Response letter; Editor/reviewer comments in blue, replies in black.

Dear Arthur Vienne & co-authors

thank you very much for the revised manuscript. We have now revised back the reports from one original and two new reviewers on your revisions/reply. One of the new referee suggest to include back again SCE. I do believe the majority of referees made a good argument against the inclusion of the SCE part.

I invite you to reply to the comments of all reviewers & provide revised manuscript.

Best wishes

Daniel Goll

Dear editor,

Please find below our answers to all the questions of the three reviewers. As the majority of reviewers made a good argument against inclusion of SCE, SCE was left out of the manuscript also in this revised manuscript.

We sincerely hope that these revisions adequately address the reviewers' questions and meet the journal's requirements for publication.

Kind regards,

Arthur

Anonymous Referee #2

Overall the authors have provided an improved version of the manuscript and I have no major concerns at this stage. However, after a second reading, I would still suggest that two points be addressed before the final publication.

1) The limitations of the sampling methodology should be more clearly contextualized. Sampling only the 0 to 5 cm layer and performing cation measurements only twice (day 0 and day 550), combined with the use of a leaching test on the same 0 to 5 cm layer at day 550, is not an optimal strategy for detecting weathering signals. It is plausible that the absence of detectable treatment effects on cations is due to vertical percolation of dissolved elements beyond the sampled depth during the 550 days of the experiment. The explanation currently provided in lines 407 to 409 is therefore only one possible interpretation. The authors should acknowledge that a different sampling strategy, including deeper soil layers or more frequent temporal sampling, may be required to identify these effects.

We added a limitations section and added this; Line 440: This study sampled only the 0–5 cm soil layer and measured cations at two time points (day 0 and day 550), with leaching tests performed solely at day 550. Such a restricted temporal and vertical sampling scheme may have limited detection of treatment effects, as dissolved elements could have percolated below the sampled layer over the course of the experiment. Future studies incorporating deeper soil horizons and more frequent measurements would provide a more robust assessment of treatment-induced changes in soil cation dynamics.

2) The manuscript does not report any information on SOC or SOM changes in the results section, despite having described the analytical methodology. It would be very valuable to know whether the treatments influenced SOC or SOM, and if available, these results should be included.

SOM was measured through LOI and was shown in the supplement, but not explicitly referred to in the main text. We add the following text to the main manuscript and also nuance the limitation that measuring SOC was only measured for a relatively short timeframe (<2 years) ;

Line 446: Last, the absence of observation of significant changes in SOC (**Fig. S11**) may result from the relatively short experimental duration of this study.

Anonymous referee #4

3) Further discussion about why there are seasonal differences in the mustard yield results would be beneficial- particularly **why the combined treatment is higher than biochar or basalt alone in the first season, and why that changed in the second harvest.** Overall yield levels don't seem to vary between the two seasons so a seasonal effect seems unlikely.

Interesting comment, one option that could explain the higher yield for mustard in the combined basalt-biochar treatment relative to the biochar only treatment in the first harvest but not in the second harvest could be a short-term nutrient retention of basalt-released nutrients by biochar. By the second harvest, these nutrients may have been depleted or leached, reducing the synergy effect. However, this is a speculative explanation and we therefore whish to leave this interpretation out.

4) It needs to be specified which harvest the plant elemental concentrations were measured on, it seems to only be for one?

We added these details. Line 210: Plant elemental concentrations were determined in the first crop harvest, six months after sowing. The growing period for elemental biomass analysis was thus November 2023-May 2024 for mustard and summer harvest May 2023-November 2023 for clover.

5) I think the values for SOC and organic matter are switched in table 2 (OM should be higher than SOC)

Thank you for noticing this mistake, the values for SOC and SOM indeed seem to have been switched by the external lab that characterized the Belfast site. Luckily we also measured SOM ourselves in our experimental plots through loss on ignition, our own measured SOM was very close to the value that was in the table for SOC previously.

I replaced the values in Table 2 with values from the initial control soil we measured ourselves (see also Fig. S11).

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6) The clover trial was setup as a randomized block design and could have been analyzed as such, possibly with better results, but it may not make much difference

This comment was not entirely clear to us, but I think that the reviewer is referring to a statistical analysis where the plot code is added as a random effect. We added plot codes as random effects in every statistical analysis where measurements were repeatedly measured in time.

Reviewer 3: M.E Vorrath

Dear authors of Vienne et al. 2025, I recommend this study for publication after a major revision to implement the SCE again and consider Vorrath et al. (2025) in the introduction and for a more in-depth discussion. 7) I enjoyed reading this important and well-written study. Although the experimental design was not ideal for quantifying CDR I think it still can gain a lot of insights on the combination of EW and biochar. This combination will become more and more important in CDR, heading towards “stacked CDR” to increase CDR per area and aim for multiple co-benefits. I know, it is a very shady move to recommend your own study as a referee/reviewer, but I think the introduction and discussion will benefit a lot if the authors consider my publication Vorrath et al. (2025, doi: https://doi.org/10.3389/fclim.2025.1592454 , for further understanding you can also look at doi: https://doi.org/10.3389/fclim.2025.1631368). This study will support several findings (e.g. accelerated weathering was not observed but synergistic effects are present) and can give more food for thought for the discussion. I also invite the author(s) to a videocall for a deeper discussion on this topic.

Thank you for the invitation for the positive assessment of our manuscript, the interesting literature which we have incorporated in the manuscript and invitation for the videocall. We are happy to discuss biochar-EW interactions further in a meeting, u can reach out to me for a meeting at arthur.vienne@uantwerpen.be.

8) I agree with reviewer 1 that the results of SCE are very interesting but must be discussed more in depth. I agree with reviewer 2 that the quantification of CDR is missing. An estimation of CDR based on the measured parameters and comparison to SCE would provide a more holistic picture of the cycling of inorganic and organic C. I strongly, strongly recommend to NOT delete the SCE part from this study. I think, the SCE is the most important outcome of this study as it may show that the addition of biochar to EW may significantly mitigate SCE! Further, SCE from EW is still lacking a lot of data and each published experimental data will contribute to the overall understanding.

Unfortunately, our SCE data could not be trusted, as the majority of reviewers made a good argument against the inclusion of the SCE part (the risk of saturation when CO_2 is measured too long in a closed chamber, making interpretation impossible). Therefore we remain the decision to not include SCE in the manuscript. Also the interpretation of SCE data in planted fields is very complex (see e.g. Boito et al. (2025), doi: 10.1111/gcb.70373) and without a proper partitioning of CO2 from rhizosphere respiration and SOM decomposition, it is hard to draw conclusions about the fate of SOC.

In the current manuscript, the discussion of SCE is thus out of scope for this manuscript, but could definitely be an interesting follow-up study, that would require a distinct set-up similar as to in Boito et al. (2025) to test for positive/negative priming effects and basalt/biochar interaction effects on SOC. See also Q23.

Here are several comments on specific part of the manuscript that need clarification/revision: 9) L. 64-69: This part should consider Vorrath et al. 2025. Increased weathering rate can also be expected from improved hydrology a) when biochar allows water to drain in a water-logging soil and facilitates the export of weathering products from minerals or b) when biochar allows a higher water holding capacity and slows down the water drainage in a sandy soil. The soil used in this study is very sandy and could likely benefit from b).

Interesting mechanism! We added this in Line 67: Biochar-induced hydrological changes may also enhance weathering. In sandy soils (like will be used in our study, see further) increased water-holding capacity could prolong water–mineral contact and thus promote weathering (Vorrath et al., 2025).

We however did not observe a significant biochar effect on soil moisture (see Q24).

10) L. 112: Please add from what type of biomass remains the digestate used as biochar feedstock.

We added this in line 114: To produce this digestate, 20m^3 of dairy cattle slurry and 4t of perennial ryegrass silage were fed daily to a continuously stirred tank reactor anaerobic digestion facility consisting of a 650 m^3 main digestion tank, heated to 39°C, with a hydraulic retention time of 28 days for biogas production. Biochar was produced from mechanically separated (Screw Press) anaerobic digestate solid fraction.

11) L114-116: Please add the cation exchange capacity of the soil. I couldn't find it in the data or text.

We measured only exchangeable bases, but at this high soil pH, exchangeable acidity will be minor so this is a good proxy for CEC in this soil. I added the exchangeable bases in meQ/100g to **Table 2**.

Exchangeable bases were 7.72±0.47 meq/100g.

12) L. 116: The SOC content of the soil is quite high with 6.38%. This must be discussed regarding CO2 efflux. To avoid SCE from EW you would rather chose soils with lower SOC (see also Cascade Climate).

Thank you for this remark, this was a mistake in Table 2;

It was actually 3.7% (see Q5), I expect that the external lab switched SOC and SOM in the earlier version of Table 2. We adapted it with our own measurements now. Still, a relatively high SOC%, indeed good to keep in mind that low-SOC soils should be chosen for EW to avoid potential SOC losses!

13) Table 1: It is important to also show the H/Corg ratio, as this is a key parameter for the grade of pyrolysis, quality and durability of the biochar. Parameters of stability of pyrogenic carbon (random reflectance, hydropyrolysis, etc.) should be included if available.

H/Corg ratio was 0.11; O/C molar ratio was 0.011 – both on a dry basis. We don't have anything on the pyrogenic carbon stability however, remember the pyrolysis process was ~650oC and together with these ratios is indicative of a stable biochar (but we agree it is not a perfect proxy for stability, see Petersen 2025) https://onlinelibrary.wiley.com/doi/epdf/10.1111/gcbb.70049, no random reflectance data was gathered.

We added the H/C ratios in line 121: The biochar had a H/Corg ratio of 0.11 and an O/C molar ratio of 0.011.

14) L.198-204: I am not in favour of using a linear model here as observations often show an exponential behaviour of change in weathering rates, cation leaching etc in the initial phase of ERW experiments. Please explain why you chose the linear model above other approaches. Figure 3: **The abbreviations in the legend for basalt and biochar, B and Bi, are confusing. It would help to have something like "B"** and "BC" or "**Bio"** or "Py" (for pyrogenic carbon).

Thanks for the suggestion, I changed the caption of biochar from "Bi" to "Bio" in all figures.

15) L. 305-: How do the authors explain the possibility that no weathering of the basalt occurred by the soil CO_2 efflux showed a clear sign of a change in the soil?

To get an impression of how fast the released cations could have redistributed to the lower soil, the knowledge of the CEC is crucial.

Low-CEC soils show a fast leaching of released cations from weathering which likely would also happen in the soil here.

With the presence of 6% olivine and the obvious strong change in SOC weathering is very likely to have happened and cations were likely already leached out of the samples soil horizon. (besides the explanation of clay formation).

We do not think the SCE changes among treatment are reliable (see Q8) and therefore do not whish to discuss them. I added the CEC (7 meq/100g) of the control soil, which is in the typical range for a sandy loam soil (5-10 meq/100g).

For comparison, we see in other experiments open to Belgian rainfall with a large application rate of basalt~10cm on top of a layer of sand of ~50cm(sand with a CEC of 1 meq/100g) that leachate pH does not increase within three months (but temporarily decreases, likely due to the exchange of base cations with acid cations (protons, Fe, Al). It's thus likely that exchange in the top soil will drastically retard CDR. See also the data in this preprint (https://egusphere.copernicus.org/preprints/2025/egusphere-2025-1667/) where we could not see increases in DIC or TA leaching after ~ 100 days in a soil with a CEC of 3 meq/100g for 10-200 t basalt ha-1.

We did not observe a significant SOC change, but I agree it is very likely that some of the olivine has weathered given its fast weathering rate.

We commented that we may have missed a peak of cation leaching in between the sampled dates (day 0 and 550) where we measured cations in leachates. This is incorporated into a limitation section:

Line 440: This study sampled only the 0–5 cm soil layer and measured cations at two time points (day 0 and day 550), with leaching tests performed solely at day 550. Such a restricted temporal and vertical sampling scheme may have limited detection of treatment effects, as dissolved elements could have percolated below the sampled layer over the course of the experiment. Future studies incorporating deeper soil horizons and more frequent measurements would provide a more robust assessment of treatment-induced changes in soil cation dynamics.

16) L. 352: And as also found by Vorrath et al. 2025.

Adapted line 365: The latter increase in carbonate base cations is consistent with previous findings, as biochar is known to increase base cation exchange, contains carbonate minerals and promotes the formation of SIC (Amann & Hartmann, 2019; Dong et al., 2019; Vorrath et al., 2025).

17) L. 356-359: Please consider Vorrath et al. 2025.

We added in line 383: In addition, more research should be done to assess how co-pyrolysis of basalt and biochar (Vorrath et al., 2025) influences inorganic CO2 removal and weathering products in the soil.

18) L. 364-373: For this part is it crucial to know in which form the biochar was added to the soil. When pure biochar is added to soil it is very likely that it absorbs nutrients which can lead to a deficiency of nutrients and a failure of plant growth. It is common to charge biochar with nutrients (manure, compost, liquid fertilizer etc.) to prevent this. Therefore, it is important to know the pre-treatment of biochar and, if not charged with nutrients, discuss the potential of biochar to absorb cations from the soil as well as from cations released from mineral weathering.If pure biochar is added to a soil but plant growth is still increased the effect of nutrient scavenging of the biochar is lower than the effect of the added nutrients in the biochar as you found that P2O5 in the biochar might have been the reason for increased biomass.

Due to the original biomass material (a slurry / silage mix resulting in a low %C, high mineral biochar) the digestate and the resulting biochar is already 'preloaded' with nutrients and will add nutrients to the soil rather than removing them. There is no need for it to be charged.

This was clarified in line 121: The utilized slurry / silage mix resulted in a biochar relatively low in C but high in minerals; Both the digestate and resulting biochar were therefore already nutrient-loaded and would supply nutrients to the soil rather than removing them, reducing the need for additional biochar nutrient charging.

19) On the deleted SCE chapter 4.4: I did not fully understand why congruent weathering lead to a higher SOM decomposition. Half a sentence on the mechanism working here would help a lot.

The SCE section was thus removed from the paper and is thus not of importance anymore for this version, for your information I add the theory of this weathering congruency and priming here:

This mechanism is explained in detail here (Doi: 10.1038/s41467-022-35671-x , Fang et al. (2023)). In Figure 1 of that paper, u can see how with a high congruency of weathering, base cations in solution increase, strip organic matter to dissolve, leading to priming and higher CO_2 efflux. If congruency is low, the DOC stays sorbed on secondary minerals, making it unavailable for microbes and priming, with lower CO_2 effluxes as a result.

Our hypothesis was thus that biochar sorbs the base cations and DOC and thus prevents positive priming through a similar mechanism as visualized in Figure 1 of Fang et al. (2023). Lu et al. (2014) for example found negative priming with biochar. These are interesting mechanisms, but we can not include anything about biochar priming effects based on the dataset of this manuscript.

20) Biochar alone did not lead to very high SCE, this is very unexpected. Many studies determine the stability of biochar by CO_2 outgassing as this is the main pathway how the labile part of the biochar is released. The release of CO_2 in the first few months/years is the highest, so you would expect to see this in the SCE measurements. However, the SCE is much lower than from the basalt amendment, so either the biochar contained very small amounts of labile pyrogenic carbon, the remineralization effect of basalt is much stronger than biochar remineralization or biochar promoted a reduced SCE in the soil itself which counterbalances the release of CO_2 from the labile biochar part. Additionally, the biochar amendments led to a higher biomass and likely root respiration activity which would also lead to higher SCE. Observing, that SCE was so low may be a sign of the high potential of biochar to reduced SCE.

As mentioned earlier, we think that these SCE results are left out of the manuscript.

If the SCE measurements in the previous version would be reliable and only reflect SOM decomposition changes, then indeed, I would think there should be little labile pyrogenic C, and the negative priming of the biochar compensated for the small amount of pyrogenic C respiration.

21) How do you explain your anticipation that biochar would reduce SCE from enhancing the inorganic CDR from basalt? Do you mean that CO_2 released (from biochar or mineral weathering) would have been turned into DIC directly in the soil because weathering itself was accelerated?

The initial hypothesis was indeed that CO_2 released from SOM/biochar respiration in the soil would be turned into DIC because of accelerated weathering. Now we know that this is however an oversimplification as also organic C can be affected by EW.

22) VERY IMPORTANT: The observation that the SCE in the BBi treatment is not as high as the sum of B and Bi is again a hint that biochar can mitigate SCE evoked by basalt because it has a high potential to diminish SCE. This is a very strong and important finding to a) continue to study this effect to understand the mechanisms and b) to consider the combination of EW and biochar in agriculture for CDR and to mitigate CO_2 release through weathering. Such “stacked” CDR will

have several benefits: higher CDR rates per hectare, combining CDR that is immediate (biochar) with CDR that will progress over time (EW), establishing circularity by using increased biomass yields (and therefore increase waste biomass) to produce biochar, promoting soil restoration, health, and fertility by adding both organic and mineral nutrients and improve SOC, and by improving farmers economic outcome by having multiple revenue streams (CORCs from EW and biochar, higher harvest, healthy soils, reduced fertilizers). These points contribute to the scalability of CDR in agriculture and are therefore of very high interest when science is transferred into real-world application.

Thank you for this comment. As mentioned earlier, we think that these SCE results are unreliable and therefore do not whish to include them further in the manuscript. I do think these thoughts on biochar-basalt synergies and stacked CDR (increasing the CDR per area unit) are valuable. Also the concept to first use EW to remineralize degraded soils to kickstart biomass production after which this biomass could be used to make biochar and treat more degraded soils with biochar is intriguing.

We do think that these thoughts are a bit out of scope of the current focus of the manuscript. Still, If the editor so whishes we can integrate these ideas.

23) Conclusion: **The conclusion benefits from giving more specific ideas on how experiments with EW but also EW+biochar should be designed for a successful outcome**. I also think that the reduced SCE from the BBi treatment is a very important outcome and may not be missing here.

As mentioned in Q8, it is important if future studies monitor SCE, they should be able to partition it in rhizosphere respiration and SOM respiration, we added this methodological comment to the limitation section. We think it fits better there as we don't speak about SCE anymore in the conclusions.

Line 446: Last, The absence of observation of significant changes in SOC (**Fig. S11**) may result from the relatively short experimental duration of this study. Monitoring soil CO_2 efflux (SCE) can help overcome the difficulty of detecting short-term changes in SOC caused by spatial heterogeneity (Vienne et al., 2024). However, to draw mechanistic conclusions about SOC dynamics in planted soils, SCE measurements must be partitioned into SOM–derived respiration and rhizosphere respiration, which requires a specific experimental set-up (Boito et al., 2025).

24) Supplements: **In the plots in figure S1, S2, S3, S4, S5 and S6 the treatments are sorted in alphabetical order and the legend is missing.** For convenience it should be ordered like the plots in the main manuscript and each figure shall have a legend to be used individually. **In figure S10 "Bi" is missing in the legend**. Is this data missing? I don't see soil moisture discussed in the main manuscript. Was there no difference between the treatments? As biochar alters the soil hydrology it would be good to mention if a difference in the soil moisture was present or not. This could also contribute to the discussion if the hydrology/soil moisture change from biochar may accelerate mineral weathering or not.

The order of treatments in S14 is also in alphabetical order. Please change.

- All figures now contain the treatments in the right order and Bi was changed in “Bio” throughout the manuscript.
- We could not detect significant basalt and biochar effects on soil moisture in our experiment (Figure S10). We now also refer to this in the main text.

  We added this in line 371: We hypothesized that biochar would increase basalt weathering through retention of base cations in the exchangeable pool or through increased water–mineral contact (yet we did not observe a significant biochar effect on soil moisture, Fig. S10).