Response to RC1

We are grateful to the reviewer for their positive and encouraging assessment of our manuscript. We are delighted that our study was well received and have carefully considered their comments to improve our work. As an overview, and in response to all reviewers, we will address the following aspects in the revised manuscript:

- Shortening the paper and re-organizing our results: We will make considerable effort to shorten our paper (and focus on the novelties of this study), by identifying content in Sections 3.1.1, 3.1.2, 3.2, 3.3, and 3.4 that does not signficantly contribute to our main points. We will move Table 2 to the appendix. We will remove Fig. E2 as it is not referenced in text, and Fig. 14a as it more or less shows the same as 14b-c. We will make Fig. 2 a single column, similar to Fig. 3, 5, 7, 8 and 9. We will re-organize and re-write certain statements as per the reviewers' comments.
- Enhancing the discussion: We will add a PULSE experiment to Sections 3.2-3.3, attribute weathering fluxes to either temperature or runoff in Section 3.1.2, and expand on the potential caveats in our study (related to erosion limitation and tipping points) in Section 5. We will also further discuss the additional positive climate-carbon cycle feedback caused by CH_4 in Section 4.4.
- Adding additional figures and tables: We will add subplots to Fig. 13 for A_i vs E and τ_i vs E, and a figure for temperature evolution in the intCH4 experiment in the appendix. We will also provide a table of lithological values (e.g., activation energy of silicates for the different lithologies), as well as the fitting parameters for the multi-exponential decay fit of the PULSE experiment in the appendix.
- Clarifying certain aspects: We will clarify the treatment of organic carbon in the revised manuscript. Additionally, we will expand on the weathering scheme by including a table of lithological values for Equations 2 and 3, and we will incorporate the equations for carbonate weathering specific to loess and carbonate sedimentary rocks. Furthermore, we will clarify in our figure captions whether time is counted from the beginning of the pulse, or from maximum CO_2 concentration.

Please find our point-to-point responses to the individual comments given by Reviewer #1 below (reviewer comment in black, our response in blue).

In their paper 'Assessing the lifetime of anthropogenic CO2 and its sensitivity to different carbon cycle processes' Kaufhold et al. apply the CLIMBER-X Earth system model of intermediate complexity to investigate the atmospheric lifetime and the removal processes of cumulative CO2 emission in a set of 100 kyr experiments.
They include a variety of sensitivity experiments to further investigate the role of the landbiosphere and weathering feedbacks in removing atmospheric CO2 perturbations.
Overall, the paper is well written and provides interesting results and nicely addresses the question of the landbiosphere and weathering feedbacks for the removal of atmospheric CO2 perturbations and provides and a wealth of figures. The spatially explicit weathering scheme of CLIMBER-X is an important addition to the investigation! In summary, the study is well suited for publication in Biogeosciences.
I have three more general aspects and a few minor comments the authors may address during revision.

We would like to thank the reviewer for their positive comments on our paper, and will make improvements where highlighted.

General:

1) Presentation of the C-perturbation
In the paper, the authors alternate between presenting the atmospheric C-perturbation (or the perturbation of other reservoirs) in absolute values (i.e. ppm CO2 or PgC) and as fraction of the maximum CO2 perturbation. For comparison with other studies and to address non-linearities it would, in my eyes, be much easier to show results as fractions of the CO2 perturbation rather than as absolute values (for example also in Fig. 1). The fact that the atmospheric CO2 perturbation (in ppm) is larger for larger cumulative emissions is not surprising and it could be interesting to investigate the non-linearities in more detail instead. An alternative could also be to normalize the results by the response to a certain pulse size to highlight the non-linearities (e.g. Fig. 2).
We acknowledge that alternating between absolute values (as in Section 3.1) and fractional values (as in Sections 3.2-3.4) could be confusing, and that expressing some responses as fractions of cumulative emissions may be more interesting for exploring nonlinearities. In principle, we could provide a version of Fig. 1 that shows the change in atmospheric CO_2 as a fraction of cumulative emisions. However, the method used to determine atmospheric CO_2 concentration from other publications (i.e., visual inspection) is not accurate enough, meaning that the figure is semi-qualitative and mostly serves an illustrative purpose.We will provide a statement on this in the figure caption of the revised manuscript. Furthermore, we are cautious about modifying the current presentation of these results in Fig. 2 (either by normalizing it by cumulative emissions, or by a certain pulse size) as it could make it difficult for future comparisons with our work, especially given the ramp up period. We have aimed to remain consistent with how values have been presented in prior studies. For instance, atmospheric CO_2 concentrations have consistently been reported in absolute values (e.g., Archer 1998, Lenton & Britton 2006, Ridgwell & Hargreaves 2007, Archer & Brovkin 2008, Archer et al. 2009, Lord et al. 2015, etc.), and emissions removed or remaining given in such normalized values (e.g., Archer 1997, Eby et al. 2009, Lord et al. 2015, etc.). There are a few exceptions to this, but these are typically made for specific reasons. For instance, Joos et al. (2013) report atmospheric CO_2 only as a fraction of emitted emissions, but their study's objective was to compare the response of different models.

2) how emissions are prescribed
The way emissions are prescribed in this study (as Gaussian function) complicates the comparison with studies featuring a pulse-like release of carbon (as often done) to a certain degree. This leads in this study, for example, in the case of small total cumulative emissions, to a large fraction of the atmospheric CO2 perturbation already having been removed before reaching the maximum atm. CO2 perturbation and also to less timescales required when fitting the response as a sum of exponentials (section 3.3) as compared to studies with an instantaneous pulse-like emission. While this is acknowledged in the text, I think it should be made more clear, especially for the discussion of the timescales in sections 3.2-3.4. Further, it might be interesting to add one additional emission pathway sensitivity experiment, where all the carbon is emitted in the first timestep, as done in many of the studies discussed in the

paper. In light of how fast the CLIMBER-X model is (10'000 years per day), this might be doable.
This is a very good point. We agree that this complicates comparisons to other studies, especially as removing the ramp up of emissions may affect the calculated timescales in Section 3.3. We will make the limitations of our analysis (for the REF experiment) more clear going forward. Originally, we chose to not use a pulse-like CO_2 perturbation due to concerns about model stability. However, following the reviewer's suggestion, we promptly initiated such experiments for a new emissions pathway ensemble called 'PULSE.' Preliminary results indicate that these experiments behave as expected, with atmospheric CO_2 converging to the REF experiment after some time (without any stability issues). At this stage, it is not feasible to revise all results to use the PULSE experiments as the reference. However, assuming no unforeseen issues arise in the PULSE ensemble, we will aim to integrate its results into Sections 3.2–3.3. We also believe this addition will enhance the manuscript by providing greater robustness to the estimated fraction of emissions remaining and timescales.

3) Length of the paper
While the paper does a very nice job in thoroughly describing processes and visualizing a lot in figures, I found it quite lengthy to read. Maybe during revisions this could be kept in mind. For example, in my opinion, sections 3.2-3.4 could be merged and shortened with a focus on the novelties of this study (timescale of the silicate weathering feedback).
We acknowledge that the paper is quite lengthy (as Reviewer #2 also pointed out). We agree that some of the sections mentioned can be shortened and will do so in the revised manuscript. We will also look for other areas that may not significantly contribute to the discussion (e.g., in Section 3.1) as to reduce the overall length. However, we are hesitant to fully merge Sections 3.2–3.4, as we believe maintaining their separation helps preserve structure in the analysis. Furthermore, we believe that our manuscript is stronger when it includes the complete analysis related to determining the atmospheric lifetime of anthropogenic CO_2 and its removal timescales. Considering the additional requests for a PULSE experiment, the attribution of weathering fluxes to either temperature or runoff, and an expanded discussion on erosion limitations and tipping elements, it may be challenging to significantly reduce the overall length, however, we will make considerable effort to do so.

Minor comments:

- p. 5, l. 105: why is the conservation of phosphate and silicate enforced and how is it done?
As mentioned in the CLIMBER-X description paper (Willeit et al. 2022, 2023), the weathering module includes equations for silicate weathering fluxes. Riverine fluxes of silicate have been disabled in the model set-up here, as they would introduce additional challenges related to nutrient conservation in the ocean. Instead, the budgets for silicate and phosphorus (as well as organic carbon, not mentioned in the manuscript) are balanced by assuming that sediment burial fluxes are returned in remineralized form to the surface ocean. Spatially, these surface fluxes are distributed proportionally to annual runoff. This simplified approach ensures the conservation of phosphorus and silica inventories within the ocean–sediment system throughout the simulation.

We will provide more information clarifying this (as well as that of organic carbon) in the revised manuscript, as this issue was also raised by Reviewer #3.

- p. 6, l. 127-130: please provide values for the parameters

We agree that it would be logical to provide values for α, b, and E_a, and will provide these values in the revised manuscript. We will specify in the revised manuscript that i represents the different lithologies to sum over, and explicitly define b(i), $E_{a,sil}$(i), and α(i), and as functions of lithology. We will also add the following table to the Appendix in the revised manuscript for reference:

Table A1: Lithological classes in GLiM (Hartmann & Moosdorf 2012) and their parameters. These lithological classes are summed using the Arrhenius equation. The lithological classes of loess (lo) an carbonate sedimentary rock (sc) are not shown here. The evaporites class (ev) is used only to compute phosphorus fluxes, and is therefore not considered here.

	Lithological code	Lithological class	Molality / weathering rate, b ((1/12)×molC/kg water)	Activation energy of silicates, $E_{a,sil}$ (kJ/mol)	Fraction to weather as carbonate rocks, α
1	mt	Metamorphics	0.007626	60	0.75
2	pa	Acid plutonic rocks	0.005095	60	0.42
3	pb	Basic plutonic rocks	0.007015	50	0
4	pi	Intermediate plutonic rocks	0.007015	60	0.42
5	py	Pyroclastics	0.0061	46	0
6	sm	Mixed sedimentary rocks	0.012481	60	0.76
7	ss	Siliciclastic sedimentary rocks	0.005341	60	0.36
8	su	Unconsolidated sediments	0.003364	60	0
9	va	Acid volcanic rocks	0.002455	60	0
10	vb	Basic volcanic rocks	0.007015	50	0
11	vi	Intermediate volcanic rocks	0.007015	50	0

- p. 7, l. 149ff: looking forward to the interactive ice-sheet simulations!

Thank you for the positive comment, we are also excited to share these simulations in a follow up study.

- p. 7, l. 155: 'pulse' might be a misleading term, maybe replace with 'idealized CO2 emission histories'?

This is a good point, especially with the potential inclusion of the ‘PULSE’ experiment ensemble. We will change this to ‘function’ in the revised manuscript.

- section 3.1.3: very nice!

Thank you for the positive comment on this section.

- Fig. 13: maybe clarify in the figure caption as well, why the smaller cumulative emissions lower fractions removed (-> more already taken up by other reservoirs before reaching the max. CO_2 perturbation)
We will added a sentence clarifying this, as suggested.

- section 4: I really liked this section!
Thank you for the positive comment on this section.

- Fig. 16: check caption text, not fully clear
We assume that the reviewer is referring to the description of the cumulative stacked barplot in Figure 16. Our intention of this description was to communicate that the barplot is cumulative. By default, plotting a stacked barplot in Python would be non-cumulative, and the length of each bar would represent the magnitude of carbon uptake. For example, as it is shown now, Figure 16a REF shows that land in the 500 PgC scenario takes up ~130 PgC carbon whereas land in the 1000 PgC scenario takes up ~240 PgC. Should the stacked bar plot be non-cumulative, the 500 PgC scenario would still be the same, but the 1000 PgC bar would instead reach ~370 PgC. However, we understand how it is currently written may cause confusion, so we will clarify this by rewriting the second sentence in the Figure caption.

- p. 32, l. 590: 'effect' -> 'affect'?
Thanks, we will change this in the revised manuscript.

- p. 35, l. 646ff: move this part to the other statements about silicate weathering before
Thanks, we will make this change.

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