We thank the reviewer for their careful comments, which we are certain will greatly improve 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.

Thank you for identifying these issues- we will strive to correct them in the revised manuscript.

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 will revise 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.

Given recent evidence (https://arxiv.org/abs/2607.01835) that time lags associated with export of carbonate alkalinity from sandy Danish soils may not be trivial, we intend to revise the paragraph starting in line 482, which should address the first part of this comment.

In the paragraph beginning in line 519, we will make more clear that though the increase in MAOM was not statistically significant (due to higher variability), the scale of the observed increase in the MAOM pool relative to the POM pool implies that the observed significant increase in SOC overall must be attributed to the MAOM pool.

4. 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 will correct them in the revision.

5. 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?

Si release is not typically analyzed in ERW experiments as it does not contribute to CDR.

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 will add this citation: Sarkar, S.R., 2021. Glacial Rock Flour: its Characteristics and Enhanced Weathering (PhD Thesis). University of Copenhagen.

Line 115 – Please add the exact texture as well.

The texture is "Loamy sand", as indicated.

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

Readers can refer to the cited Sarkar (2021) for more information about the material used, including particle size distribution.

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

We will define this acronym in the revision.

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

We will clarify this in the revision.

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

We will clarify this in the revision.

Figure 1 – Missing what panels (a) and (b) are?

We will clarify this in the revision.

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).

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

We will clarify this in the revision.

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 reviewed the papers cited here and could not find values faster than the currently stated range.

Lines 535-536 – Are there studies supporting this?

We will add these citations:

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.