Response to RC2

We sincerely thank the reviewer for their thoughtful and constructive review of our manuscript. We are encouraged that they found merit in our study and confident that incorporating their suggestions will enhance the quality of the 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 simulation, or from maximum CO_2 concentration.

Please find our point-to-point responses to the individual comments given by Reviewer #2 below (reviewer comment in black, our response in blue).

This study assesses the lifetime of anthropogenic CO2 and its dependence on total emissions and sensitivity to different carbon cycle processes. The authors use the fast EMIC CLIMBER-X to simulate an ensemble of 100,000-year simulations. They thoroughly analyze which carbon reservoir takes how much carbon and when. Special attention is paid to the timescales of silicate weathering and this study provides a shorter estimate of this timescale compared to previous studies. Next sensitivity experiments are discussed to assess how sensitive the results are to several important processes.
The study is interesting and textually well written. I believe most of the reasoning, as well as the used methodology, are sound. I have a couple of major comments, minor comments and specific comments for the authors to address.
We would like to thank the reviewer for their positive comments on our paper.

Major comments:

My first main comment is that the paper is quite long and has a lot of figures. I think the paper would benefit from being a bit shorter. I will leave it to the authors to decide exactly what to change, but I have provided some suggestions below:
We acknowledge that the manuscript is quite lengthy; also noted by Reviewer #1. We agree that some of the sections mentioned can be condensed and will make these adjustments in the revised manuscript. Additionally, we will review other areas, such as parts of Section 3.4, to identify content that may not significantly contribute to the discussion as to reduce the overall length. 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 will be challenging to significantly reduce the overall length, however, we will make considerable effort to do so.

- Section 5 mainly focuses on the weathering processes, and I believe this is also the main novelty in this study. Section 3 is much more elaborate. Is it necessary to, for example, put 3.1.1 and 3.1.2 in the main text or could they also go into the supplementary?

We disagree that weathering processes are the main novelty of our work. The main novelty of our work is that we have carried out long-term simulations with a comprehensive Earth system model, and have analyzed the response of the carbon cycle on different time scales – not just for weathering processes. Therefore, we disagree that Sections 3.1.1 and 3.1.2 do not contribute significantly to the main text. While it is true that the weathering processes and their behavior under different cumulative emission scenarios are a key novelty of our manuscript, the spatially explicit response of the land biosphere is also critical. In previous studies, the land has often been completely omitted (e.g., Archer et al. 1997; Lord et al. 2015; Köhler 2020), simulated with very low complexity and left unreported (e.g., Lenton & Britton 2006), or modelled with similar complexity but not over such long timescales (e.g., Brault et al. 2017). As highlighted by Reviewer #3, the non-monotonous response of soil carbon over long timescales observed in our study may even represent a novel finding. Furthermore, as ocean processes and carbonate chemistry are responsible for the majority of anthropogenic CO_2 removal, we also believe it would not be appropriate to completely omit this section. However, we agree that this section in particular could be streamlined and will make considerable efforts to shorten it where possible.

- Is it possible to combine some figures? E.g. Figures 7 and 8, and/or Figures 12 and 13.

We think it would be difficult to combine Fig. 7 and 8 without potentially omitting the response of a few variables. We also think it would be difficult to merge Fig. 12 and 13 considering the proposed changes to Fig. 13 suggested by Reviewer #3. However, we can remove Fig. 14a, as it more or less shows the same as Fig. 14b, and present Fig. 2 in a single column, similar to Fig. 3, 5, 7, 8 and 9.

- In the introduction the authors mention the next glacial cycle. Is it necessary to include this in the introduction?

While it is not necessary to mention the next glacial cycle, we believe it is important in discussing the current state of modelling the long-term future climate evolution. The inclusion of two examples may be excessive, however, so we will omit Lines 29-32.

- Are all figures necessary to go into the main text or can they go into the supplementary? E.g. Figure 1?

We have made full use of the appendix for figures that are not immediately essential at first glance. Indeed, our manuscript has many figures, but we believe our manuscript is stronger when including the responses of the different components (both spatially explicit and timeseries), the associated analyses, and the sensitivity experiments. We are inclined to leave Fig. 1 in the introduction as, like the previous comment, we believe it is important for establishing the current state of the field for long-term future climate modelling.

One thing that remains a bit unclear to me is how the time period where the emissions occur is treated in the analysis. I think it is necessary to mention this in a clear and explicit way. Is the period with emissions included in the 100,000 years? For what analyses is it included, and for what not?
Thank you for the suggestion. We agree that it would be helpful to clarify how the period with emissions is treated in the analysis. For figures showing the time evolution, t=0 corresponds to the beginning of the pulse. However, for all processing and further analysis (i.e., results not focused on the model response to emissions), the ramp-up period of emissions is excluded. We attempted to convey this by noting at the start of certain sections that the analyses were conducted for the time after the peak CO_2 concentration (i.e., $CO_2(t_0) = CO_2^{max}$), and to explicitly make a distinction between time after peak CO_2 concentration (yr) vs. time (yr) in the figures. In the revised manuscript, we will make sure it is clear that the emission period is part of the simulated 100,000 years and address any places where there could be ambiguities (e.g., caption of Fig. 4). Furthermore, as per Reviewer #1's suggestion, we will also incorporate results from the PULSE ensemble (where all the carbon is emitted in the first timestep), which would avoid complications arising from the emission phase duration.

Following on this comment, I also suggest including in the figures when there are CO2 emissions and/or when there is an atmospheric CO2 maximum. I think this will make the interpretation of the figures next to the text easier.
Thanks for the suggestion. We will make sure that this is explained in the figure captions, but will also try to find a way to emphasize this more clearly in the figure itself.

Minor comments:

Abstract: Is it possible to add how the range relates to the cumulative emission range?
We assume the reviewer is referring to the percentage range presented in the abstract that indicates land carbon uptake over 100 kyr. We will add a sentence linking this to the cumulative emission range in the revised manuscript.

Line 51: this statement misses a reference.
We will add a reference to Archer et al. (2009), which corroborates our statement here.

Line 105, 106: and through emissions.
That's a good point, thanks. We will add that as suggested.

Line 118, 119: I suggest mentioning here explicitly what processes the weathering scheme depends on. Since the weathering plays a major role in the manuscript, I think it is important that the weathering scheme is as explicit and clear as possible which also makes it easier to compare it to previous studies using different weathering schemes.
This has already been explicitly mentioned on Page 5, Line 116-117 of the manuscript: "PALADYN includes a rock weathering scheme influenced by runoff and temperature (Hartmann, 2009a; Börker et al., 2020), accounting for 16 different lithologies as described in Hartmann & Moosdorf (2012)." However, as per Reviewer #3's recommendation, we will also explicitly include the equations for loess and carbonate sedimentary rock weathering in Equation 2, and as per Reviewer #1's recommendation, we will add a table focused on weathering parameters for the different lithologies. We will also correct the "16 lithologies" as this was erroneous and should have been 13. This will help further clarify our weathering scheme.

Line 138: where do the numbers come from? Is there a source?
The reviewer here is asking where the values given for volcanic outgassing (0.0738 PgC yr^{-1} or 6.15 TmolC yr^{-1}) come from. We set volcanic outgassing to half the global silicate weathering rate at the pre-industrial time (this is already mentioned in the manuscript on Page 6, Lines 137-138). The numbers come from the 100,000 year equilibrium spin-up of the carbon cycle model as described in Willeit et al. (2023), which is mentioned a few lines later (Page 6, Lines 143-144). This value is not based on observations (although it is consistent with the range of observational estimates), but is determined from the condition ensuring that the carbon cycle model is in equilibrium at the pre-industrial climate state, which requires that volcanic outgassing is half the rate of simulated silicate weathering under pre-industrial climate conditions (Munhoven & François 1994; Willeit et al. 2023).

Line 185: I do not see a large response in atmospheric CO2 concentrations, whereas the response in temperature and land carbon are much clearer. I interpret this as that it is not the CO2 concentrations that cause the warming. What does cause this warming?
Many thanks for bringing this to our attention. Reviewer #3 also highlighted this issue. Upon re-evaluating the data, we recognize that this statement was erroneous. Our analysis suggests that the temperature stabilization observed in the 5000 PgC scenario during the first millennium is not driven by the release of soil carbon into the atmosphere, but rather by ocean dynamics and the AMOC. The extended decline in AMOC results in a cooling in the Northern Hemisphere which prevents global mean temperature (GMT) from rising after year ~150. After some time, this cooling is offset by Southern Hemisphere warming via the bipolar seesaw, and explains why GMT stabilizes during the better part of the first millennium. This behaviour continues until the abrupt AMOC recovery, which triggers a rapid increase in GMT (and there is a small bump in global temperature at this time). The role

of AMOC on temperature, rather than CO_2 (radiative forcing, log(CO_2)) is demonstrated in Fig. A1 of the "Additional material" section at the end of this document. We will revise this statement accordingly in the updated manuscript.

2d, e are not referenced.
While Fig. 2d is already referenced in Section 3.1.2, Page 12, Line 229, we acknowledge that Fig. 2e was indeed not referenced in the text. We will reference Fig. 2e in the revised manuscript.

4 is referenced before Fig. 3.
Thanks, we will switch these two.

Section 3.1: The sediments are not treated as explicitly as the other reservoirs. Is this for a reason?
Section 3.1 largely introduces the general response of our experiments and the relative partition of anthropogenic carbon into the different reservoirs. The reviewer here asks why we did not treat sediments as their own reservoir.
This was intentionally done here as we performed our experiments with an open carbon cycle. As mentioned in the Fig. 3 caption (and in Appendix B), we focus on changes in cumulative carbon flux from the atmosphere relative to the pre-industrial, instead of changes in carbon inventory (as typically done), as a way to get around this issue. Given that there is no direct air–sediment flux, any anthropogenic CO_2 absorbed by sediments must first go through the ocean. This is why we conglomerated the two as one reservoir.

Line 214: Is it possible to give a one or two sentence summary of Kaufhold et al. (2024) here?
We will add more information about Kaufhold et al. (2024) here.

Line 219: Fig. 4a is referenced, but there is not explicit treatment of soil carbon in Fig. 4a. Is the right figure referenced?
Thanks, instead of "soil carbon", this should have referred to "land carbon" inventory, as it explains how land carbon in low emission scenarios is prevented from declining as quickly as in high-emission scenarios, thanks to the sustained levels of soil carbon stocks.

Figure 7a and b show more or less the same thing. Is it necessary to show them both?
Indeed, the Revelle factor and pH are related to ocean chemistry and CO_2 absorption, but they focus on different aspects of the carbon cycle and acidification process. The Revelle factor indicates how easily the ocean can absorb CO_2, relative to the relative increase in atmospheric CO_2. Surface ocean pH indicates the acidity of sea water, which is affected by the actual amount of CO_2 absorbed. Although the Revelle factor is a critically important metric for communicating buffering capacity, it is unfortunately often not reported in such long-term studies on the uptake of anthropogenic CO_2 (both in terms of peak magnitude and duration across the different emission scenarios). We thought that calculating this explicitly might be useful for future studies to reference.

Line 313: Is it possible to determine how much of the changes in weathering rates are because of changes in temperature, and how much due to changes in run off? I think this would make for a nice addition.
This is something which we had already considered, but did not attempt as the manuscript was already quite long at that point. However, we agree that it would be a nice addition, and we will quantify the relative contribution of temperature and runoff to changes in the weathering rate in the revised manuscript.

Line 315-317: I do not fully understand this sentence.
The reviewer is referring to the following sentence: "For carbonate weathering, large changes are not only limited to the equatorial regions, although the highest and lowest weathering rates in South East Asia and Central Asia can also be explained by increases and decreases in precipitation (Fig. 11b,c)".
Here, we mean that large increases in carbonate weathering with increasing emissions is not only limited to the equatorial regions (as with silicate weathering), but it is more globally distributed. However, the change in carbonate weathering (where it increases and decreases) can also be explained by precipitation. We will clarify this in the revised manuscript.

Section 4.1: I suggest mentioning the noLAND term earlier.
Thanks for the suggestion. Reviewer #3 also raised a similar point. We will introduce the term in the experimental section of the text, remind the reader that this refers to the experiment with land carbon disabled, and include a reference to the experiment table.

Figure 16: Would it be beneficial to also construct panels for weathering?
We considered this as well but ultimately decided against it, as weathering is not a true carbon pool like the others (land, ocean, and sediments). Fig. 16 effectively shows the magnitude of carbon uptake of the different pools across the different emission scenarios at different timeslices. The main novelty here is to distinguish how the different sensitivity experiments change the magnitude of carbon uptake. As weathering consumes more CO_2 over time, we decided that it would not provide additional information, as it would only increase in relative magnitude between 1 kyr and 100 kyr.

Line 592: Is permafrost treated in the methane model?
Permafrost is implicitly treated in the methane model through the representation of anaerobic decomposition in saturated soils (which can occur in permafrost regions). In these areas, carbon in the active layer may decompose under anaerobic conditions, leading to methane emissions. However, methane emissions from permafrost regions are generally small compared to emissions from low-latitude wetlands.

Line 597: How does temperature evolve in intCH4 compared to REF as there is quite a strong increase in CH4 concentrations?
We initially included a more detailed discussion on this topic but later condensed it to maintain the manuscript's conciseness. In general, interactive methane increases peak temperature by up to approximately 0.5 °C, with higher emission scenarios exhibiting greater differences in peak temperature compared to the REF experiment. This is already noted on Page 34, Lines 600–602. However, temperatures in the intCH4 experiments tend to converge

to the REF experiments around 1 kyr (similar to CO_2 concentrations, Fig. 15). While there are some differences in simulated temperature, the intCH4 and REF experiments largely follow each other over time. At what time temperature is larger/smaller in the intCH4 experiment compared to the REF experiment is primarily influenced by the non-monotonic response of soil carbon to increasing emissions (see Fig. A2-A4 in the "Additional material" section at the end of this document). We will add a figure to the appendix of our revised manuscript showing the temperature evolution in the intCH4 experiment.

Line 674: I think it is good to explicitly mention that ESMs do not agree on centennial timescales.
Thanks, we will change this as suggested.

I suggest adding a discussion on how tipping points (might) affect the estimation of the timescales. The AMOC is already mentioned a couple of times in the text, but I think it would be good to reiterate that, and other tipping points, in Section 5.
Potential tipping points are inherently accounted for in our simulations, as CLIMBER-X already incorporates all "fast" tipping elements (e.g., AMOC, sea ice, permafrost, boreal forests, and Amazonian forests, etc.). The only exception to this are ice sheets, as the Greenland and Antarctic ice sheets are prescribed by their present day configurations in our study. However, we know that the Antarctic ice sheet will not entirely melt in our scenarios (Winkelmann et al. 2015). Only the West Antarctic Ice Sheet will (likely) melt, though most of its area lies over the ocean. As the impact of its melting on the AMOC remains uncertain (Wunderling et al. 2024), it is difficult to predict how this might affect the removal timescale of ocean invasion, and its subsequent influence on other timescales. On the other hand, the Greenland Ice Sheet (GrIS) is projected to melt under the strongest scenario, which could influence weathering rates. This effect has been previously investigated using CLIMBER-2 (Munhoven et al. 2007), but it was shown to be nearly negligible.
Without experiments explicitly simulating the crossing of tipping points (and all analyses therein), the influence of these events on the removal timescales of anthropogenic CO_2 cannot be assessed and remains highly uncertain. However, we can provide a statement acknowledging this uncertainty, and noting that the crossing of such tipping elements (although not seen in our experiments) might affect the long-term capacity different components to absorb carbon over time.

Specific comments:

Line 61, 62: It is not obvious how non-linear translates to the exponential functions. They are linked here through the word 'therefore', suggesting an obvious connection. I suggest either explaining why non-linear means exponential in this case or rewriting the second sentence a bit.
We will rewrite the second sentence to the following: "The decline in anthropogenic CO_2 is usually presented as a superposition of exponential decays (Maier-Reimer and Hasselmann, 1987; Archer et al., 1997; Archer and Brovkin, 2008; Colbourn et al., 2015; Lord et al., 2015b), with each function representing a different process in the carbon cycle that takes up carbon.".

Line 109: I suggest mentioning the units of Catm (i.e. PgC).
Thanks for the suggestion, we will add the unit for Catm as recommended.

Line 194: 'At peak CO2 concentrations, …'
We will change it as suggested.

Line 493: the double fraction does not look so nice in the text.
This is a good point. We will change this to $\tau(t) \propto \left(\frac{dCO_2(t)}{dt}\right)^{-1}$.

Line 532: remaining where? In the atmosphere?
Thanks for pointing this out. You are right that we did not clarify, and that it should indeed be "…leads to higher fraction of emissions remaining [in the atmosphere]". We will change this in the revised manuscript.

Line 566: I suggest rewriting this sentence a bit. I first thought that it meant that if a simulation has lower CO2 concentrations, it has a lower ECS.
We apologize for the miscommunication. We will change "a lower ECS is associated with lower atmospheric concentrations of CO_2 (and vice versa)" to "simulations using a lower ECS produce lower atmospheric concentrations of CO_2 (and vice versa)" in the revised manuscript.

Line 642: 'the presence of land' feels a bit awkward here.
We will change "the presence of land…" to "the inclusion of land carbon cycle processes effectively…".

Additional material:

Fig. A1: Role of radiative forcing and AMOC on the evolution of global mean surface temperature. Trajectories have been plotted for the entire 100,000 years.

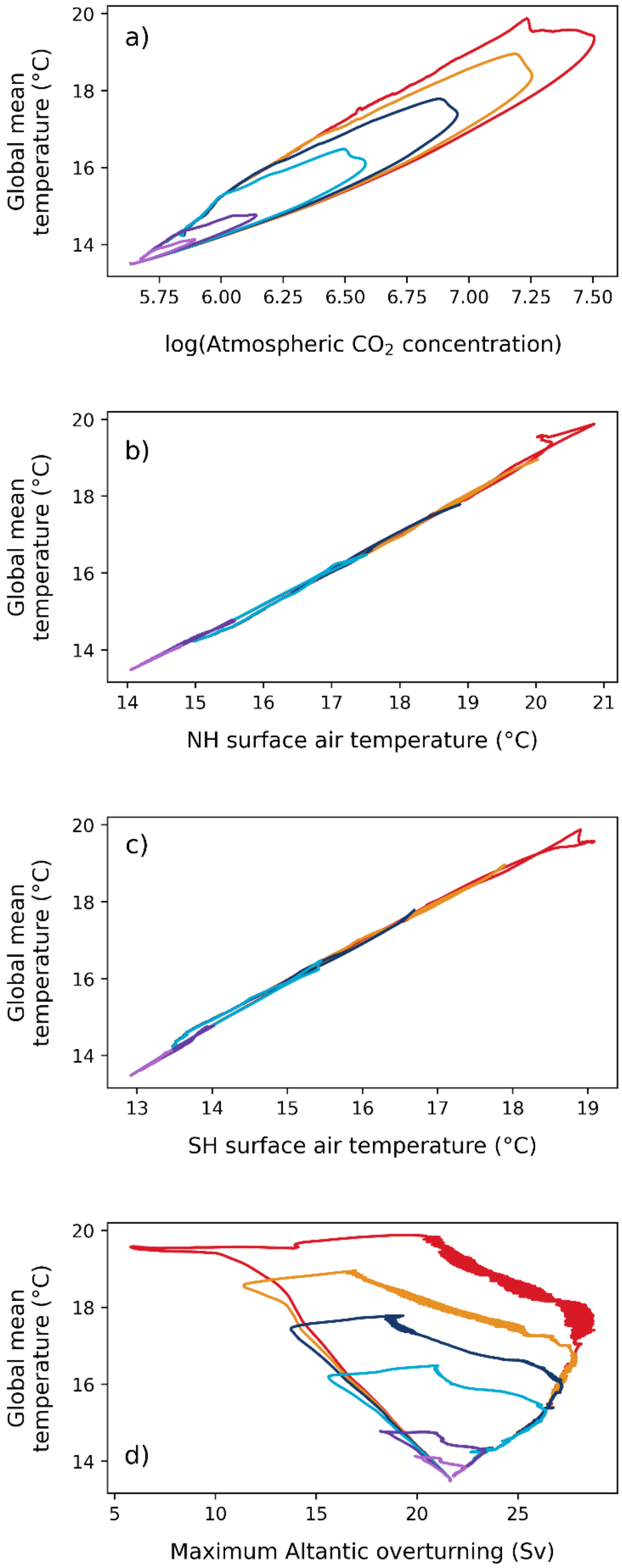


Fig. A2: Change in global mean surface temperature in the 0-3000 PgC emission scenarios. Colours here correspond to the cumulative emission scenarios shown in Fig. 2 of the manuscript. The response in temperature is shown here for two experiments: solid line for the intCH4 experiment and dashed lines for the REF experiment.

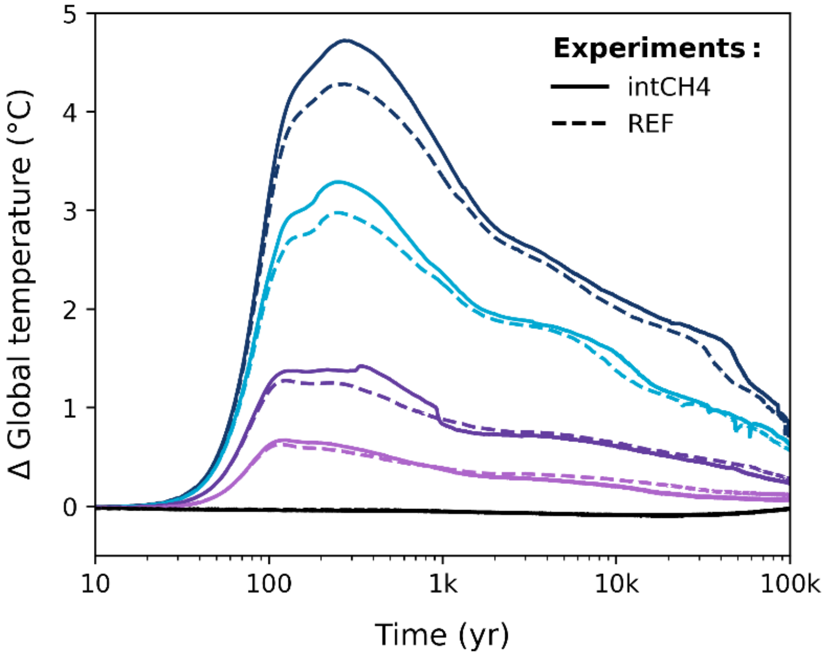


Fig. A3: Change in vegetation carbon inventory in the 0-3000 PgC emission scenarios. Colours here correspond to the cumulative emission scenarios shown in Fig. 2 of the manuscript. The response in vegetation carbon is shown here for two experiments: solid line for the intCH4 experiment and dashed lines for the REF experiment.

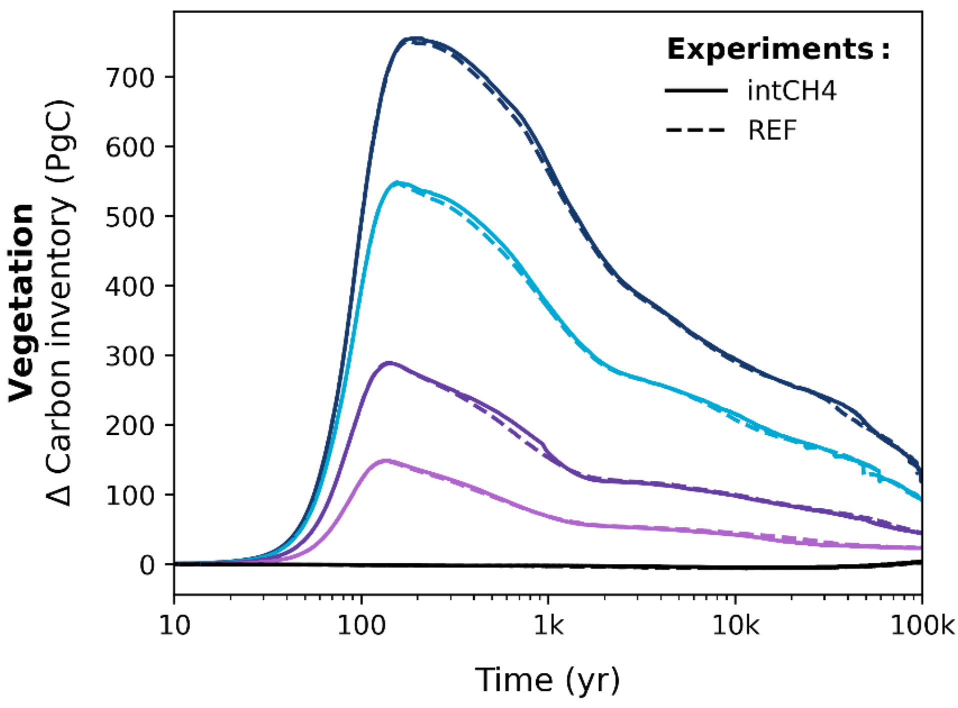
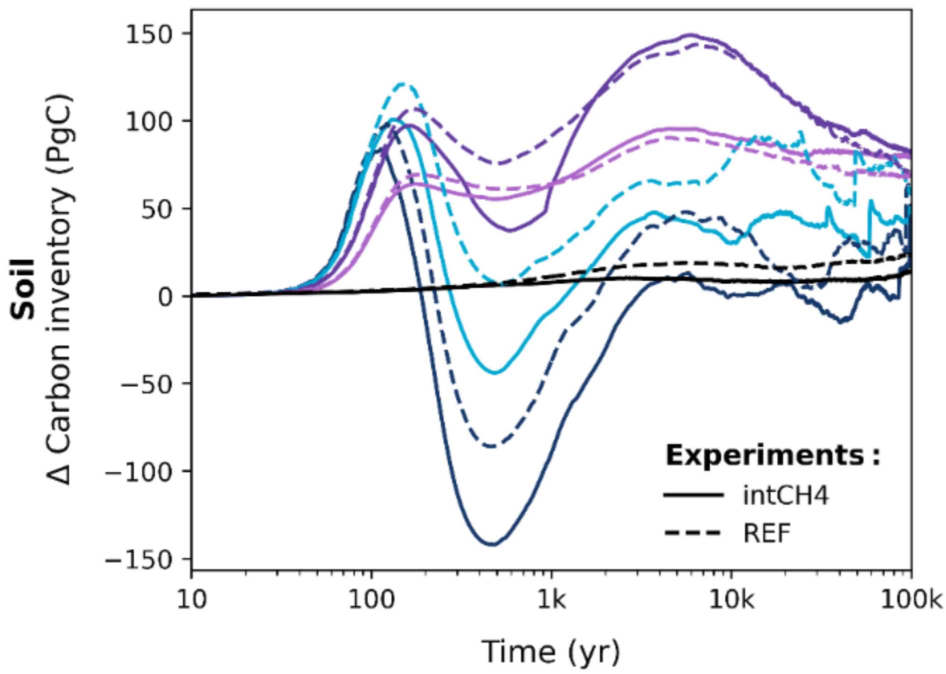


Fig. A4: Change in soil carbon inventory in the 0-3000 PgC emission scenarios. Colours here correspond to the cumulative emission scenarios shown in Fig. 2 of the manuscript. The response in soil carbon is shown here for two experiments: solid line for the intCH4 experiment and dashed lines for the REF experiment.



References:

Archer, D., Eby, M., Brovkin, V., Ridgwell, A., Cao, L., Mikolajewicz, U., Caldeira, K., Matsumoto, K., Munhoven, G., Montenegro, A. & Tokos, K. (2009). Atmospheric lifetime of fossil fuel carbon dioxide. *Annual Review of Earth and Planetary Sciences*, 37(1), 117–134. https://doi.org/10.1146/annurev.earth.031208.100206

Archer, D., Kheshgi, H. & Maier-Reimer, E. (1997). Multiple timescales for neutralization of fossil fuel CO_2. *Geophysical Research Letters*, 24(4), 405–408. https://doi.org/10.1029/97gl00168

Brault, M.-O., Matthews, H. D. & Mysak, L. A. (2017). The importance of terrestrial weathering changes in multimillennial recovery of the global carbon cycle: A two-dimensional perspective. *Earth System Dynamics*, 8(2), 455–475. https://doi.org/10.5194/esd-8-455-2017

Köhler, P. (2020). Anthropogenic CO_2 of high emission scenario compensated after 3500 years of ocean alkalinization with an annually constant dissolution of 5 Pg of Olivine. *Frontiers in Climate*, 2. https://doi.org/10.3389/fclim.2020.575744

Lenton, T. M. & Britton, C. (2006). Enhanced carbonate and silicate weathering accelerates recovery from fossil fuel CO_2 perturbations. *Global Biogeochemical Cycles*, 20(3). https://doi.org/10.1029/2005gb002678

Lord, N. S., Ridgwell, A., Thorne, M. C. & Lunt, D. J. (2015). An impulse response function for the "long tail" of excess atmospheric CO_2 in an Earth system model. *Global Biogeochemical Cycles*, 30(1), 2–17. https://doi.org/10.1002/2014gb005074

Munhoven, G. & François, L.M. (1994). Glacial-Interglacial Changes in Continental Weathering: Possible Implications for Atmospheric CO2 . In: Zahn, R., Pedersen, T.F., Kaminski, M.A., Labeyrie, L. (eds) Carbon Cycling in the Glacial Ocean: Constraints on the Ocean's Role in Global Change. NATO ASI Series, vol 17. Springer, Berlin, Heidelberg. https://doi.org/10.1007/978-3-642-78737-9_3

Munhoven, G., Brovkin, V., Ganopolski, A. & Archer, D. (2007). Impact of future Greenland deglaciation on global weathering fluxes and atmospheric CO_2 [Paper presentation]. 17th V. M. Goldschmidt Conference 2007, Cologne, Germany.

Winkelmann, R., Levermann, A., Ridgwell, A. & Caldeira, K. (2015). Combustion of available fossil fuel resources sufficient to eliminate the Antarctic Ice Sheet. *Science Advances*, 1(8). https://doi.org/10.1126/sciadv.1500589

Wunderling, N., von der Heydt, A. S., Aksenov, Y., Barker, S., Bastiaansen, R., Brovkin, V., Brunetti, M., Couplet, V., Kleinen, T., Lear, C. H., Lohmann, J., Roman-Cuesta, R. M., Sinet, S., Swingedouw, D., Winkelmann, R., Anand, P., Barichivich, J., Bathiany, S., Baudena, M., Bruun, J. T., Chiessi, C. M., Coxall, H. K., Docquier, D., Donges, J. F., Falkena, S. K. J., Klose, A. K., Obura, D., Rocha, J., Rynders, S., Steinert, N. J. & Willeit, M. (2024). Climate tipping point interactions and cascades: A Review. *Earth System Dynamics*, 15(1), 41–74. https://doi.org/10.5194/esd-15-41-2024