Authors’ response to the review by Kirsten Zickfeld

We thank Kirsten Zickfeld for the positive assessment of our manuscript and for providing constructive and valuable criticism. We have carefully revised our manuscript, addressing each point raised as outlined in our point-by-point response below (original comments in gray, italic font). Proposed verbatim alterations or additions to the manuscript are highlighted in red.

Asaadi et al. quantify carbon cycle feedback in scenarios with positive and negative emissions using an ensemble of ESMs. They use cumulative and flux-based carbon cycle feedback metrics to quantify global as well as regional carbon cycle feedbacks. They also use a decomposition approach to quantify the contribution of various carbon cycle processes to carbon-concentration feedback strength. They find that the carbon-concentration and carbon-climate feedbacks as well as the uncertainty in these feedbacks increase during the negative emissions phase.

The manuscript is a valuable contribution to a small existing body literature on carbon-cycle feedbacks under negative CO2 emissions. It is mostly well written, clearly structured and informative. There are, however, several aspects that need to be addressed before the paper is acceptable for publication in Biogeociences.

Thank you for this positive evaluation.

- *Non-CO2 radiative forcing in SSP3.4-OS simulations. The inclusion of non-CO2 radiative forcing in SSP5-3.4OS simulations hampers the comparability of carbon cycle feedbacks with the idealized 1pctco2 simulation. The temperature changes induced by non-CO2 forcings are significant in some models and the response to these changes in the BGC simulations confounds the response to changes in atmospheric CO2. Given this complication, along with the effect of residual land-use changes and the small period of net negative emissions in SSP5-3.4OS, I would like to encourage the authors to think about whether inclusion of this scenario in the paper is warranted. The paper is quite long and a stronger focus would be beneficial. If the authors decide to retain analysis of the SSP3.4-OS simulations in the manuscript, a more in-depth discussion of the non-CO2 induced temperature effects is needed.*

We agree that the complications related to non-CO_2 radiative forcings in the ssp534-over scenario were not discussed in enough detail. The feedback framework as used here is in principle suited to be applied to simulations with non-CO_2 forcings as long as the assumption of linearity holds (see discussion in proposed new text). We believe that adding a concise discussion of this issue (rather than removing the ssp534-over scenario altogether) would strengthen our manuscript. We propose to add a short paragraph after line 178 of the preprint manuscript as follows:

“By combining equations (1) and (2) to yield

$$\beta_X = \frac{1}{\Delta[CO_2]}\left(\Delta C_X^{BGC} - \gamma_X \Delta T^{BGC}\right) \qquad (3)$$

it can be seen that, in order to calculate β_X, the carbon stock changes in the biogeochemically coupled simulation are corrected for global mean temperature changes using γ_X. Hence, temperature changes in the biogechemically coupled simulation are fully accounted for as long as the underlying assumption of linearity holds. However, this assumption might be problematic, for example, if the spatial pattern of warming in a biogeochemically coupled scenario simulation arising from non-CO_2 forcings is very different from the warming patterns in the fully coupled simulation, particularly if the sign of the local temperature change is different from the global average (e.g., local cooling vs. global average warming). Such effects could become important on regional to local scales and will be discussed in Section 3.4."

We do see effects of non-CO_2 forcing in the regional β- and γ-values (Figs. 9 and 10), most notably negative β-values in high latitudes in some models that are not found in the 1pctCO_2 simulations (NorESM2-LM and UKESM1-0-LL, and to a lesser extent CanESM5). We attribute this difference to the very different spatial pattern of temperature changes in some models in the ssp534-over compared to the 1pctCO_2 simulation (see Fig. S9 below, which will be added to the supplementary; note the figure shows normalized temperature changes $\Delta T/\overline{\Delta T}$). In NorESM2-LM, UKESM1-0-LL, and CNRM-ESM2-1, the ssp534-over BGC simulation shows local cooling, which is only marginally present in the fully coupled simulation. This (together with other changes in local climate) can lead to local carbon losses and negative β-values. In NorESM2-LM and UKESM1-0-LL, these negative values are then reinforced by positive γ-values in this region and positive global mean temperature change via equation 3. This indicates that, in the case of non-CO_2 forcings (particularly aerosol forcing, which is regionally fragmented) the global mean temperature change is not a good proxy for regional climate changes.

We will revise and expand the discussion on the effects of non-CO_2 forcing in Section 3.4 (from line 682 of the preprint and after line 732). Please see our detailed response to the corresponding specific comment below.

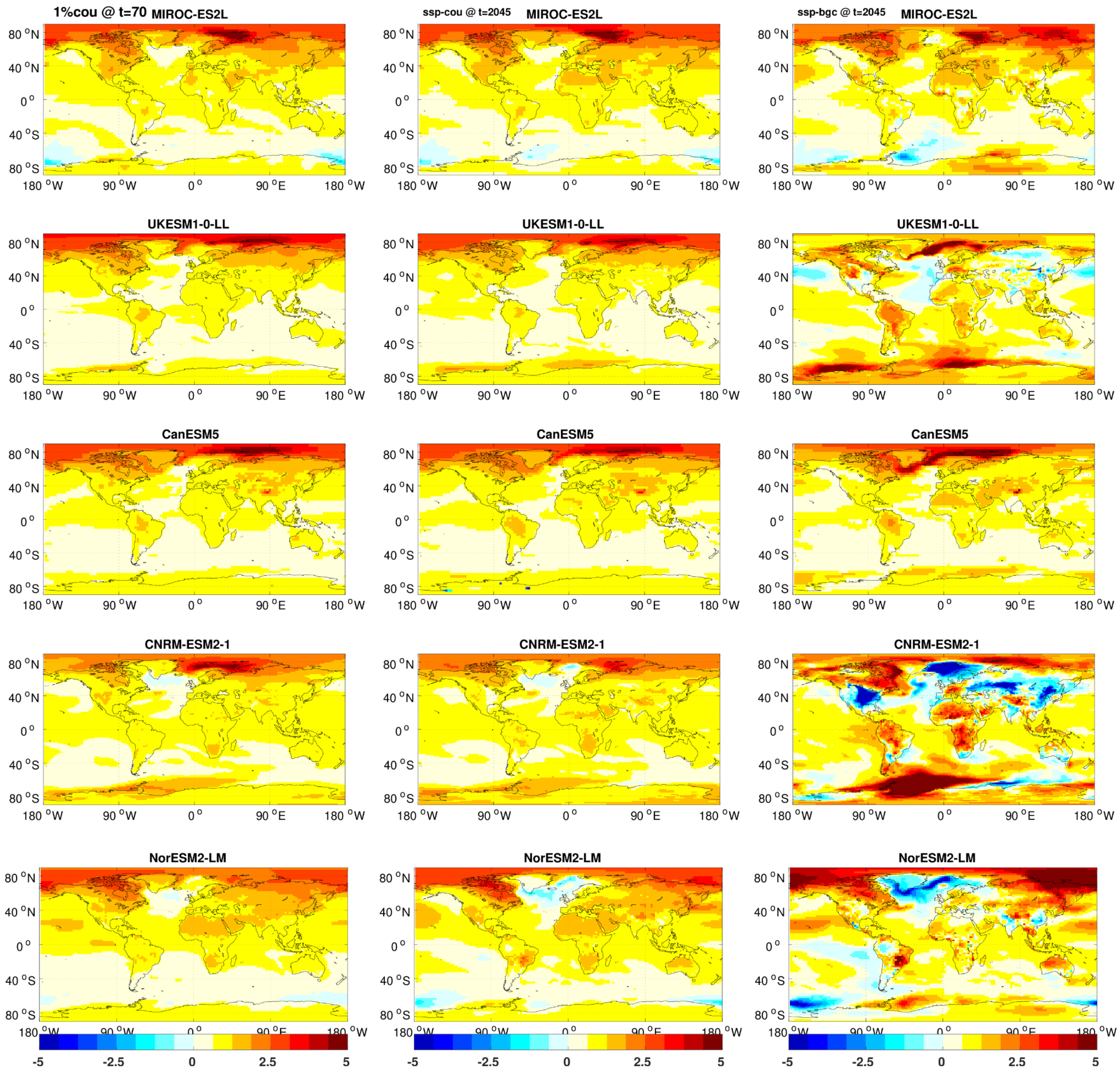

Figure S9: ΔT normalized by its global mean value for individual models. Temperature deviations are averaged over 21-year time intervals centered on the year 70 for the fully coupled $1pctCO_2$ experiment and the year 2045 both for the fully and biogeochemically coupled versions of the ssp534-over scenario. The BGC version of the ssp534-over simulation represents non-CO_2 induced radiative forcing, along with the effects of land-use changes. The fully coupled $1pctCO_2$ represents only CO_2 induced warming.

- *Hysteresis: The manuscript describes in the hysteresis in the integrated land and ocean carbon flux response to changes in CO2 and temperature, but misses to provide an explanation for why this hysteresis may occur. Identification of possible causes (e.g. lagged response to forcing) could help to explain some effects described in the paper (see specific comments).*

Thank you for this suggestion, we have revised our manuscript accordingly and discuss possible causes of hysteresis in more detail. Please see our detailed responses to the specific comments below.

- *Calculation of feedback metrics: The authors chose to calculate the feedback metrics during the negative emissions phase using the same reference year (pre-industrial) as for the positive emissions phase. This leads to feedback metrics being ill-defined as the pre-industrial state is approached in the ramp-down phase of the 1pctco2 simulation. An alternative approach has been proposed (Chimuka et al., 2023) that uses the time of transition from positive to net negative emissions as the reference year. The advantage of such an approach is that it quantifies carbon cycle feedbacks specifically under conditions of declining atmospheric and cooling, which is consistent with the stated objective of the manuscript (l. 134-145). These alternative approaches should at least be acknowledged and discussed.*

We agree that this interesting new approach has to be acknowledged and discussed in our paper. We have introduced a new paragraph in the introduction (after line 101 of the preprint manuscript), which reads as follows: "One open question regarding carbon cycle feedbacks under negative emissions is relative to which state the feedbacks should be measured. A sensible definition requires that any gain or loss of carbon is calculated relative to a state where the carbon cycle is in equilibrium. Schwinger and Tjiputra (2018) have opted to keep the pre-industrial state as the reference also after the onset of negative emissions. We follow this approach here, but we note that recently Chimuka et al. (2023) proposed an alternative approach, which defines the feedbacks during the negative emission phase relative to the state at the onset of negative emissions. Since, the Earth system will be in disequilibrium at this point in time, this approach requires additional simulations (e.g., 500 years of zero-emission simulation initialized from the peak of [CO2] at the beginning of the ramp-down phase) that allow to estimate and remove the lagged response of the Earth system to this disequilibrium."

- *Description of model results lacks precision in some instances (see specific comments).*

Thank you for your feedback. We improve the description of our results, addressing the specific comments you provided.

Specific comments

l 19: The goal of the Paris Agreement is to limit warming to "well below 2°C" above pre-industrial levels.

We have replaced "Limiting global warming to 1.5°C…" by "Limiting global warming to well below 2°C…"

l 45, "exhausted within the next few decades". Decades -> years. Include reference to updated carbon budget estimates in Forster et al., 2023.

We have revised "decades" to "years" and included a reference to the updated carbon budget estimates in Forster et al. (2023).

l 50-51: Include more recent references, e.g. IPCC Synthesis Report, State of CDR report (Smith et al., 2023).

More recent references are included, including the IPCC Synthesis Report and the State of CDR report (Smith et al., 2023) and Forster et al. (2023).

l 118: A recently published study by Chimuka et al. explores land carbon cycle feedbacks under negative emissions. Please include reference.

The reference has been included now. Thank you! See also our response above.

l 138-139: "We briefly explore … the impact of alternative metrics". The need for alternative metrics in mentioned in the Conclusions, but the paper does not include an exploration of these metrics. Please rephrase.

Here we referred to the instantaneous flux-based approach described in Section 2.1, as presented by Boer and Arora (2009). The text has been revised as follows: "We also briefly …the impact of alternative feedback metric definitions that rely on instantaneous carbon fluxes rather than carbon stocks in the context of negative emissions ."

l 173-174: Please clarify whether the assumption DT_BGC=0 was used in the calculation of feedback metrics for 1pctCO2 and SSP5-3.4-OS simulations; for the latter this assumption is not justified due to non-CO2 forcings applied in the BGC simulation.

We did not use the assumption DT_BGC=0. All feedback factors (global and regional) were calculated using the full expression for gamma and beta. Only for comparison and to assess what impact this assumption would have, results based on the assumption of DT_BGC=0 are shown as dotted curves in Fig. 5d and Fig. 8. The impact of this assumption was relatively small on global average. We clarify this by adding the sentence: "All results presented here are calculated using the complete expression for β and γ (without the assumption $\Delta T^{BGC} = 0$). For comparison, we also provide feedback factors calculated using the simplified (rightmost) definition of β and γ in some figures."

l 306: "smaller magnitude of the temperature anomaly". I think this should read "larger magnitude".

Corrected, thank you!

l 308-309; "suggests that a substantial part of the carbon-climate feedback…". Unclear how you reach this conclusion. Please explain.

This is based on the magnitude of temperature changes in the BGC simulation: For 4 out of five models, the peak warming in the BGC simulation reaches about 10-30% of the peak warming in the fully coupled simulation. Assuming that land use change is not a strong driver of global average (non-local) temperature changes, we conclude that the non-CO_2 forcings contribute "substantially" to the temperature changes, and thus will cause a substantial part of the carbon-climate feedback. We agree that we could make our statement more quantitative: "The relatively large magnitude of the temperature anomaly in the BGC simulation of the ssp534-over scenario (peak warming of 12% - 29% of the peak warming in the fully coupled simulation) suggests that warming due to non-CO_2 forcings might contribute substantially to the carbon-climate feedback in the ssp534-over scenario."

l 324-325: "the terrestrial CO2 source … is larger….": Models with a larger terrestrial sink have a larger source in the ramp-down phase of the BGC simulation. This suggests that these models have a larger sensitivity (DC_L/DCO2) to both atmospheric CO2 increase and decrease.

Thank you, we have added this as follows: “We also observe (Fig. 2c,d) that models which take up more (less) terrestrial carbon during the CO_2 ramp-up phase (1pctCO_2) release more (less) carbon towards the end of the CO_2 ramp-down phase (1pctCO_2-cdr-bgc), indicating that these models have a larger (smaller) sensitivity ($\Delta C_L/\Delta CO_2$) to both atmospheric CO_2 increase and decrease.”

l 369: Which “simulations”?

We rephrased the text as “At the end of the ssp534-over and 1pctCO_2 simulations,... ”

l 372 (and elsewhere in sections 3.2.1 and 3.2.): “the ocean carbon-concentration feedback is larger…”. Need to explain how the magnitude of feedbacks is inferred. I assume you are using the slope but his needs to be clarified.

Thank you, we have clarified this by adding: “Generally, the ocean carbon-concentration feedback (as indicated by the cumulative carbon uptake per unit increase of CO_2 concentration, Fig. 3a-c) is larger in the ssp534-over scenario, which can most likely be explained with the slower growth rate of [CO_2] in this scenario compared to the 1pctCO_2 simulation.” We add similar text in line 409 (section 3.2.2).

l 408: It is worth pointing out that, in contrast to the ocean, the integrated atmosphere-land flux starts to increase, albeit with a lag, in response to cooling in the negative emissions phase in most models.

Thank you for your suggestion. We have incorporated your input into the text as follows: “It is worth mentioning that, unlike the ocean, the COU-BGC accumulated atmosphere-land flux starts to increase, albeit with a lag, in response to cooling during the negative emissions phase in most models (Figs. 3e and 4e).”

l 411-413: “This is because…”. This needs to be explained and justified more clearly. Are you saying that because “cropland grid cells” have a smaller cumulative flux in the SSP-3.4-OS simulations, this can also be expected for grid cells with a cropland fraction <25%?

Yes, this is what we intended to say. Within our “grid cells dominated by natural land”, we can still have up to 25% crop fraction, and this fraction of the grid cells can be expected to behave similar to crop dominated grid cells. We have revised the text as follows: “...is the driver behind the small (negative for NorESM2-LM and UKESM1-0-LL) carbon accumulation for crop land grid cells. Since grid cells that are dominated by natural land according to our separation approach, may contain up to 25% croplands, we expect a reduction of cumulative carbon fluxes due the remaining land use (changes) in the natural land grid cells.”

l 415: “driver”: How about the role of non-CO2 forcings in SSP5-3.4-OS?

We see a very pronounced difference between grid cells with >=25% crop fraction and <25% crop fraction. When viewed spatially, β becomes strongly negative in agricultural areas (not shown, we have masked “crop dominated” grid cells out in Fig. 9). Although we cannot rule out some influence of non-CO_2 forcers (particularly aerosols, see also our response above), we believe that in this context the mentioning of non-CO_2 forcings would be too speculative.

l 424: "remains very similar": Several models show significant differences (MIROC, CanESM, UKESM).

We apologize, the text was somewhat imprecise. We reworded the sentence as "... the model-mean carbon-climate feedback for cropland and natural land remains very similar between the ssp534-over and 1pctCO_2 simulations (Fig. S3f)."

l 439-442: It would be helpful if the authors could point to potential causes for the hysteresis, such as lagged response to forcing and/or tipping points/state changes. This could also help with the interpretation of results. E.g. if the larger concentration-carbon feedback in some models is dominated by tree-PFTs (which appears to be the case based on the statement in l. 658-659), the longer response timescale of these PFTs could explain why models with a larger carbon-concentration feedback also have larger hysteresis.

We have added a paragraph after line 448 of the preprint, summarizing the main causes for hysteresis as follows: "For the ocean carbon cycle, hysteresis in the carbon-concentration feedback occurs mainly due to the long time scales of ocean overturning circulation. Schwinger and Tjiputra (2018) have shown that hysteresis strongly increases with water mass age. Young waters, which reside close to the ocean surface, exchange quickly with the atmosphere and show little hysteresis, whereas old, deep ocean water masses can only respond to the declining atmospheric CO_2 when they are re-ventilated to the surface layer, which can take hundreds to thousands of years, and thus show considerable hysteresis. Over land, both the vegetation and soil carbon pools show a lagged response to decreasing CO_2 due to the fact that transient changes in [CO_2] lead to a long term disequilibrium between the CO_2 fertilization effect, vegetation biomass, litterfall, and soil carbon (e.g., Krause et al. 2020). Therefore, despite declining [CO_2] levels at the beginning of the ramp-down phase there is still an increase in vegetation biomass due to CO_2 fertilization, and consequently an increase in soil carbon due to still increasing litterfall. Warming-induced hysteresis appears to be larger for soil carbon in most models. Similar to the large warming induced hysteresis in the ocean, this is caused by the fact that even though warming levels start to decline shortly after the onset of the ramp-down phase, environmental conditions are warmer than in the pre-industrial period over the whole time of the ramp-down simulation."

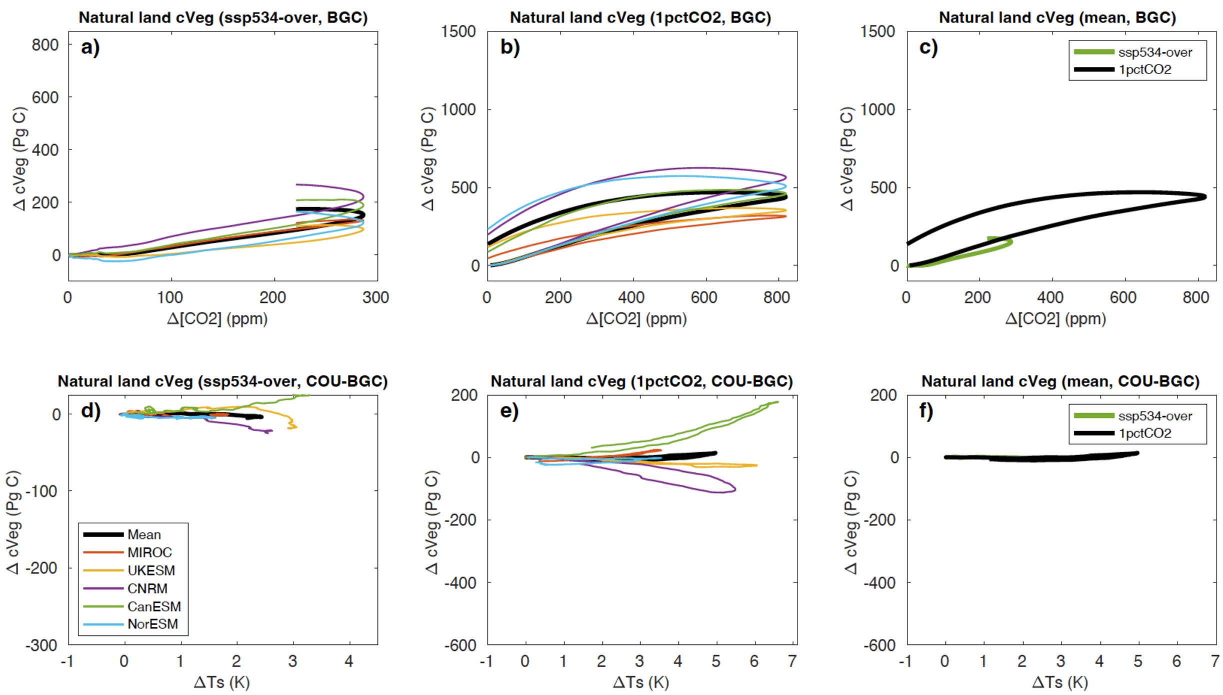

Figure AC1: same as Fig. 4 in the preprint manuscript but for vegetation carbon cycle feedbacks.

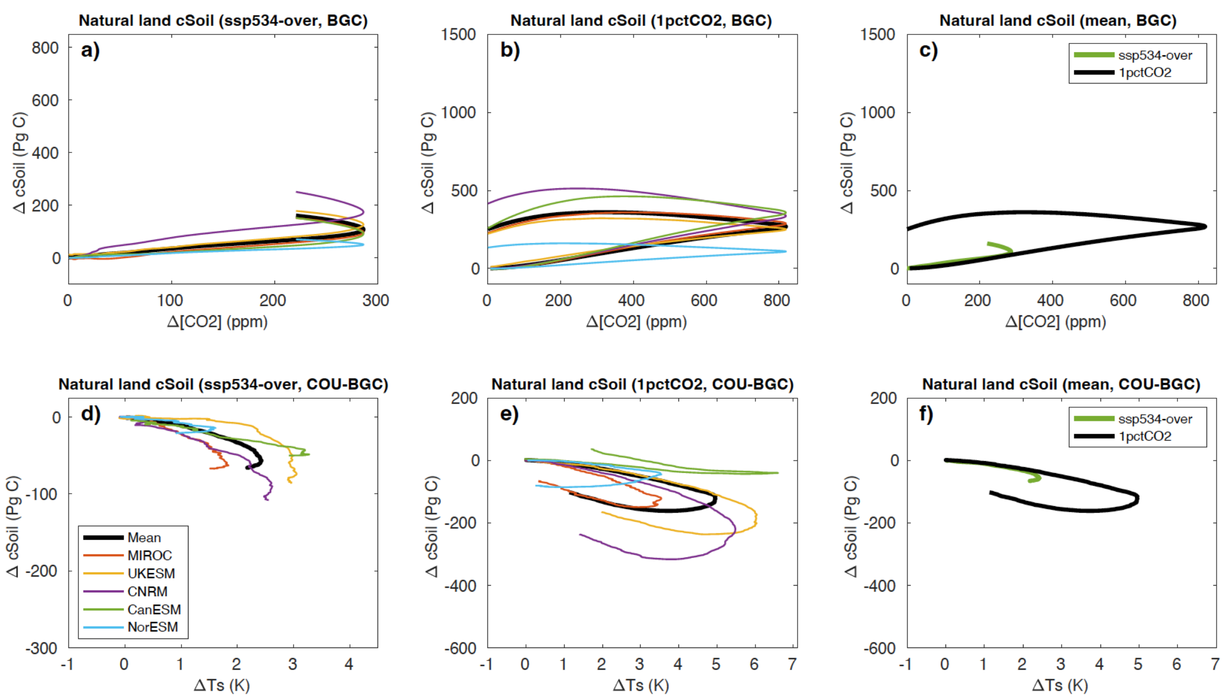

Figure AC2: same as Fig. 4 in the preprint manuscript but for soil carbon cycle feedbacks.

l 509-510: “This implies …”: The fact that feedback metrics as calculated in this study become ill-defined at the end of the 1pctCO2 simulation is not a problem of the metrics themselves, but the choice of *reference year used for the calculation of the anomalies in the ramp-down phase. An alternative that addresses this problem is to use the transition year from positive to negative emissions as reference year (see Chimuka et al., 2023).*

Thank you for pointing this out, we mention this as follows: "We note that this problem is connected to the choice of the reference relative to which the feedbacks are calculated. In the approach of Chimuka et al. (2023), where the reference is chosen to be at the transition from positive to negative emissions, singularities towards the end of the 1pctCO_2-cdr simulation are avoided."

l 516-517: To interpret this increase in model uncertainty it would be valuable if the authors could address the additional processes that become relevant in ramp-down phase. E.g. the uncertainties would be expected to increase if models exhibits different lag times in response to the prior CO2 increase.

A bit further down, lines 573-576 of the preprint text, we already discuss reasons for the increased uncertainty in β_L: "It is worth noting that for four out of six terms of Eq. 3 … the model disagreement is significantly larger during the ramp-down phase of the 1pctCO_2 simulation, indicating that changes in these processes are responsible for the strong increase in model uncertainty in β_L between positive and negative emission phases pointed out in the previous section". We believe it would be confusing to add additional explanations around line 516 and would prefer to leave the text as it is.

l 654.: "consistent with the lagged response": this is the first time a lagged response is mentioned. The possibility of such responses should be discussed earlier in the context of hysteresis.

We have revised our manuscript to mention the "lagged response" earlier in Section 3.2.3 on hysteresis, please see our response above.

l 683-685: This inference is incorrect. Calculating the metrics assuming DT_BGC=0 changes the value of the feedback parameter, but does not remove the confounding effect of non-CO2 induced warming on beta.

We see that this statement was not well explained. We agree, there is an effect of non-CO_2 forcings on the carbon uptake ΔC_X^{BGC}, regardless of how we calculate the feedback metrics. However, β_X is corrected for the global average warming that might occur in the BGC simulation by using γ_X: $\beta_X = \frac{1}{\Delta[CO_2]}\left(\Delta C_X^{BGC} - \gamma_X \Delta T^{BGC}\right)$ (Equation 3 in the revised manuscript, see also our response above). Therefore, negative β_X values could occur even if ΔC_X^{BGC} was positive. We calculated β_X with the assumption $\Delta T^{BGC} = 0$ to check whether the carbon uptake is actually negative (which is the case).

We have revised and expanded the discussion of the negative β_X values (and differences in γ_X, which are related) in the ssp534-over scenario and their relation to non-CO_2 forcings: "Unlike in the 1pctCO_2 experiment, temperature changes are not negligible in the BGC simulation of the ssp534-over experiment (Fig. 1). Furthermore, the spatial pattern of temperature changes is very different for some models, particularly for UKESM1-0-LL, NorESM2-LM, and CNRM-ESM2-1, which show local cooling that is not present (or much weaker) in the fully coupled simulations (Fig. S9). This cooling (and other changes in surface climate related to non-CO_2 forcings) lead to local carbon losses and negative β-values in

UKESM1-0-LL and NorESM2-LM in northern high latitudes. In addition, according to Eq. 3, these negative values are reinforced by positive γ-values in this region and a positive global mean temperature change in ssp534-over in these models (see Eq. 3). In contrast, CNRM-ESM2-1 does not show negative values of β in northern high latitudes (despite local cooling), which can be explained by much larger β-values to begin with, and a smaller (and negative) temperature sensitivity γ in high latitudes."

Further down (line 734) we have added: "These differences are related to the negative β-values (discussed above) for these models, which make the carbon gain due to warming (the difference $\Delta C^{cou} - \Delta C^{bgc}$) considerably larger than in the 1pctCO$_2$ simulation. Again, this is reinforced by the fact that the global average temperature change in the ssp534-over simulation is positive and thus ($\Delta T^{cou} - \Delta T^{bgc}$) is smaller than the actual (local) temperature differences. This indicates that, if the global mean temperature change due to non-CO$_2$ forcings does not broadly reflect local changes correctly (e.g., local cooling vs. global warming), regional scale feedback factors might show unexpected results."

l 685-687: It would be helpful if cropland grid cells that were omitted in the global feedback metric calculations could be clearly identified in the maps in Figs. 9 and 10 for both the 1pctco2 and SSp5-3.4-OS simulations (e.g. by a contour line delineating these grid cells).

Thank you for your suggestion. We have used gray color for the (masked out) cropland areas to make them distinguishable from other areas with small changes in the figure.

l 700-701: "predominantly negative value of gamma_o": by closely looking at the maps it looks like gamma_o exhibits a banded pattern of positive and negative values.

Thank you, we reworded our sentence as follows: " Figure 10 indicates that the ESMs considered here simulate predominantly negative values of γ_{O} over the ocean. Positive values of γ_{O} are found in the Arctic, and in the Southern Ocean most models simulate a banded pattern of positive (adjacent to Antarctica), negative (centered between 60 and 50°S), and positive (between approximately 50 and 40°S) values."

l 756-757: "Hysteresis is stronger relative..". Sentence unclear. Please rephrase.

We reworded this sentences as "At the same level of atmospheric CO$_2$ concentration, the ocean exhibits a larger hysteresis than its corresponding feedback during the ramp-up phase, while the terrestrial carbon uptake displays a larger hysteresis in absolute magnitudes."

l 774-775: The singularity of beta and gamma at the end of the 1pctco2 simulation is not a problem of the experimental design but the choice of reference year.

We have mentioned the alternative approach of Chimuka et al. in the revised version of our summary and conclusions.

l 782-783: Unclear what is meant by "relative strength of the feedback".

We have deleted this sentence in the revised version of the summary and conclusions, which was shortened in response to a comment of reviewer #1.

l 789-790: This "additional component of uncertainty" could be the different response timescales exhibited by the models in response to prior forcing. See earlier comment.

Thank you for your suggestion. We have incorporated the following statement into the text: "This additional component of model uncertainty can be attributed to the varying response timescales of individual models in response to the preceding forcing."

l 799: "strong negative feedback": Unclear which feedback you are referring to.

Thank you for bringing this to our attention. We concur that selecting precise wording is crucial; although the feedback itself is positive, the gamma values are indeed negative. We have adjusted the text as follows: "strong positive feedback (i.e., negative γ) ".

l 805: Given these complications as well as the complications arising due to inclusion of non-CO2 forcing, what is the value of including these simulations in the feedback analysis?

The value is mainly in describing these complications. Now that the next phase of C4MIP is being discussed, we believe it is valuable to highlight the complications in analysing scenarios with land-use change and non-CO_2 forcings. If such biogeochemically coupled simulations of SSP scenarios were to be included in future C4MIP phases one might need to request additional model output or request additional simulations.

l 825: "these metrics become difficult to interpret": discuss alternative approaches proposed in Chimuka et al., 2023.

We have included a discussion of the Chimuka et al. approach in the revised version of our summary and conclusions.

References

Boer, G. J., and V. Arora, 2009: Temperature and concentration feedbacks in the carbon cycle. Geophys. Res. Lett., 36, https://doi.org/10.1029/2008GL036220.

Chimuka, V. R., Nzotungicimpaye, C.-M., and Zickfeld, K.: Quantifying land carbon cycle feedbacks under negative CO2 emissions, Biogeosciences, 20, 2283–2299, https://doi.org/10.5194/bg-20-2283-2023, 2023.

Schwinger, J., and J. Tjiputra, 2018: Ocean Carbon Cycle Feedbacks Under Negative Emissions. Geophys. Res. Lett., 45, 5062–5070, https://doi.org/10.1029/2018GL077790.