We sincerely thank the reviewer and editor for their thoughtful and positive assessment of our work. We appreciate the recognition of the importance of our findings on cation retention beyond exchangeable pools, and we are glad that our study contributes valuable insights to the field of enhanced weathering. We also appreciate the constructive suggestions provided, which will help us further improve the clarity and impact of our manuscript.

Please find our point per point incorporations of the reviewers suggestions below:

reviewers comments in blue, our replies in black.

General remarks
This study addresses the important matter of cation retention in soils treated with minerals for the purposes of enhanced weathering (EW). Cation retention is a major obstacle to inorganic carbon capture on timescales consistent with climate change mitigation, via DIC or cation export in effluent. As with many previous experiments, this study does not observe significant alkalinity/cation export resulting from feedstock treatment even though weathering can be inferred to have taken place.
While cation exchange is a key process leading to cation retention, it is not the only process. Here, four soil cation pools are examined, and results do suggest significant retention of cations in non-exchangeable pools. This is a key result that deserves to be published even though the details of retention (e.g., in secondary minerals) have not been quantified.
I did not review the original manuscript. It looks like the authors have addressed many of the points raised during the initial review stage, although there are a few places where I think further improvements may be advised (see below).

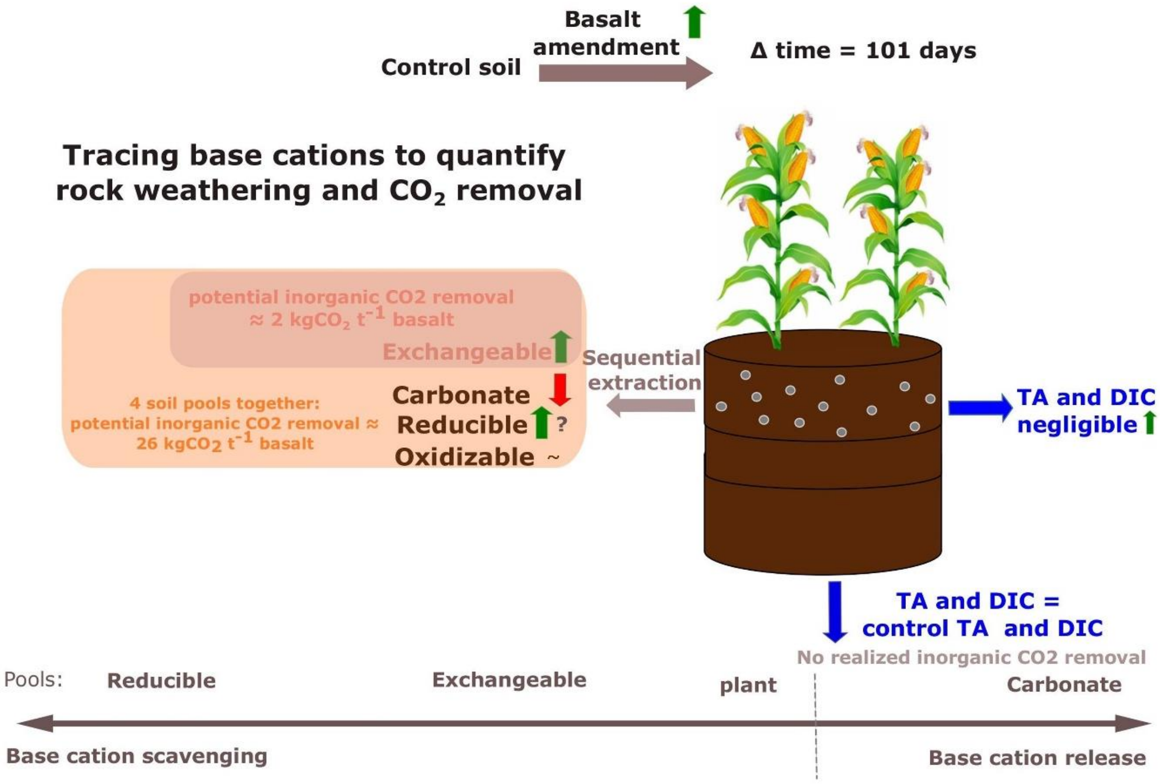
Q1) Graphical abstract) I don't particularly like the representation of the log weathering rate ~ -12 mol TA/m2basalt/s, which looks like reverse weathering due to the negative sign and juxtaposition of the unlogged units. It might be better to represent this as log Wr (mol Ta/m2/s) ~ -12 instead. This value (-12) agrees with line 510 (discussion) but disagrees with the abstract (-11) in any case. Please add the correct value throughout the manuscript. Potential inorganic CO2 removal is ambiguous in the literature (this was raised by the original reviewers and the text has apparently been tightened up). In this graphical abstract, two different figures are given for it. It is not immediately clear what these values represent.

Thank you for pointing out the ambiguity in the graphical abstract. Regarding the two different values for log Wr and potential inorganic CO_2 removal in the previous graphical abstract, the distinction arises from considering different soil cation pools:

- The value around **-12** corresponds to weathering based on the **exchangeable pool only**,
- While the value near **-11** includes **all measured pools** (exchangeable plus non-exchangeable).

Quantification of weathering rates is complex and the obtained rate depends on which pools are included in the quantification. We may have missed unextractable base cations if secondary minerals formed. Therefore, we think it is better to not exclude weathering rates in the graphical abstract and only show 'potential inorganic CO_2 removal' (for the exchangeable as commonly done and for all extractable pools).

To improve clarity, we have revised the graphical abstract by enclosing numbers for potential inorganic CO2 removal in separate boxes with only “exchangeable” or “4 soil pools together,” making it explicit what each value represents.



We also added a comment in the methodology that weathering rates only follow from quantification of detected base cations and may be an underestimation.

Line 287: Weathering rates are uncertain due to variability in soil cation pools considered, potential underestimation from unextractable base cations in secondary minerals and methodological limitations in detecting all weathering products.

Q2) Abstract

Line 25: This refers to neoformed solid inorganic C; consider adding the word "neoformed" here.

OK. We added neoformed.

1. Introduction

Q3) Line 62: The residence time in the ocean water column discussed by Renforth and Henderson is relevant to climate change mitigation, but the timescales of oceanic carbon precipitation are actually longer (this is the pathway that is thought to regulate Earth's climate on multimillion year timescales, e.g. Berner's work).

We agree that oceanic carbonate precipitation occurs on much longer timescales and have added a sentence to the introduction citing Berner to clarify this point. We rephrased line 47:
In this study, rather than aiming to quantify a full greenhouse gas budget, we focus on DIC export from soils to the ocean. This pathway is considered the most durable form of carbon sequestration, storing C on timescales exceeding those required for climate change mitigation (Phil Renforth & Henderson, 2017; Berner, 1991).

Q4) Line 70: "is comprised of" should be replaced by "is composed of".

OK, we applied this suggestion.

Q5) Figure 1: The percentages in this figure should be explained briefly in the caption.

We explain the percentages in the caption in line 94: **Realized CDR (100%)** means that all alkalinity produced by mineral weathering is fully leached from the soil, achieving the maximum possible inorganic CO_2 removal for that amount of weathering. In contrast, **Realized CDR (50%)** accounts for pedogenic carbonate formation (e.g., Reaction 4), where half of the alkalinity released by weathering is consumed locally, reducing the inorganic CO_2 removal by 50%.

Q6) Line 96: There are multiple SOM pools that can sorb cations (e.g. Tipping and Hurley's Windermere model, which could be cited here). Please clarify which SOM groups/pools comprise part of the exchangeable pool as determined by an NH4Ac extraction. On Line 101, SOM is also given as a potential "reducible" pool, and on line 105 the carboxylic and phenolic groups are assigned to the "oxidizable" pool. It is possible that some readers could be confused about the role of SOM; please clarify which SOM groups will contribute to the different pools.

Thank you for raising this important point. We agree that the terminology around soil organic matter (SOM) pools can be confusing. Rather than associating specific functional groups (e.g., carboxylic or phenolic) directly with exchangeable or oxidizable pools, it is more accurate to distinguish these pools based on the **strength of chemical binding**. The **exchangeable pool** comprises weakly bound cations that can be displaced by NH_4Ac extraction, while the **oxidizable pool** includes more strongly bound organic matter that requires chemical oxidation for release. Thus, the classification depends primarily on binding strength rather than the identity of particular functional groups.

To clarify this, we revised the text and included an additional relevant reference to Tipping and Hurley's Windermere Humic Aqueous Model. We also refer to the schematic of ion sorption on mineral surfaces (Fig. 5.6) from the soil science textbook by Blume, Scheffer, and Schachtschabel to provide further conceptual support.

We clarified in line 112: In the fourth soil pool considered here, the oxidizable pool, SOM can form strong bounds with cations after deprotonation (Kalinichev et al., 2011; Tipping & Hurley, 1992). SOM can thus scavenge cations in both the exchangeable and oxidizable pool. The binding strength between SOM and a cation determines whether the cation resides in the exchangeable or oxidizable pool (as graphically schematized in Blume et al., (2016), Fig. 5.6).

And in line 102: Exchangeable pool cations form relatively weak chemical bounds in a diffuse layer or with outer-sphere interactions (Blume et al., 2016).

Q7) 2. Materials and Methods
2.1 experimental set-up
Table 1: How were these soil properties measured? It would be useful to add a third column

briefly explaining those procedures or giving references for them. Alternatively, the caption or footnote could refer readers to Sections 2.3 and S3.7, where some of the measurements are discussed. The number of replicates contributing to the standard errors for each measurement should be included in this table. Also, please clarify that the percentages are mass (weight) percentages. Finally, it could be useful to see some information about the base saturation and Fe/Mn oxide content of this soil, if these data are available. At present, there is no way to assess the capacity of this soil to sorb cations to oxides.

Thank you for the helpful suggestions. We have added detailed explanations for each soil property measurement, either directly in the table footnote or by referencing the relevant literature. Specifically, we cite Brown (1943) for the determination of CEC and base saturation.

The number of replicates and the soil depths at which control soils were sampled have also been included to clarify the reported standard errors. We specify that all percentages are mass (weight) percentages (w%), except for base saturation, which is expressed as the percentage of exchangeable cation charges occupied by base cations and is not a mass percentage. We did not measure Mn oxides so we cannot add this information to Table 1. We did include Fe (hydr)oxide content for the control soil, estimated from sequential extraction data of the reducible pool in the 0–20 cm soil layer, to provide insight into the soil's capacity to sorb cations onto oxides.

Q8) 2.2 Leachate and pore water analysis

Line 181: Why is Na missing from the list of major cations measured? Na does not appear in the extraction schemes given in Table 3.

Thank you for pointing this out. At the time the experiment was conducted (2021), Na was not included in the soil water analyses and plants because its importance was not fully considered. We began measuring Na only later when analyzing soils . Therefore, Na was not measured in the soil water samples and plants. However, total alkalinity (TA) was measured in the soil water, which indirectly accounts for Na through charge balance. We have now added a clear statement in the Methods section to specify that Na was not measured in soil water samples.

Line 196: Na was not directly measured in soil water samples; however, its charge contribution is accounted for indirectly in the measured soil water alkalinity.

Line 477: Na was not analyzed in plants and is thus not included in the harvested base cations.

Line 259: Na was not measured in aboveground biomass, because the amount of weathered base cations accumulated in plants is relatively small compared to that in soils, we do not expect this omission to substantially affect our results.

See also this discussion that was already there in the previous manuscript version: Line 315: Na was not quantified at the time of plant biomass elemental analysis, which may lead to an underestimation of the alkalinity equivalent increase in the plant pool. However, given that base cation charges in the plant pool were about two orders of magnitude smaller than in the soil pool, we expect the effect of this omission to be limited. In addition, maize plants aim to actively increase their K/Na ratio which avoids salt stress, the K content of maize shoots is typically about 2 orders of magnitude larger than Na (Gao et al., 2016; Suarez & Grieve, 1988).

Q9) Line 186: The usual name of Si in English is "silicon" rather than "silicium". As you have used "sodium" rather than "natrium" for Na, and "potassium" rather than "kalium" for K, it seems consistent to use "silicon" here.

Indeed, thank you for noticing, silicium was replaced by silicon.

Q10) 2.3 soil collection and pretreatment: How many soil samples contributed to the standard error calculations?

One sample was sampled for each depth and mesocosm. Throughout the manuscript if there are standard errors on measurements / error propagation we reported the way of calculating the error in figure captions as requested in the previous review round. We now also added details for the amount of samples analyzed in Table 1 for control soil characteristics as requested in Q7.

Q11) 2.4 Sequential base cation extractions Line 204: To the best of my knowledge, the carboxyl groups of SOM are indeed extracted by NH4Ac (exchangeable pool). However, line 105 states that the SOM carboxylic and phenolic groups are part of the "oxidizable" pool. Please address this inconsistency here as well as in the Introduction.

We adapted the introduction regarding this point (see also Q6). As explained in Q6, we emphasize the difference in binding strength rather than the exact functional group of the SOM. See the rephrased sentences in line 221: As conceptualized by Tessier et al. (1979), base cations can reside in four different soil pools: the exchangeable pool (cations weakly bound to SOM or clays), the carbonate pool (cations bound in pedogenic carbonates), the reducible pool (cations bound to Al/Mn/Fe hydr(oxide)) and the oxidizable pool (cations strongly bound to SOM).

Q12) 2.5 Plant responses
Was there a reason why roots were not included in the biomass measurements? Are roots treated as part of the SOM? Live root sorption properties may differ considerably from those of SOM!

We focused on aboveground biomass because it represents the harvested fraction and primary pathway for cation export. Root biomass and nutrient contents were measured and published in Rijnders et al. (2025; see line 256). Soil samples were sieved at 2 mm, removing most root biomass.

Rijnders et al. (2025) reported additional root Mg and Ca concentrations of ~0.75 mg Mg/g root biomass and ~1 mg Ca/g root biomass. With an estimated 56 g additional root biomass per mesocosm (Rijnders et al. (2025), this corresponds to ~6 meq cations per mesocosm, or 0.033 μmol equivalent TA/kg soil. These amounts are negligible compared to the changes in the soil that are several tens of μmol equivalent TA/kg soil (Figure 5).

We added in line 256: For the results on root biomass we refer to Rijnders et al. (2025)).

And in line 218: Estimated base cations in sieved-off roots were negligible relative to soil pools and thus excluded from our main cation budget.

Q13) 2.6 Calculation of Wr and potential CO2 removal
Here, it is assumed that the conservative anion content of the unamended and amended soils is the same because the basalt contained no such anions. However, nitrogen cycling (nitrification and denitrification) can be sensitive to soil chemistry, and could therefore differ between treated and control mesocosms. It may be worth stating that such possible effects are neglected here. I assume that the P content of the basalt is neglected here because phosphoric acid is a weak

acid and would not appear in the TA equation based on conservative cations and anions.

Thank you for this insightful comment regarding nitrogen cycling. Phosphate is also a conservative anion that could influence alkalinity if its levels differ between control and treatment. We have therefore added a the following text in line 269:

We thus explicitly assume no changes in conservative anions after basalt amendment. Still, nitrogen cycling processes (nitrification and denitrification) may be sensitive to soil chemistry and may therefore differ between treatments. Further research is needed to verify whether conservative anions such as nitrates and phosphates are affected by rock amendment.

Q14) Line 310: There is an assumption of no physical transport of basalt particles here. Please acknowledge this possible limitation of the analysis.

Thank you for observing this limitation. If basalt would be physically transported deeper, Equation 7 corrects too much for cations added through feedstock in the top 20 cm and we would overestimate cations in the deeper layer (below the amendment where no correction occurs) , we added this reflection in Line 340:

A limitation of the approach in Equation 7 is that it assumes no physical transport of basalt to deeper soil layers, which may lead to underestimating weathered base cations in the 0–20 cm layer and overestimating them in the 20–30 cm layer.

Q15) 2.8 Data analysis
The differences (delta TA) are calculated by subtracting the mean of the five controls for all treatments, including the single-replicate treatments of Table 2. Is this statistically robust? Perhaps a reference to the statistical literature is appropriate here?

Although several application rates were not replicated, the slope-based approach uses the full gradient of treatments and therefore integrates information across all application levels. Replicated control and 50 t ha^{-1} treatments provide an estimate of variability used in the propagated errors.

Subtracting the control mean (a form of blank normalization) shifts the data vertically (seeFigures 3 and S22), but does not change the slope of the relationship between basalt addition and the response variable. Since our analysis focuses on the slope, this normalization does not affect the resulting weathering rate estimates. The statistical book of Gelman and Hill (2007) shows that subtracting a constant shifts the intercept but leaves slope estimates unchanged, preserving interpretability and inference. We added this reference clarify this in line 305: Normalizing for mean control alkalinity equivalents before regression shifts the intercepts in Fig. S22 but does not affect the slope, preserving the relationship between basalt addition and the response variable (Gelman & Hill, 2007).

We also performed a sensitivity analysis of our slope regression method (see Fig. S13), where individual application rates were normalized against control soils, incorporating control variability through error propagation. The resulting weathering rates for individual applications were similar/even higher than the aggregated slope-based rate (Log Wr = –11) reported in the main manuscript, supporting the conservativeness of the slope approach.

Q16) Discussion: Line 559: Smectites and montmorillonites can also contain base cations; you may wish to mention them here.

We added these clays as examples of base cation bearing secondary clay minerals.