Response to RC3

We appreciate the reviewer's positive assessment of our manuscript and are pleased that they found the study valuable. We thank Pierre Maffre for his detailed and constructive feedback, which has certainly strengthened 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 simulation, or from maximum CO_2 concentration.

Please find our point-to-point responses to the individual comments given by Reviewer #3 below (reviewer comment in black, our response in blue).

Kaufhold et al. manuscript addresses the question of the fate of anthropogenic CO2 and climate in the long-term future (100 thousand years). The authors used an Earth system model of intermediate complexity, which has several new implemented processes compared to previous similar studies. They clearly explain the novelties of their study, and the new findings. The manuscript is very well written, and well organized. It is well-suited for publication in Biogeosciences, with some minor revisions.
We would like to thank the reviewer for their positive comments on our paper, and will make improvements where highlighted.

Major comments:

I only have one major comment, which concerns the silicate weathering sensitivity to climate.

The authors emphasize their re-estimation of the timescale of carbon removal by silicate weathering, to shorter values than previously thought. They partly attribute this finding to a stronger weathering feedback, which is compared to several estimations (Fig. 10b) and found to fall within the range, though on the upper part (doubling of weathering flux at +4°C, that is +18% per °C of warming).
Among the processes not represented in CLIMBER-X weathering model is the erosion limitation of weathering, or the "soil shielding effect", which is a different point of view of the exact same process. Soil shielding was extensively discussed in Hartmann et al. (2014) (cited in the manuscript), but wasn't yet implemented in Hartmann et al. (2009). Actually, soil shielding is not explicitly represented in any of the model presented in Fig. 10b.
I admit that there is no consensus on how this would affect the sensitivity of weathering to global climate (i.e., the weathering feedback strength), which is the point of interest here. Yet, there are several clues that it would significantly reduce the feedback strength:

- Godderis et al., Geoderma, 2008 (10.1016/j.geoderma.2008.01.020) showed that the sensitivity of tropical weathering to runoff is largely overestimated (~ 5-fold) if considered similar than for temperature climates. Indeed, in the present manuscript, tropical environments dominate the weathering flux, and its response to global warming.
- Maher & Chamberlain, Science, 2014 (10.1126/science.1250770), who also addressed the issue of erosion limitation, suggested a "maximal" weathering sensitivity, in actively eroding mountains, of +5% per °C of warming (which is lowest estimate presented on current Fig. 10b), and an average sensitivity of +1.2% per °C of warming.
- Another weathering model taking into account erosion limitation, and that is spatially explicit, Maffre et al., Clim. Past, 2023 (10.5194/cp-19-1461-2023), suggests a global weathering sensitivity of ~ +9% per °C of warming, though it is unclear if the best fitting functional form should be exponential or linear.

Given the absence of consensus on a value for weathering sensitivity, I do not consider that the present results should be revisited. Simply, I vividly recommend the authors to add more nuances on their statement about weathering timescale (which is one of their main conclusions), and to provide more discussion about weathering sensitivity, the large uncertainty that exists in the literature concerning its value, and how it should affect the weathering timescale.

We are aware of the soil shielding effect, and it was commented on in the CLIMBER-X carbon cycle description paper (Willeit et al., 2023): “The effect of soil shielding on the weathering rate suggested by Hartmann et al. (2014) has not been considered since information on soil shielding is not readily available for periods beyond the recent past.” As the reviewer correctly identifies, the effect of soil shielding has not been considered by our model (and others) largely because there is no consensus on how it would effect the weathering feedback. However, we do not dismiss the possibility it could significantly change –and potentially weaken— the strength of the weathering feedback. In saying this, we will add a paragraph discussing this potential caveat. We also appreciate the compilation of references; they will be added to our manuscript and will give considerable depth to our discussion.

Specific comments:

Section 2.2 (lines 100–110): there is a missing information here about the organic carbon cycle. As far as I understand, the sediment component is run as an open system (with sediment loss through burial), and this sediment contains organic carbon generated by marine primary productivity (lines 255–256). Therefore, and given Eq. (1), setting Fvolc to half of the global silicate weathering flux (as indicated lines 136–138) would not result in a steady-state carbon cycle, because of this additional C sink (organic carbon burial), that would result in a net ocean-to-atmosphere flux lower than the remaining term "Fvolc – Fweath". Unless the organic carbon cycle is forced to work as a closed system (like silicate and phosphate, lines 106–107), and all buried organic carbon is put back into the atmosphere?
Many thanks for pointing out this critical issue! We have an open carbon cycle in CLIMBER-X but, indeed, a closed nutrient cycle. We recycle organic carbon in marine sediments along with nutrients, and sediment burial fluxes are returned in remineralized form to the surface ocean while compensating for the subduction of inorganic carbon by volcanic outgassing. Reviewer #1 also raised a similar concern, so we will provided a sentence clarifying the behaviour of phosphorus, silicate, and organic carbon in CLIMBER-X.

Lines 125–126: I do not understand why "carbonate sedimentary rock" should be different than "carbonate", in term of weathering (Eq. 2). Moreover, why not indicating the equations for "carbonate sedimentary rocks" weathering and loess weathering?
Thanks for your comment; we hope that we can clarify this. In Hartmann & Moosdorf (2012), there are three carbonate-rich sedimentary lithologies, which are mixed sedimentary rocks (sm), evaporites (ev), and carbonate sedimentary rocks (sc). The evaporites class (ev) is used only to compute phosphorus fluxes, and is therefore not considered here.
However, other lithologies still maintain information on carbonate content, such as unconsolidated sediment (su) and metamorphics (mt). When we specify “carbonate sedimentary rocks”, we mean that that the contribution of the lithology “sc” to carbonate weathering rates in a grid cell is not calculated using an Arrhenius equation. In addition to the 13 rock lithologies as listed in Table A2 in Hartmann & Moosdorf (2012), we also consider loess (lo) as another lithology. The contribution of “lo” to carbonate weathering rates in a grid cell is similarly not calculated using an Arrhenius equation. We included a table of the different lithologies (Table A1) in the “Additional material” section below, and will add this to the appendix of the revised manuscript.
Therefore, the Equation 2 presented in text is what is used for all other lithologies. As it is now, the “accounting for 16 different lithologies” (Page 6, Line 116) and sum over 14 is erroneous, and it should be 13 and 11 respectively. We agree it would be useful to show the equations that are used for the other two lithologies (loess and carbonate sedimentary rock), which is why we will incorporate them into the revised manuscript.

Lines 132–136: I think that orbital forcings could be mentioned here, among the "external forces" (line 132) excluded in the study, although it may be redundant with line 145.
We had a similar thought, and deliberated which section would be most appropriate for this information. However, we will repeat it here, especially in light of the recommended changes in the following comment.

Lines 145–146: It is not completely clear here whether the fixed orbital forcings concern only the spin-up run, or all simulations (including the spin-up).
On Page 7, Lines 152-153 we state that “All simulations run for 100,000 years with constant orbital parameters and without any climate acceleration technique”. We will move the aforementioned Lines 145-146 before Line 152 to highlight that orbital parameters are constant in all simulations.

Lines 169–170: I think it would be useful here just to indicate that climate sensitivity is altered by rescaling the pCO2 seen by the radiative code as a function of the actual pCO2, and then refer to Appendix A.
Thanks for your suggestion. We will change this part as recommended.

Lines 185–186: This statement, "temperatures temporarily stabilize instead of decreasing due to the release of soil carbon into the atmosphere" seems erroneous. Temperature does stabilize during between 150yr and 1000yr in the 5000 PgC scenario (Fig. 2b), but pCO2 declines just as in the other scenarios (Fig. 2a). So how could it be an effect of the "release of soil carbon into the atmosphere"? It rather seems that there is a decoupling of CO2 and temperature, that is likely due to oceanic dynamics. Indeed, there is a small bump of global temperature at 700yr (without any pCO2 change), which coincides with abrupt AMOC recovery (Fig. 7e).
Many thanks for pointing this out. This is indeed an erroneous statement. Upon reviewing the data, we agree that the temperature stabilization in the 5000 PgC scenario within the first millennium is not explained by the release of soil carbon into the atmosphere. It does appear that the likely cause is oceanic dynamics and AMOC, as pointed out. 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 indeed the 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.

Lines 215–221: this non-monotonous behavior is interesting. Has it been already suggested, or is it a new finding of current study?
To the best of our knowledge, this has not yet been explicitly observed in a previous study on the long-term effects of anthropogenic CO_2. This is mostly because land carbon was often not considered (or the response unreported, as in Lenton & Britton 2006). However, we are not prepared to conclude that this is necessarily a new finding (e.g., a strong positive climate-carbon cycle feedback related to soil respiration has already been highlighted in studies such as Cox et al. (2000)). On a global level, the response of soil carbon to increasing emissions is generally dictated by (1) that which is gained from increases in primary production and litterfall, and (2) that which is lost from higher soil respiration, influenced by different competing feedbacks.

Line 222: This statement, "In our simulations, the land is a net carbon sink for the entire 100 kyr" also seems erroneous. From Fig. 3a, it appears that land becomes a (slight) net source of carbon at 200kyr in all simulations. Besides, I don't think that "land carbon" is defined anywhere in the manuscript. Is it simply "soil + vegetation" carbon?
Thanks for bringing this to our attention. We assume the reviewer here means 200 years, not 200 kyrs. Indeed this is an erroneous statement and it will be corrected to "the terrestrial storage of anthropogenic carbon is positive during the entire run". We will also make sure to explicitly define land carbon in the manuscript (as the sum of vegetation and soil carbon).

Line 364: The mention of "noLAND" comes quite abruptly here, given that the sensitivity experiments are only discussed in a later section (4). Could you remind "experiment with land carbon disabled", and refer to Table 1?
Thanks for the suggestion. Reviewer #2 also raised a similar point. We will introduce the term earlier in the experimental section of the text, clarify that this refers to the experiment with land carbon disabled, and include a reference to the experiment table.

Fig 13: It is difficult to visualize the trends of Ai and τi versus cumulative emission (trends that are discussed in the current section). I suggest adding two small panels in the figure, plotting Ai vs E and τi versus E.
That's a good idea, we will add two subplots for A_i vs E and τ_i vs E in Figure 13 (see Fig. A2 in the "Additional material" section at the end of this document).

Lines 371–377: It might be useful to indicate here that Ai do not sum at 1 because the IRF does not start at 1, and that the initial value (= the sum of Ai) depends on the cumulative emission scenario.
Thank you for the suggestion. To some extent, this information was included in the Table 2 caption, but we will integrate it into the main text to ensure its visibility in the revised manuscript.

Lines 565–572: It seems that there is a positive feedback here: warmer temperature (for a same pCO2) generates higher pCO2, because of the warming-induced soil carbon release. It would be useful to indicate that it is a positive feedback.
We will add this to the revised manuscript.

Line 594: Is methane lost by converting it into CO2? Granted that 2200 ppb of methane should not generates more than 2 ppm of CO2, with is much less than the pCO2 anomaly reported in Fig. 15d.
Firstly, we would like to clarify that 2200 ppb is peak CH_4 concentration in the 5000 PgC scenario, whereas the sensitivity analysis in Section 4 is limited to 3000 PgC and less. This, of course, does not answer the reviewer's question, as peak CH_4 of 1600 in the 3000 PgC scenario alone cannot explain an additional 25 ppm of atmospheric CO_2.
Methane is oxidized assuming a constant lifetime of 9.5 years (Willeit et al. 2023). In reality, this results in CH_4 being converted into CO_2 in the atmosphere, but this flux is small. For simplicity (and for carbon conservation), we add carbon from methane to surface CO_2 flux in CLIMBER-X (e.g., soil CO_2 emission).

The reason why CO_2 is higher in the intCH4 experiments is because, as mentioned in the previous comment, CH_4 causes an additional positive climate-carbon cycle feedback (as additional warming enhances soil respiration; see Fig. A3-A4 in the "Additional material" section at the end of this document).
We originally had a larger discussion on this, but it was cut in our efforts to (already) shorten the paper. However, Reviewer #2 had a similar question, asking how temperatures evolve in the intCH4 experiments compared to the REF experiments (as a result of this large increase in CH_4 concentration), so we will elaborate on this more in the revised manuscript.

Lines 644–645: Would it really influence the ATMOSPHERIC lifetime of CO2? It seems to me that the longer weathering timescale is the just a delay because of carbon storage in land before it is stored through weathering, instead of being directly stored though weathering, and that this sink transfer has no consequence regarding carbon in the atmosphere.
You make a good point, and it is true that the longer (effective) weathering timescale is caused by the temporary storage of carbon on land. However, the land carbon pool on its own does increase the residence time of anthropogenic CO_2, as the land stores about 20-40% of anthropogenic carbon (Page 11, Fig. 4) before gradually releasing it into the atmosphere. This ultimately slows down the CO_2 decline on long timescales (Fig. 17a).

Technical corrections:

There are several occurrences where it should be more accurate to talk about weathering "flux", than weathering "rate", which rather refers to a specific flux (in mol/m2/yr): line 138, line 281, caption of Fig. 9, line 291, line 301...
Thanks, we will change the word "rate" to "flux" where appropriate.

Line 130: It seems that "run-off" should be spelled "runoff", to be consistent with the other occurrences of that word in the manuscript.
Thanks for pointing this out. It will be changed to "runoff" in the revised manuscript.

Caption of Fig. 6: The mean net annual NPP is in (a–c), not (a–b).
Thanks, this will be corrected.

Fig. 11: A mere suggestion: it feels more "natural" to use a colorscale with "wetter" colors (e.g., blue) for precipitation increase and "dryer" colors (e.g., red) for precipitation decrease.
Thanks for the suggestion. We will change Figure 11 h-i to a brown–bluegreen colormap, which is often used to indicate "drier" and "wetter" conditions.

Line 526: I believe that "begin" should here be a singular, "begins".
Thanks, this will be changed in the revised manuscript.

Line 638: Shouldn't "variation" be a plural here?
Thanks, we will correct this.

There are a few inconsistencies between US and British spelling. I noticed the use of "behavior" and "behaviour" in the text. Please check.

Thanks for pointing this out. The reviewer points out inconsistencies between US and British spelling (e.g., the use of “colour” but then at the same time “parametrize”). Some of these inconsistencies can be explained by the chosen variety of English, Canadian English, which is the first author’s first language and is accepted by the EGU. However, we will change “behavior” to “behaviour” in text, and willl check for other inconsistencies (e.g., “…ise” → “…ize”) to remain consistent with Canadian English.

Many DOIs link have duplicated "https://doi.org/https://doi.org/" in the reference list. Pleas check.

Many thanks for pointing this out. We will correct the DOI links in the references.

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.

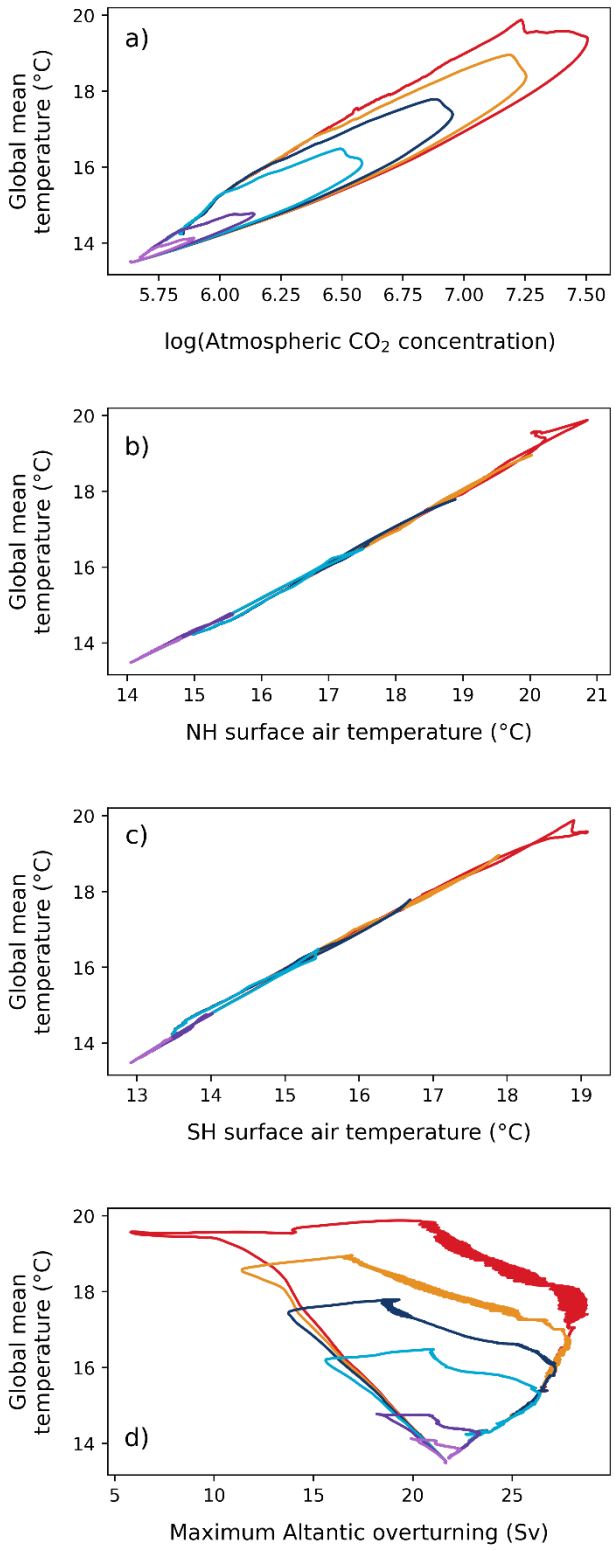

Fig. A2: Preliminary version of the revised Fig. 13 in the manuscript. This figure now includes subplots for (b) A_i vs E and (c) τ_i vs E. The results for the PULSE experiment will also be shown here once available.

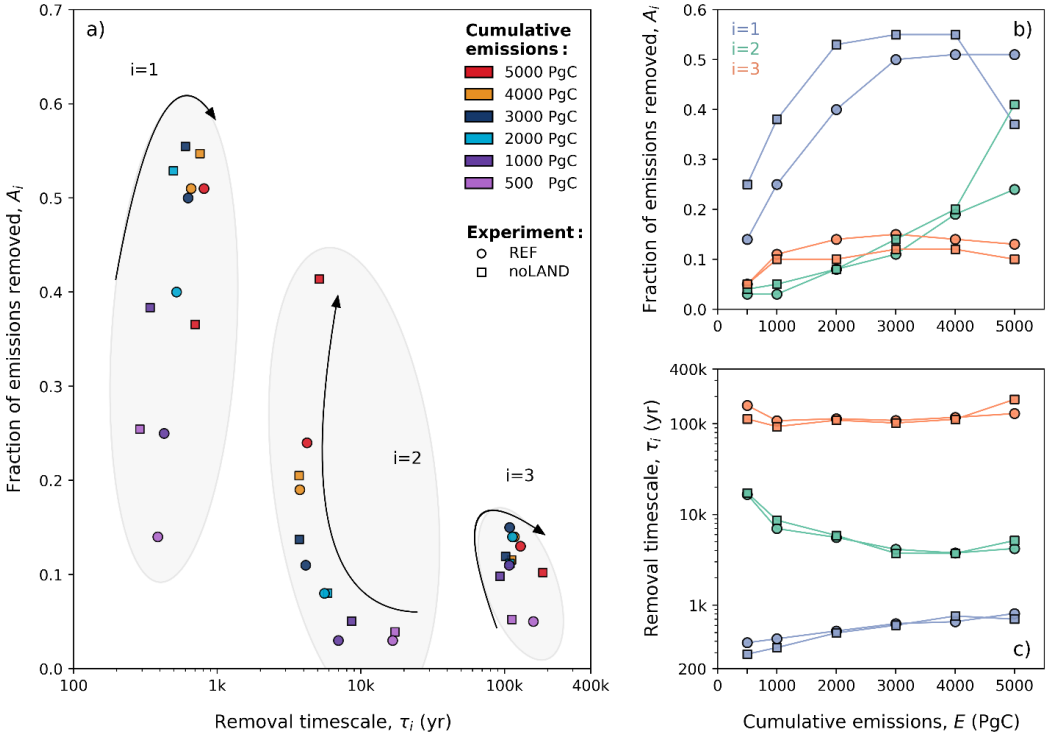

Fig. A3: Change in global mean surface temperature in the 0-3000 PgC emission scenarios. Colours here correspond to the cumulative emission scenarios shown in Fig. 2 of the manuscript. The response in temperature is shown here for two experiments: solid line for the intCH4 experiment and dashed lines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

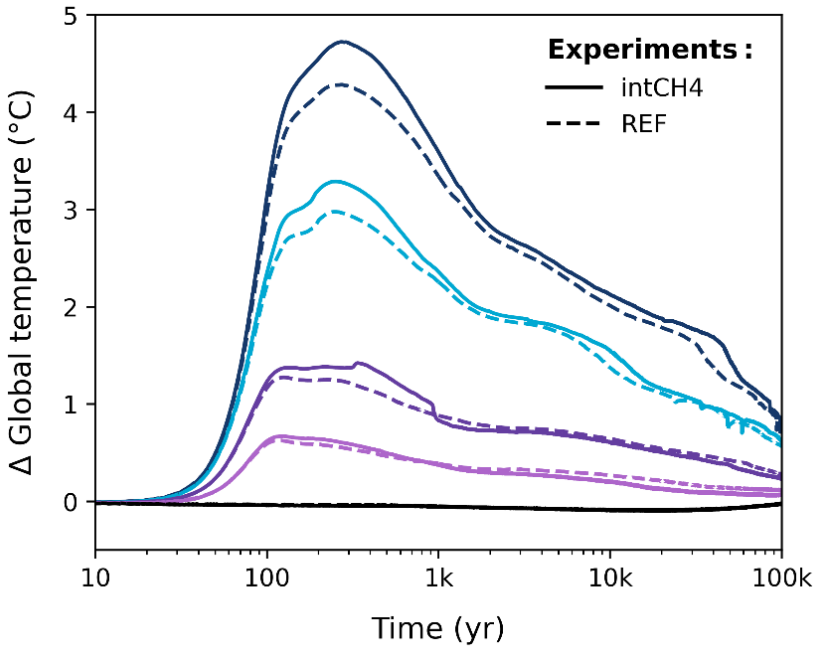

Fig. A4: Change in soil carbon inventory in the 0-3000 PgC emission scenarios. Colours here correspond to the cumulative emission scenarios shown in Fig. 2 of the manuscript. The response in soil carbon is shown here for two experiments: solid line for the intCH4 experiment and dashed lines for the REF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

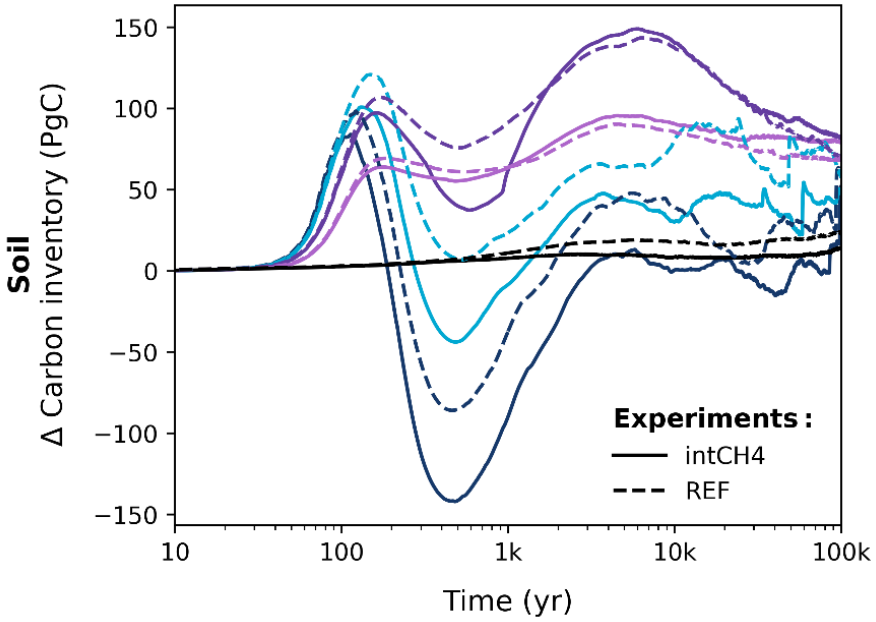

Table A1: Lithological classes in GLiM (Hartmann & Moosdorf 2012) and their parameters. These lithological classes are summed using the Arrhenius equation. The lithological classes of loess (lo) an carbonate sedimentary rock (sc) are not shown here. The evaporites class (ev) is used only to compute phosphorus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

References:

Cox, P. M., Betts, R. A., Jones, C. D., Spall, S. A. & Totterdell, I. J. (2000). Acceleration of global warming due to carbon-cycle feedbacks in a coupled climate model. *Nature*, 408(6809), 184–187. https://doi.org/10.1038/35041539

Hartmann, J. & Moosdorf, N. (2012). The new global lithological map database GLiM: A representation of rock properties at the Earth Surface. *Geochemistry, Geophysics, Geosystems*, 13(12). https://doi.org/10.1029/2012gc004370

Hartmann, J., Moosdorf, N., Lauerwald, R., Hinderer, M. & West, A. J.: Global chemical weathering and associated P-release — The role of lithology, temperature and soil properties, *Chemical Geology*, 363, 145–163, https://doi.org/10.1016/j.chemgeo.2013.10.025, 2014.

Willeit, M., Ilyina, T., Liu, B., Heinze, C., Perrette, M., Heinemann, M., Dalmonech, D., Brovkin, V., Munhoven, G., Börker, J., Hartmann, J., Romero-Mujalli, G. & Ganopolski, A. (2023). The Earth System Model CLIMBER-X v1.0 – Part 2: The global carbon cycle. *Geoscientific Model Development*, 16(12), 3501–3534. https://doi.org/10.5194/gmd-16-3501-2023