Point by point responses to reviewer comments can be found below.
Key changes include:

- Addition of Figure 2
- Correction of the surface-area normalized weathering rate
- Refining the discussion to more appropriately reflect the strength and limitations of the evidence

Response to RC1:

We thank the reviewer for their careful comments, which we are certain have greatly improved the final article.

Responses to reviewer comments are in italics:

This study evaluates enhanced rock weathering using Greenlandic glacial rock flour applied to sandy agricultural soil in Denmark over five years. The research topic is timely and of interest to the EGUsphere reader community. The work is an extension and re-evaluation of work performed previously by the authors where they used an updated SOMBA framework to estimate ERW and CDR. While I agree that that the long-term 5-year evaluation is powerful, my impression is that this manuscript could be expanded and improved. Currently, its added benefit does not meet the expectations.

General comments:

1. The introduction may be written more concisely. There are several repetitions – for example on the surface area of GRF, and the fact that the data of the previous 3-yr study was re-evaluated in this study… I suggest re-organizing the paragraphs so that the introduction flows better and has less redundancies.

We thank the reviewer for this suggestion. We have revised the Introduction to improve concision and reduce repetition. In particular, we shortened the discussion of the limited availability of long-term field data, condensed the description of the previous approximately three-year assessment and its methodological limitations, and removed repeated discussion of the high surface area of GRF by combining the relevant text on its potential role in organo-mineral SOC stabilization. These changes retain the motivation for the present study while improving the overall flow of the Introduction.

2. I am wondering regarding the use of sodium hexametaphosphate in the soil OM fractionation protocol – could the hexametaphosphate alter the native OM–mineral association? Various phosphates are commonly used to displace adsorbed OM due to their high affinity to mineral surfaces, so could the hexametaphosphate replace some of the MAOM?

We thank the reviewer for this important comment. Sodium hexametaphosphate was used as a dispersing agent following established POM-MAOM fractionation protocols, with only minor adaptations to improve recovery of mineral material associated with coarse organic matter. We

acknowledge that phosphate-containing dispersants may potentially compete with organic matter for mineral sorption sites and alter some native OM-mineral associations. However, all material passing through the 53 μm sieve, including all sieving and rinsing solutions, was retained and recovered as part of the MAOM fraction. Therefore, any organic matter potentially displaced from mineral surfaces during dispersion would still have been included in the quantified MAOM pool rather than lost during fractionation. In addition, the MAOM mass was corrected for the contribution of sodium hexametaphosphate added during dispersion. To clarify this point, we revised the Methods section to explicitly state that all sieving and rinsing solutions were collected and included in the MAOM fraction.

3. The Discussion section seems slightly weak, and does not add significant insights to the results written before. For example, I do not see how the paragraph starting in line 482 adds anything that was not mentioned beforehand. Also, in the paragraph starting in line 519, the authors attribute the SOC gain to the MAOM pool, yet there were no statistical differences for the latter. This is missing a justification/explanation.

We thank the reviewer for this comment and agree that the previous Discussion did not sufficiently develop the broader implications of the results. We have substantially revised and expanded the Discussion throughout. In particular, we now more explicitly discuss why the five-year SOMBA estimate differs from the earlier exchangeable-cation-based estimate, including the potential importance of seasonal leaching and the value of time-integrated approaches for MRV. We have also expanded the discussion of how background soil variability affects the ability of different cations to constrain weathering and why Mg provides the most robust constraint at this site. We have additionally expanded the discussion of the fate of weathering-derived alkalinity beyond the sampled topsoil. The revised manuscript now distinguishes initial CO2 uptake under soil-solution conditions from the potential long-term atmospheric removal following downstream transport and ocean equilibration, and explicitly discusses uncertainty associated with hydrological transport, cation exchange and neutralization of deeper soil acidity, possible attenuation in acidic subsoils, and subsequent transport through groundwater and surface waters. We also emphasize that the ocean-equilibrated estimate is conditional on successful alkalinity export and that resolving these downstream losses and time lags will require deeper-soil and groundwater measurements together with coupled hydrological and reactive-transport modelling, and explore the implications of two recent studies in this context.

Regarding the SOC response, we agree that the previous wording attributed the effect too strongly to MAOM. Total SOC stocks were significantly higher in GRF-treated plots, whereas treatment effects on MAOM and POM stocks were not statistically significant when considered individually. The numerical treatment difference was larger for MAOM (+2.89 t C ha^{-1}) than for POM (+0.50 t C ha^{-1}), suggesting this is the primary driver of the observed effect. We have revised the Abstract, Discussion, and Conclusion to distinguish the statistically significant total SOC response from the fraction-specific observations and now present enhanced mineral-associated stabilization only as a possible mechanism consistent with the observed pattern. We have also expanded the broader discussion of potential temporal changes in SOC following mineral amendments and the uncertainties associated with interpreting the observed endpoint SOC

Throughout the paper, there are inconsistencies in formatting, capitalization of titles, subscripts (e.g., Table 1), difference in spacing (e.g., Table 2) and missing acronyms (e.g., line 143).

Thank you for identifying these errors, we have corrected them and defined ICP-OES.

4. I am curious regarding the Si levels and trends in the experiments? Si dissolution is an important part of the ERW process, so why was this not even mentioned by the authors?

We agree that Si can provide a useful complementary constraint on silicate dissolution and subsequent secondary-phase formation. Dissolved Si concentrations or fluxes were not measured during this field experiment, so we unfortunately cannot reconstruct temporal Si release from the present dataset. The present analysis was designed around solid-phase mass balance of base cations relative to an immobile tracer because these cations directly constrain the alkalinity-generating component of weathering.

Specific comments:

Lines 51-52 – I suggest adding here the actual particle size and surface area of the GRF.

In this paragraph we are discussing GRF generally- exact particle sizes and surface areas will vary between GRF deposits, so we leave the specifics of the feedstock used in this experiment in the methods.

Lines 52-53 – Is there literature or other reports confirming these claims?

We have added Sarkar (2021) to support the statements regarding the physical characteristics of Greenlandic glacial rock flour. The subsequent statements regarding its potential for carbon capture and crop-yield improvement are also now supported by relevant field studies.

Line 115 – Please add the exact texture as well.

We have clarified the soil texture in Section 2.1 and now explicitly describe the site soils as having a loamy sand texture.

Line 125 – It would be helpful to show the particle distribution and not only report the d_{50}.

We agree that the full particle-size distribution provides useful additional characterization. The particle-size distribution of this GRF material has been reported previously in Sarkar (2021). We therefore retain the d50 value in the Methods and now explicitly direct readers to Sarkar (2021) for the full particle-size distribution and additional feedstock characterization.

Line 143 – Please spell out ICP-OES and add details of the instrument that was used.

We have now spelled out ICP-OES as inductively coupled plasma optical emission spectroscopy and added the instrument details (iCAP PRO, Thermo Scientific) in Section 2.4.

Table 2 title – What is the time of sampling for Ti and mobile cations? Perhaps specify directly?

We have revised the Table 2 caption to specify that Ti and mobile cation concentrations were measured at the time of sampling, approximately five years after GRF application.

Lines 352-355 – To be clear, maybe first spell our what SD and CI represent?

*We have now defined SD as standard deviation and CI as confidence interval in the Methods when the uncertainty analysis is introduced.*Figure 1 – Missing what panels (a) and (b) are?

We have revised the Figure 1 caption to explicitly identify panel (a) as the bootstrap distribution of the dissolution fraction (τ) and panel (b) as the inferred GRF application rate.

Lines 372-375 – The authors suddenly mention BET analysis measured over the 5-year period. There is no mention of this in the Materials and Methods, and it is unclear to me which samples exactly were measured? How were the samples prepped? Which instrument was used? Where are the results presented? This raises uncertainty as to the calculated surface-area-normalized rate of total alkalinity release.

We acknowledge that this sentence is unclear and will revise it. The BET surface area refers to the surface area of the GRF feedstock, as measured in Sarkar (2021). This has been clarified in the main text.

Table 5 – The title does not describe what the Table actually presents.

We have revised the Table 5 caption to clarify that the table presents soil pH, bulk density, and bulk and fractionated SOC concentrations and stocks for control and GRF-treated plots, together with treatment contrasts and p values.

Line 425-426 – Looking at the cited papers, it seems that the surface-area-normalized weathering rates reported for acidic conditions are within a wider range, including faster rates similar to those reported in this work.

We thank the reviewer for prompting us to revisit this comparison. In doing so, we identified an error in the originally reported surface-area-normalized weathering rate. The corrected five-year rate is approximately 1.06×10^{-12} mol m^{-2} s^{-1} (log Wr = −11.97), rather than 10^{-9} mol m^{-2} s^{-1}. The corrected value falls within the range reported in the cited soil incubation and mesocosm studies. We have corrected the value in the Results and revised the Discussion accordingly; we no longer argue that the field-derived rate exceeds these experimental rates.

Lines 535-536 – Are there studies supporting this?

Thanks, we have added Schjønning et al. (2018) and Tiessen et al. (1994) to support the statement regarding the potential benefits of increased SOC for soil structure, nutrient retention, and water-holding capacity.

Schjonning P, Jensen JL, Bruun S, Jensen LS, Christensen BT, Munkholm LJ, Oelofse M, Baby S, Knudsen L (2018) Chapter two – the role of soil organic matter for maintaining crop yields:

evidence for a renewed conceptual basis. In 'Advances in agronomy'. (Ed. DL Sparks) pp. 35–79. (Elsevier Inc.)

Tiessen, H., Cuevas, E., Chacon, P., 1994. The role of soil organic matter in sustaining soil fertility. Nature 371, 783–785.

We thank the reviewer for their time, effort, and suggestions which we feel have clearly improved the quality of the manuscript.

Response to RC2:

We thank the reviewer for their positive feedback and helpful comments, which we believe have significantly improved the revision of this paper.

Our responses to comments are in italics below:

General comments:

The paper contributes new, promising findings and methodological frameworks, specifically SOMBA, to account for Enhanced rock Weathering (ERW) rate estimates. I believe this to be an exciting and significant contribution to understanding effective carbon dioxide removal strategies (CDR), given that this was one of the few long-term field studies of five years duration. It also took into consideration both organic and inorganic carbon components. The improved and reevaluated methodology adds value and validity. This looks promising for scaling of new technologies for carbon mitigation in temperate climate agricultural settings.

The methods section is very detailed and long. Given many technical components of data analyses, some ideas could be elaborated, and other sections could be shortened to reduce the length of the methods. Some table captions in the results need re-evaluation. Results can be enhanced with more visual figures. The importance of temporal data assessment of cumulative responses of dissolution and weathering rates are mentioned in different parts of the manuscript, so a figure could be added as a visual graph to demonstrate this and add a clear message in the Results.

We agree that visualizing a temporal weathering trajectory would be valuable if directly comparable measurements were available. However, after reconsidering this suggestion, we decided not to add a quantitative time-series figure because the earlier ~2.5-year estimate and the present five-year estimate were obtained using fundamentally different approaches. The earlier estimate was based on the transient accumulation of exchangeable Mg, whereas the present SOMBA estimate quantifies time-integrated depletion of feedstock-derived cations from

the bulk soil inventory. Plotting these values as successive points on a single dissolution trajectory would therefore imply direct comparability that the measurements do not support. Instead, we have expanded Section 4.2 to explain this methodological distinction and its implications for interpreting the apparent change in cumulative weathering. Relatedly, we have nevertheless added Figure 2, to demonstrate the temporal evolution of the acidity loss fraction over time / system compartments.

Specific comments:

1. Line 143, Section 2.4: what is ICP-OES? It wasn't mentioned before

Thanks for the suggestion, we have added the definition to the text.

2. Line 200, Section 2.6 (or following related sections) could use more explanation on what the different linear mixed effects model designs were. For e.g. In Line 201, name or list the metrics/ parameters for the "measured parameters". Additionally, could show the model equations too.

We have expanded Sections 2.6 and 2.7 to specify the parameters analyzed and clarify the two types of fixed-effect comparisons.

Line 275: Briefly define and fully name "pCO2"

We have now defined pCO2 as the partial pressure of CO2 at its first occurrence.

3. Line 404: Section 3.5 Results can be expanded. What important implications are construed when the C assessment results are different for "bulk carbon concentrations" vs. "SOC stocks"?

We have expanded the first paragraph of Section 4.3 to clarify the distinction between SOC concentration and SOC stocks. SOC concentration describes carbon per unit soil mass, whereas SOC stocks additionally incorporate the amount of soil within the sampled volume. Although neither SOC concentration nor bulk density showed a significant treatment effect independently, the calculated SOC stocks showed a significant treatment contrast. We discuss the implications of this result in the Discussion rather than expanding the Results beyond reporting the statistical outcomes.

4. Line 457: The language here can be made more accessible for general and interdisciplinary audience by emphasizing how "robust constrains of dissolution" is meaningful, why the different cations matter and eventually connected to weathering. If such attempts were made to explain the concept in previous sections, it can be reiterated in this discussion section for immediacy and clarity.

Thank you for this suggestion. We have clarified the meaning of a "robust constraint" on dissolution in Section 4.2. Specifically, a cation provides a useful constraint when its depletion relative to the predicted no-weathering concentration can be resolved against natural background variability. We then explain why Mg provides the strongest constraint at this site,

whereas Ca, Na, and K are less well resolved because of their larger and more heterogeneous background soil pools.

Technical corrections:

- Line 347: Table 3 description mentions “Ti” but is not actually listed in the table.

We have corrected the Table 3 caption to remove the reference to Ti.

- Line 390: Table 5 description could state or spell out what the three different variables pH, %C and C(t/ha) exactly are in brackets.

These variables have now been defined in the caption.

- Line 412: “Mean SOC stocks increased from 45.3 t C ha-1 in control plots to 50.3 t C ha-1 in treated plots,”
    - Consider restructuring the sentence to say that treated plots had higher mean SOC stocks than control, not that SOC increased from… to …. This makes it sounds like the control and treated plots are compared to each other as before and after

We have revised the sentence accordingly to describe the result as a treatment difference rather than implying a temporal increase.

- Line 457: “these” should be replaced with “the” as it is the first sentence of a new paragraph

Done.

We thank the reviewer for their time, effort, and suggestions which we feel have clearly improved the quality of the manuscript.
