Authors' response to the reviewer #1

We thank reviewer #1 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.

The authors explore how land and ocean carbon sinks react to rising and falling atmospheric CO2 levels and associated temperature changes in five Earth System Models. They examine both a stylized scenario and a more realistic overshoot scenario, finding a large hysteresis in the sink response and an ill-definement of the metrics when atmospheric CO2 approaches its initial level.

General comments

Overall, the paper is well-written and involves a large amount of work. I believe it is suitable for publication in Biogeosciences after some revisions.

Thank you for the positive evaluation.

Here are some suggestions to improve the manuscript:

- *I am sceptical on how the authors tried to remove the effects from land-use change. Firstly, instead of removing gridcells with cropland fractions above a specific threshold (thereby removing gridcells with stable land cover over time), wouldn't it make more sense to remove gridcells with large changes in cropland fraction? Secondly, it seems the authors only considered croplands but not grazing land. Depending on how the ESMs implemented transitions between natural land and grazing land, this might cause substantial land-use emissions or carbon uptake (from forest regrowth on abandoned grazing land) in some regions. Thirdly, it would be good to compare your calculated metrics for the selected gridcells to all gridcells in the idealized simulations to see whether the selected gridcells are indeed representative for the entire globe.*

We agree that land-use changes (LUC) are a complication in the context of carbon cycle feedbacks, particularly since only limited LUC related ESM output has been made available for CMIP6. There have been two excellent studies published prior to ours (Melnikova et al. 2021, 2022) that discuss land use changes and carbon cycle feedbacks in the ssp534-over scenario in depth.

Our intention with including ssp534-over in our study was to compare the carbon cycle feedbacks on natural land only. To achieve this, we have to choose some threshold for crop-fraction below which we consider a gridcell as "dominated by natural land". Although the selection of the 25% threshold is arbitrary, it has been chosen following a series of sensitivity analyses. It serves as a representation of the "maximum" cropland fraction spanning from 2015 to 2100. To clarify, our methodology already includes gridcells with substantial shifts in cropland fraction. Conversely, those with changes resulting in a fraction consistently below 25% are regarded as minor changes (i.e., stable land cover over time). By taking *changes* in crop fraction over time as a criterion, we would have kept grid cells with large (> 25% in 1850) but stable crop fraction (which are discarded in our approach). However, the number of grid cells and associated surface area where this is the case is small, such that our results do not depend on this choice.

In response to your second point, we acknowledge that we do not treat pasture grid cells (or transitions from other land use types to pasture) explicitly in our approach, and that this was not discussed appropriately in our manuscript. In ssp534-over, the strong expansion of bioenergy crops after 2040 is assumed to replace pasture (to avoid carbon emissions from deforestation). After the expansion of bioenergy crops into pasture land is completed around 2070, pasture area remains very stable in this scenario (see O'Neill et al. 2016, Fig. 4). Therefore, the majority of grid cells with transitions of pasture to cropland will be captured by our approach, while other transitions involving pasture are small in the ssp534-over scenario. We therefore believe that our approach that neglects treating pasture grid cells separately is defensible, but we will discuss this better in a revised version of our manuscript, added to the end of Section 2.2, as follows: "We acknowledge that our approach does not explicitly address pasture gridcells or transition from other land use types to pasture. Nonetheless, in the ssp534-over scenario, a substantial expansion of bioenergy crops between 2040 and 2070 is assumed to replace pasture areas, remaining relatively stable thereafter (see O'Neill et al. 2016). Hence, our approach captures the majority of such gridcells with transitions from pasture to cropland."

Regarding your third point, the metric (cumulative carbon uptake) used here, is of course sensitive to the total area. The selected gridcells therefore show a somewhat smaller carbon accumulation than the "entire globe". Nevertheless, regarding the overall shape and relative model differences the Figure AC1 below suggests that the selected gridcells indeed are representative of the entire globe (except for the CanESM5 model that appears to behave as an outlier in cropland areas as already mentioned in the text).

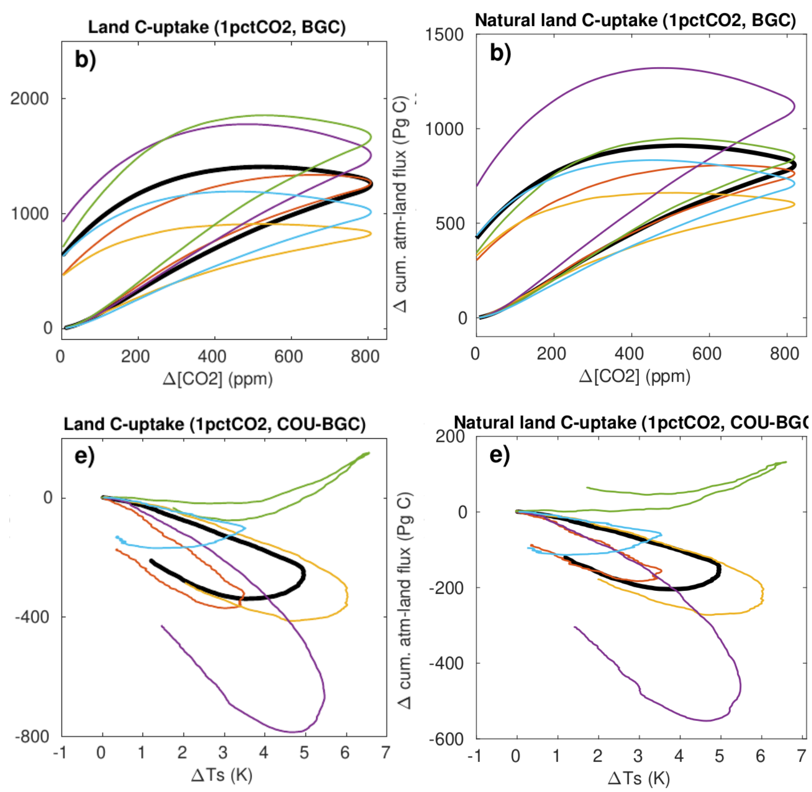

Figure AC1: Terrestrial carbon cycle feedbacks in the 1pctCO_2 and 1pctCO_2-cdr simulations for the entire globe (left column) and natural land (right column). An 11-year moving average has been used in all panels. Color coding is the same as Fig. 4 in the manuscript.

- *The authors find a large hysteresis in the sink response to declining atmospheric CO2 levels. This is likely a result of sinks responding not only to decreasing CO2 but are also still affected by the* *previous CO2 increase (e.g. Chen et al., 2019; Chimuka et al., 2023; Krause et al., 2020). It would be good to discuss (and if feasible investigate) this more in the paper.*

We agree that the carbon cycle responses will be influenced by the effects of prior CO_2 increases. Also in response to comments by reviewer #2, we have expanded Section 3.2.3 by a discussion of the main causes for the 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' responses to declining atmospheric CO_2 will be delayed, 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 remain warmer than in the pre-industrial period over the whole time of the ramp-down simulation."

Specific comments

L19: I wonder whether the 1.5° target is still realistically feasible, I suggest to change "1.5" to "well below 2°"

This has been done.

L31: Does this mean the growth is the same everywhere or that there are spatial variations but all models show the same patterns?

It is intended to convey that although there are spatial variations, all models still exhibit the same general pattern. We reworded the text as "We find that this growth over time occurs such that the spatial patterns of feedbacks do not change significantly for individual models."

L107: I thought the Arctic is warming at least 3x faster, please double-check. Also the references seem to be model-based.

We have added other references and reworded the text as "... that Arctic temperatures are increasing at a much faster rate than the global average (Liang et al. 2022; Rantanen et al. 2022) …"

L118ff: Include Chimuka et al. (2023). Also it's not entirely clear to me how the study goes beyond those previous studies (e.g. SSP535 is also used in Melnikova et al.). Can you elaborate a bit more?

We have added a reference to Chimuka et al. (2023). We have also included a discussion of the work by Chimuka et al. (2023) in several places in the revised manuscript following other comments.

Our study is different from Melnikova et al. (2021, 2022) because we focus on the 1pctCO_2 and 1pctCO_2-cdr simulations. A biogeochemically coupled 1pctCO_2-cdr simulation was not part of CMIP6 and has been performed by 5 ESMs for this study. Therefore, we are able to provide an analysis of CO_2 and temperature induced feedbacks during the ramp-down phase of the 1pctCO_2-cdr simulation for the first time. We consider the ssp534-over scenario only to compare feedbacks on the fraction that is not affected by land-use changes with the 1pctCO_2 scenario.

L169: "gases"

Corrected.

L239: Maximum for the average over the whole time period or maximum of individual years?

The original crop fraction data for individual models is available in monthly timesteps. The maximum is calculated over the entire time series. In other words, we determine the highest value from the original crop fraction data for individual months over the period 2015-2100. To make this clearer we rewrite this sentence as follows: "... we define natural land as grid cells with a maximum cropland fraction of less than 25% at all time steps during the period 2015-2100."

L239: "cropland"

Corrected.

L240: remove comma

Done!

L254: This seems a major limitation, I assume models without dynamic vegetation to have a lower hysteresis. In general, in addition to calculating the different terms (Fig. 7), can you say more on which differences in modelled processes you think drive the differences in the carbon response to decreasing CO2?

We agree that the inclusion of dynamic vegetation should lead to a larger hysteresis of land carbon pools during an overshoot. However, results from our small model ensemble remain inconclusive, since it seems that model uncertainty stemming from a variety of processes is larger than this effect. This is highlighted in Fig. 4 which demonstrates that although UKESM1-0-LL has a strong hysteresis of the carbon-climate feedback, CanESM5 (without dynamic vegetation) has an even larger carbon-climate feedback.

Also regarding other differences in modelled processes, the results of our calculations presented in Fig. 7 are less conclusive than we had hoped for. For example, the three models that include a representation of the nitrogen cycle ("nitrogen limitation", MIROC-ES2L, NorESM2-LM, UKESM1-0-LL) do not show generally a lower CO_2 fertilization effect.

L256: Remove comma.

Done!

L268: Another comma I think should be removed.

Done!

Fig. 1: Please use consistent names (not only here but throughout the manuscript – there is a mixture e.g. of "SSP5-3.4-OS", "ssp534-os" and "ssp534-over"), avoid too many abbreviations ("ROC"), use subscripts for CO2. "1pctCO2" is instead a combination of "1pctCO2" and "1pctCO2-cdr", I don't think this has been made clear. Maybe term this combination "1pctCO2-1pctCO2cdr" (or simply "1pctCO2-cdr" as it is an extension of "1pctCO2") and use this name throughout the manuscript?

To ensure clarity and consistency, we have now used the naming "ssp543-over" throughout the manuscript, used subscripts for "CO2", and removed the "ROC" abbreviation from Fig. 1. Furthermore, we clarified the namings associated with the 1pctCO_2 experiment by adding "Unless otherwise noted, for the sake of simplicity, we will use "1pctCO_2-cdr" to refer to both positive and negative phases of this experiment (1pctCO_2 plus 1pctCO_2-cdr) to the text on lines 236-238 of the revised manuscript.

L326: "(Fig. 2c,d)" should be moved to end of the sentence.

Will do, thank you!

L329ff: Not sure I understand this interpretation, it sounds as if the carbon does not need to be released without climate warming. I think what you mean instead is that more initial carbon uptake happens in the BGC simulation and this extra carbon is then released? Also I think warming reduces the land carbon sink in SSP5-3.4-OS rather than increasing the source?

Sorry, these sentences were not very clear. We reformulated them as "We therefore interpret the increased source of carbon at the end of the 1pctCO_2-BGC simulation as a release of additional carbon that has been taken up in the absence of climate warming during the biogeochemically coupled simulation. The net negative emission phase of the ssp534-over scenario is too short to show this effect in 2100 (where the warming effect still reduces the model mean terrestrial carbon sink)."

L332: Remove second comma.

Done!

L414ff: I find this unclear. Is the mechanism that "cropland grid cells" in this scenario tend to keep/increase their cropland fraction until the end of the century so they don't benefit much from CO2 fertilization?

Yes, this is correct. We make this point clearer by rewording this sentence as follows: "This indicates, consistent with Melnikova et al. (2022) who demonstrate that carbon losses from land use changes dominate over gains through CO_2 fertilization in crop dominated areas (see their Fig. 4, panels a and c), that the prescribed …"

L416: I understand what you mean but one could argue land-use change is affected by atmospheric CO2. Change e.g. to "We note that land use change is an input rather than a feedback process in our simulations."

We reworded this sentence as suggested: "We note that land use change is externally prescribed rather than a feedback process in our simulations."

L431: I am not sure I understood how the numbers were calculated. Is it the cumulative carbon flux between year 70 and 210 (so without hysteresis and perfect reversibility it would be 0)?

Yes, this is correct, our definition of hysteresis is the difference between the two points on the ramp-up and ramp-down pathways in individual models intersected by a vertical line at 570 ppm. We see that we were not very clear on the definition of hysteresis, and we added the following text: "...or returned to this value after the overshoot. We define hysteresis as the difference between cumulative carbon uptake in year 210 minus cumulative carbon uptake in year 70 (i.e., hysteresis is positive, if cumulative carbon uptake is *larger* on the ramp-down side of the 1pctCO2 simulation)."

Fig. 5,8: Are b) and e) really needed?

We believe that panels b and e with smaller scales on the y-axis are useful since they facilitate the comparison of results between the SSP-scenario and the 1pctCO_2 simulation as well as with previous studies that only calculated feedback factors for the ramp-up phase.

Fig. 7c: ".ppm" should be "/ppm". Can you also add a legend?

The dot is removed. A legend has been added to the figure.

Fig. 8: Change "fdbk" to "feedback".

"fdbk" has been updated to "feedback", thank you!

L628: But other carbon losses would likely also increase for a higher TCR, so the contribution might be the same?

Thank you, this is a good point. Other models involved in this study, which are characterized by higher TCR (e.g., UKESM1-0-LL and CanESM5) cannot resolve permafrost carbon, so unfortunately we cannot compare their permafrost carbon losses versus other carbon losses due to warming. It might well be that the percentage contribution of permafrost losses to the total feedback remains similar also for higher TCRE models, but this is highly speculative. Since we are not able to provide any evidence for or against this hypothesis, we would prefer to leave this sentence as it is.

L629: Typo NortESM2-LM.

Thank you, corrected.

Fig. 9: The white areas (croplands) in the right figure are hardly distinguishable from areas with small changes. Consider using grey colour instead.

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

L686: These grid cells seem pretty far north to be used for croplands. Can you checker whether there is indeed some cropland expansion happening there? Could it also be related to wood harvest which may only be represented in UKESM and CanESM?

Thank you, we have double checked this and these latitudes are indeed too far north for cropland. Wood harvest is only represented in NorEMS2 and MIROC-ES2L (not in CanESM5 and UKESM1-0-LL). We have investigated the high latitude negative β-values further and attributed this effect to the warming pattern caused by non-CO_2 forcing. We have rewritten the text following line 682 as follows: "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."

L733: "in the SSP5-3.4-OS scenario". Do you have an idea why the uptake does not occur in 1pctCO2?

This difference in high latitude γ-values in NorESM2-LM and UKESM1-0-LL is directly related to the negative β-values in the same region. Since there is carbon loss in the ssp534-over simulation in these models, the carbon gain due to warming (the difference $\Delta C^{cou} - \Delta C^{bgc}$) becomes larger than in the 1pctCO_2 simulation.

L743: This section is very long for a summary and conclusion section. Consider shortening.

Thank you for your feedback. We will work on shortening this section.

L799: Is this really a negative feedback or rather a lag in a positive feedback?

Thank you for bringing this mistake to our attention. We have corrected the text as follows: "strong positive feedback (i.e., negative γ)".

L840: Do you have suggestions for such experiments?

For example, to disentangle the effects of land use change, requesting a biogeochemically coupled simulation with fixed land use could be useful. Such simulations (not biogeochemically coupled though) have been done for CMIP6 LUMIP (Land Use Model Intercomparison Project). We added this suggestion to this sentence "(e.g., scenario simulations with fixed land use)".

L844f: I don't understand, isn't it these interactions that make SSP5-3.4 so difficult to interpret? Why would it not be possible to include e.g. afforestation?

Negative emission options such as afforestation or ocean alkalinization will increase the terrestrial or oceanic carbon sink. At the same time, the additional carbon uptake will have a feedback on the whole carbon cycle. These feedbacks cannot be determined with the "traditional" C4MIP set-up. We make this clearer by extending the sentence: "Consequently, a new framework for determining feedbacks caused by large scale CDR in realistic scenarios of CDR deployment is needed and should be developed…"

References

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