases relative to piControl under selected scenarios. We draw only grids significantly different from piControl (*p* < 0.1 based on t test, N=60) and between [CO_2], [CO_2rad] and [nonCO_2] experiments using three ensemble members (*p* < 0.1 based on t test, N=60).

****

[Comment 10]

Line 238-243: This paragraph is very confusing. It seems to repeat things that have been explained in the Methods section, but in a way that I doubt is helpful for the reader. I would suggest either rewording and expanding this paragraph or deleting it. Again, the terms $\Delta U_{\gamma,CO2physiological}$ and $\Delta U_{\beta\gamma,CO2physilogical}$ have never been defined in the manuscript.

[Response]

We now deleted the paragraph, as in section 3.1 we state that we assume CO_2 physiological warming to be negligible.

****

[Comment 11]

Table 2: While CO2 (and non-CO2 GHG) concentrations are all the same in the different concentration driven experiments, this is not the case for the temperature increase. For example, SAT is 10-15% lower for [nonCO2] compared to [CO2] and [CO2rad] (estimated from Fig.2). Therefore, I am wondering if it would not make more sense to give values for gammas in this table? I would expect $\Delta U_{\gamma,nonCO2}$ be somewhat lower than $\Delta U_{\gamma,CO2rad}$ just because of the lower temperature increase, while it is actually gamma which makes the most useful comparison between the simulations. More importantly, how are the cross-term carbon uptakes (first line in the lower part of the table) calculated? Shouldn't this be the difference between the second and fourth line of the upper part of the table? I cannot see this is the case.

[Response]

We chose to report values of cumulative fluxes rather than those of the feedback parameter because we wanted to show the changes for ramp-up, ramp-down and stabilisation periods. Estimation of the feedback parameter values for the end of the ramp-up period is possible and we included it in the newly added Table S1. We also added a column with experiment's peak temperatures (mean temperature at the end of ramp-up period) to Table 2.

However, due to the lagged responses of both temperature and carbon fluxes, estimation of feedback parameters for the ramp-down period is more challenging. Furthermore, we would face numerical issues for calculating carbon cycle feedback parameters for stabilisation and total periods.

The newly added Table S1 (below) shows larger negative γ for land and smaller negative γ for ocean in the [CO_2rad] compared to [nonCO_2] experiment, but these differences are not statistically significant.

We confirmed some errors in the table for the means of the cross terms. We corrected them in the revised manuscript.

Table 2. Cumulative CO_2 and climate change-driven changes in the land and ocean carbon fluxes (GtC), shown as three- member ensemble mean. The ± indicates one standard deviation among the three members. Note that all experiments are analysed relative to their [piControl] counterparts.

Experiment	Max. warming (K)*	Terms	Years 1-50 (ramp-up)		Years 51-100 (ramp-down)		Years 101-150 (stabilisation)		Total	
			Land	Ocean	Land	Ocean	Land	Ocean	Land	Ocean
[CO_2bgc]	0.1±0.0	$\Delta U_{\beta,CO2}$	179.3±2.2	103.8±0.7	-16.3±6.0	-19.7±1.0	-106.3±0.4	-32.1±0.8	59.4±1.8	53.4±0.9
[CO_2rad]	1.1±0.1	$\Delta U_{\gamma,CO2}$	-18.6±2.4	-2.4±0.2	4.5±5.7	0.0±1.4	11.4±4.0	0.3±1.9	-2.2±2.0	-2.1±0.6
[nonCO_2]	0.9±0.1	$\Delta U_{\gamma,nonCO2}$	-14.8±3.6	-2.3±1.0	1.3±6.0	-0.5±1.3	10.2±2.4	1.3±0.5	-2.5±2.9	-1.6±0.2
[CO_2] – [CO_2bgc]	1.0±0.0	$\Delta U_{\gamma,CO2} + \Delta U_{\chi,CO2}$	-14.7±1.0	-4.5±0.2	3.8±3.0	-1.4±1.0	8.2±1.9	0.6±0.5	-3.8±3.1	-5.5±1.4
		$\Delta U_{\chi,CO2}$	3.9±2.1	-2.2±0.4	-0.7±7.3	-1.4±0.5	-3.2±5.9	0.2±2.3	-1.6±5.0	-3.4±2.0
		$\Delta U_{\chi,nonCO2}$	3.5±1.5	-1.6±1.0	2.8±7.9	0.1±1.1	-1.6±1.5	-1.0±1.3	2.0±4.0	-2.5±1.0
		$\Delta U_{\gamma,CO2} - \Delta U_{\gamma,nonCO2}$	-3.8±5.0	-0.1±0.9	3.2±3.6	0.5±0.9	1.2±2.6	-1.0±2.2	0.3±2.7	-0.5±0.7
		$\Delta U_{\chi,CO2} - \Delta U_{\chi,nonCO2}$	0.4±3.5	-0.6±0.7	-3.5±3.4	-1.6±1.0	-1.6±7.4	1.3±3.3	-3.6±5.5	-1.0±2.8

* defined as the mean ΔGSAT during years 41-60.

Table S1. Changes in the carbon cycle feedback parameters for land and ocean at the end of the ramp-up period, shown as three- member ensemble mean. The ± indicates one standard deviation among the three members. We use temperature of the fully coupled experiments to estimate γ and χ feedbacks.

Experiment	Terms	Years 1-50 (ramp-up)	
		Land	Ocean
[CO_2bgc]	β_{CO2} (GtC ppm^{-1})	1.51 ± 0.02	0.88 ± 0.01
[CO_2rad]	γ_{CO2} (GtC K^{-1})	-17.02 ± 1.44	-2.17 ± 0.14
[nonCO_2]	γ_{nonCO2} (GtC K^{-1})	-16.74 ± 4.12	-2.58 ± 1.19
[CO_2] - [CO_2bgc] - [CO_2rad]	χ_{CO2} (GtC ppm^{-1} K^{-1})	0.03 ± 0.02	-0.02 ± 0.0
[nonCO_2bgc] - [CO_2bgc] - [nonCO_2]	χ_{nonCO2} (GtC ppm^{-1} K^{-1})	0.03 ± 0.01	-0.01 ± 0.01

****

[Comment 12]

Line 282-284: As mentioned above, it is a choice to “attribute” the cross-term to the carbon-climate feedback, which makes sense in the context of previous studies. But I don’t see why this would be necessary, and I would encourage the authors to drop this attribution and just go ahead with beta, gamma, and the cross-term (as mentioned above, maybe introduce a new symbol for the cross term?).

[Response]

We thank the Reviewer for the encouragement. We have divided the original "Carbon-Climate Feedback" section by creating a new section titled "Nonlinearity in Carbon Cycle Feedback." Additionally, we introduce the symbol χ to represent the cross term.

****

[Comment 13]

Line 304: “larger climate change driven carbon source” is not precise. It is rather a larger climate change driven reduction of the ocean sink. The ocean remains a sink throughout. Same comment applies for line 312.

[Response]

Revised accordingly.

Over ocean, the contribution from the χ term leads to a greater reduction in the carbon sink driven by climate change (Fig. 3).

..

Spatially, while the Southern Ocean remains the largest ocean carbon sink in all considered experiments involving atmospheric CO_2 changes, it, along with the Atlantic Ocean, undergoes the largest climate change-driven reduction in carbon sink (Fig. 4).

****

[Comment 14]

Line 324: Why would reducing non-CO2 GHG only change ΔU_{γ}? By changing temperature, the cross-term would be affected, too.

[Response]

Agreed, changed to "implies alteration of ΔU_{γ} and ΔU_{χ} terms."

****

[Comment 15]

Line 369-370: Again, the highest GSAT is found in [CO2rad] which is inconsistent with this conclusion.

[Response]

We removed this sentence from the Conclusions in the revised manuscript.

Technical comments

[Comment 1]

Line 37: delete "over"

[Response]

Deleted

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[Comment 2]

Line 69: consider changing to "to clarify whether the climate responses to declining CO2 and non-CO2 GHGs differ globally and regionally."

[Response]

Changed to the suggested formulation.

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[Comment 3]

Line 86: Place reference to Boucher et al. 2020 after the model name, not after CMIP. Replace CMIP by CMIP6

[Response]

Changed accordingly.

****

[Comment 4]

Line 96: Confusing sentence, please consider rewording. Maybe "… between a model experiment with perturbed GHG concentration but fixed sea surface and ice temperatures and a control simulation with pre-industrial GHG concentrations." or similar.

[Response]

Changed to the suggested formulation.

****

[Comment 5]

Line 108: "referred to" could be understood as if the effective concentrations are used in the text and figures. I would suggest rewording this sentence.

[Response]

We reworded the second half of the sentence, which now reads:

The effective concentrations of CH_4 and N_2O are used as input to the radiative transfer scheme of the climate model throughout the rest of this study. In the text and figures, these are presented as the actual (equivalent) concentrations.

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[Comment 6]

Line 156: Delete "atmospheric CO2 induced".

[Response]

Deleted.

****

[Comment 7]

Line 199: thermostatic -> thermosteric

[Response]

Corrected.

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[Comment 8]

Line 201: Consider replacing "under considered timescale" by "within the time-horizon considered here" or similar.

[Response]

Changed as suggested.

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[Comment 9]

Line 250: "… which induces carbon sink…" -> "which represents the CO_2 induced carbon sink…"

[Response]

Changed as suggested.

****

[Comment 10]

Line 254: Complicated sentence. Why not say "Over the ocean beta is positive (carbon sink) in all regions …"

[Response]

Changed, as suggested. The sentence now reads:

Over the ocean β is positive (carbon sink) in all regions during the ramp-up period (Fig. 4).

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[Comment 11]

Line 278: What do you mean by "prolonged duration of beta"? Please clarify.

[Response]

Changed to “the extended period of large β influence”.

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[Comment 12]

Line 286: Please spell out what “equivalent” means (within one standard deviation?).

[Response]

Added (within one standard deviation uncertainty range)”.

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[Comment 13]

Line 287: Remove subscript betas before “in Table 2”.

[Response]

Corrected

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[Comment 14]

Line 295: the gamma -> gamma

[Response]

Corrected.

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Reviewer references

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