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 did a good job in revising their manuscript, and I think it can be accepted for publication after a few minor things (listed below) have been addressed.

We thank Dr. Schwinger for taking the time to read the manuscript once again and providing positive feedback on the revised manuscript with additional comments to improve the manuscript.

****

General comments

[Comment 1]

Section 2.3: I'm not entirely convinced about how this section is structured, and the wording is sometimes a bit misleading.

The carbon uptake is not "estimated" (line 278), it is derived from the simulations. What is "estimated" are the feedback factors. Equations 3 and 4 are very confusing since equation 4 is not gamma but gamma+chi according to the framework introduced later. Why not begin this section with the Taylor expansion? This is the basis of the whole framework, and it would make it clear that gamma actually is defined at constant CO2 (i.e. we need the RAD simulation to determine it) gamma=(del U / del T)_{CO2=const}. The definition of the cross term would also become clear from the beginning, and that the COU-BGC simulation includes the cross term.

Regarding Eq. 7,8,9 I still would favor to omit the quadratic terms, or it should at least be made clearer that, when estimating the feedback factors from simulations, the estimate includes the quadratic terms. You say "we found them [the residual terms] to be negligible" How is it possible to determine them (you can't even determine the quadratic terms, if I'm not mistaken).

[Response]

We originally added the preface in this section, following a comment from Reviewer 2 who argued that the Section "assumes that readers have a solid grasp of the carbon cycle feedback framework and the feedback parameters (β, γ) used, which may not be the case." Reviewer 2 suggested to add a brief description of carbon cycle feedback parameters, equations for quantification, units and sign convention before introducing the Taylor expansion. We tend to agree with this argument and thus keep the preface in a shortened form. Particularly, we removed Eq. (4) ($\gamma = \frac{\Delta U_{COU-BGC}}{\Delta T}$) that may be confusing in regard to the framework introduced later and a paragraph describing the experiments with the corresponding terms.

We follow your suggestion regarding the second-order terms in Eq. (6) and (7) (with revised numbering). The text was changed to the following:

$$\Delta U_{\beta} = \frac{\partial U}{\partial C_{CO2}} \Delta C_{CO2} + Res., \quad (6)$$

$$\Delta U_{\gamma} = \frac{\partial U}{\partial T} \Delta T + Res., \quad (7)$$

…

For simplicity, the second-order terms of Eqs. (6) and (7) are included in $Res.$.

[Comment 2]

Many places in the manuscript, the authors use an imprecise wording regarding the feedback factors. For example (line 316), "During the ramp-up gamma drives a carbon sink…". It is climate warming that drives the carbon sink, and a positive value of gamma is the consequence. Beta, gamma and chi are diagnostic quantities, they are not drivers. I would encourage the authors to go once more through the results section and reword this and similar sentences (e.g., but not limited to, lines 327, 334ff, 339, 344).

[Response]

We revised our wording throughout the manuscript, especially in Sections 3.3 – 3.4 that show results on the carbon-concentration, carbon-climate and nonlinearity in carbon cycle feedback.

Specific/technical comments

[Comment 1]

line 20: “influence only the carbon-climate feedback” would read better “gives rise to a carbon climate feedback only”

[Response]

Changed as suggested.

****

[Comment 2]

line 21: “… however, focused exclusively on CO2 forcing” sounds strange, please consider rewording (or deleting since it seems not really necessary in an abstract)

[Response]

We reworded to:

We introduce a framework, building on previous studies that primarily addressed CO_2 forcing, to separate the carbon-climate feedback into a temperature term and a temperature–CO_2 cross term.

****

[Comments 3–6]

line 22: the term “cross term” is not self-explanatory. Maybe better say “… into a temperature and a temperature-CO2 cross term” or similar.

line 35: of → over

line 62: “are associated” → maybe better “give rise to a carbon-concentration feedback” (I believe using the symbol beta without further introducing it here is not necessary).

line 65: same comment as for line 62

Changed as suggested.

****

[Comment 7]

line 75: Since “the cross term” is introduced here it would be good to say what this means, e.g., “the cross term arising from interactions of changing atmospheric CO2 and changing temperatures”

[Response]

Changed, now reads:

Previous studies investigated the nonlinearity in the carbon cycle feedback, showing that the cross term—arising from interactions between changing atmospheric CO_2 and temperatures—can be comparable in size with γ.

****

[Comment 8]

line 93: consider changing to “… to investigate the nonlinearities of carbon cycle feedbacks …”; consider deleting “different”.

[Response]

Changed as suggested.

****

[Comment 9]

line 130: “that includes only CO2 physiological forcing” this is confusing for the reader. First and foremost this experiment includes CO2 forcing that is only seen by the land and ocean. Then there is also a small temperature forcing due to the CO2 physiological forcing. Please consider rewording.

[Response]

Changed to:

…a biogeochemically coupled (BGC) experiment where CO_2 forcing affects only the carbon cycle of land and ocean [CO_2bgc] (with minor temperature effects from CO_2 physiological forcing)…

****

[Comment 10–12]

line 133: “feedback nonlinearities” → “nonlinearities of feedbacks”

line 144: it is the ERF that is estimated from Etminan et al. not the concentrations, right? If so please consider moving this after “3.69 W m-2” other wise it is confusing.

line 153: “a-c panels” → “panels a-c”

Changed as suggested.

****

[Comment 13]

line 221: maybe worth noting that Asaadi et al. 2024 found that the effect of the warming on beta is indeed negligible

[Response]

We added the citation.

…consistent with findings of Asaadi et al., (2024).

****

[Comments 14–15]

line 242: “maximum” → “strongest decrease”

line 344: “compared to when atmospheric CO2 is constant” → “compared to the RAD experiment, in which atmospheric CO2 is constant”

[Response]

Changed as suggested.

****

[Comment 16]

line 354: Should “carbon-concentration” read “carbon-climate”?

[Response]

We removed the clarification in brackets (originally “which involve carbon-concentration feedback alterations”) altogether, as it is thoroughly explained in the following sentence.

****

[Comments 17–20]

line 355: delete “concentration”? I find it confusing in this context.

Figure 4: I would suggest to change the y-axis labels from “Delta U_{beta gamma}” to Delta U_{Chi}

line 410: “the presence of carbon concentration feedback…” would be better worded as “increasing atmospheric CO2 amplifies the reduction of the climate change driven

line 412: maybe better: “… and a component driven by climate change and rising atmospheric CO2 at the same time, i.e. a cross term.”

[Response]

Changed as suggested.

****

Reviewer #2 (Remarks to the Author):

The manuscript is in good shape. I would just suggest one change for clarity: changing the wording in the second research question from "carbon cycle non-linearity feedback" to "carbon cycle non-linearity" (Lines 101-103 in the tracked changes version of the manuscript).

[Response]

We thank the Reviewer for the positive feedback of the revised manuscript. We made the change for clarity, and now it reads “the nonlinearities of carbon cycle feedbacks”.