Reviewer #1

This is an interesting experimental study on the effect of ERW using basalt, especially since it employed macrocosms that were monitored over time. This enabled the authors to determine the inorganic and organic budgets directly from the measurements, as opposed to modelling. It is also interesting that you found a significant CO_2 intake leading to the same extent of C storage.

comment#1: My main comments concern the discussion section, which I found to be very poor. Firstly, you dismissed the possibility of carbonate precipitation being responsible for C storage after ERW (due to the lack of an increase in dissolved inorganic carbon). See 4.2. Why should carbonate genesis occur at the surface? The localisation of carbonate precipitation depends on the ratio between the precipitation rate and the mass transport rate. How can carbonate precipitation in June–July explain C storage in August and September? I think there has been carbonate precipitation since the beginning, certainly driven by relatively high temperatures and dry conditions. However, you cannot prove this due to the lack of chemical data (e.g. Ca, Mg) and geochemical modelling.
We would like to clarify that we did not intend to dismiss the possibility of carbonate precipitation. Rather, we stated that our dataset provides no direct evidence for it, while acknowledging that our experimental design may not have been suited to detect it. For instance, carbonate formation could have occurred below 20 cm depth, as the reviewer correctly points out; but this message is indeed poorly convened in the discussion. Concerning the second part of the comment, our point was that carbonate formation rates may simply have been below the detection limit of our sampling strategy. Even small concentration changes over a three-week interval—the frequency of our sampling—could translate into significant cumulative storage at the macrocosm scale over an entire growing season. In addition, the number of samples collected during the driest period was limited, which could also explain why carbonate formation was not captured. In this sense, we are in agreement with the reviewer's interpretation, though we acknowledge that our original discussion did not convey this clearly. We revised Sections 4.2 and 4.3 to better reflect this:
Replace
"Nevertheless, two plausible explanations remain for why bicarbonates might have gone undetected: first, potential sub-surface bicarbonate formation beyond our 20 cm measurement depth, although carbonate genesis typically initiates closer to the surface; second, extremely gradual topsoil carbonate formation rates, particularly during the dry June–July period when most soil water was unavailable for sampling. While our current data provide no support for ERW-mediated reactions, we acknowledge methodological limitations prevent definitive conclusions regarding their occurrence."
by
"Nevertheless, three plausible explanations remain for why bicarbonates might have gone undetected: first, carbonate genesis occurs at the surface only if precipitation outpaces mass transport. If mass transport is faster than precipitation, carbonate can form deeper rather than being confined to the top 20cm, where we took our samples from; second, even small concentration changes over a three-week interval—the frequency of our sampling—could translate into significant cumulative storage at the macrocosm scale over an entire growing season; third, the number of samples collected during the driest period was limited, which could also explain why carbonate formation was not captured. Hence, while our current data provide no direct support for ERW-mediated reactions, we acknowledge methodological limitations prevent definitive conclusions regarding their occurrence."

comment #2: Secondly, although microbial activity is the only consistent variation, and roots are severely depleted after ERW, you emphasised the role of root exudates in explaining the increase in C storage as organic C in section 4.3. What about the increase in microbial biomass and dead cells? Wouldn't increase in microbial activity be likely to increase exudate degradation and thereby CO_2 emission? [...] However, I am not convinced that LMWOAs are preferentially released to enhance microbial activity, as it is insensitive. Nevertheless, I meant that more microbes may be related to more dead cells, which, along with secondary minerals, are an important factor in stabilising organic carbon (hence more CO_2 intake by the soil?).

We added the following paragraph at the end of the discussion:

"The two preceding explanations appear unlikely, as they are insufficient to account for the observed large increase in carbon sequestration. An alternative to enhanced root exudation is that the basalt treatment stimulated microbial biomass production. This stimulation could initially cause additional CO_2 emissions from the soil but, after microbial death, lead to substantial incorporation of metabolized carbon into associations with soil minerals. Such “microbial entombment” is known to produce highly recalcitrant carbon forms (Xiao et al., 2023). This interpretation is also consistent with our observation of increased microbial activity under the basalt treatment. If microbial turnover is particularly rapid, this mechanism could explain the significant carbon sequestration measured in our experiment and would be especially noteworthy, as it suggests that microbial entombment may represent a carbon storage pathway for enhanced rock weathering (ERW) comparable in importance to carbonate formation."

comment#3: You state in the abstract that C storage is beyond the effect of electrochemical transformation (I assume you mean Ammann et al., 2022), but I cannot find any convincing demonstration of this in the main text.

This is very likely a vocabulary mistake from our side, we mean “weathering rate” instead of electrochemical transformation. We will then correct lines 374-377: " Remarkably, this figure slightly exceeds the theoretically achievable limit of converting applied basalt into entirely carbonates (144 kg/ha for a 10 t/ha amendment). Hence, either complete transformation of the amendment occurred exceptionally swiftly within three months, which appears improbable based on existing literature (Kelland et al., 2020), or alternative mechanisms contributing to soil carbon sequestration must be considered.”

We also replaced

"Furthermore, the magnitude of carbon sequestration exceeded what could solely be attributed to electrochemical transformation."

by

"Furthermore, the magnitude of carbon sequestration exceeded what could solely be attributed to weathering rates."

comment #4: I think all the equations should be numbered, and all the units should be given.

We numbered the 4 equations in the Material & Methods section and provide units for each;

comment #5: Why should basalt at the surface lead to greater soil disturbance (and corresponding CO_2 emission)?

You are probably referring to the sentence: “Immediately after applying basalt, elevated CO_2 emissions occurred likely due to soil disturbance (Figure 2).” We understand your point that since all plots were tilled during seeding, and basalt was applied at that time, there is no obvious reason why the basalt-amended plots should have experienced greater disturbance and CO_2 release. This is a valid observation. We think that the presence of basalt may have stimulated microbial activity,

leading to higher CO_2 emissions a few days after application. We therefore replaced (in the results section)
"Immediately after applying basalt, elevated CO_2 emissions occurred likely due to soil disturbance (Figure 2)."
by
"A peak of CO_2 emissions occurred immediately after soil disturbance (tillage for seeding), and this peak was larger under basalt treatment (Figure 2)."
And in the discussion section we added the following sentence:
“Because all plots were tilled during seeding and basalt was applied at that stage, there is no clear reason why the basalt-amended plots should have undergone greater disturbance or CO_2 release. We propose instead that the presence of basalt may have stimulated microbial activity, resulting in elevated CO_2 emissions a few days after application in the treated plots.”

Comment#6: I also have plenty of minor comments, and these are only a few of them. I will give you the rest if they persist in the second round.
Table S1 : which mineral is 2:1 Al clay?
We understand this to refer to layer silicates with an Al-dominated octahedral sheet. This describes a group of minerals with a characteristic structure, rather than a single specific mineral.

Comment#7: Table S2 : special character editing issue in the headings
Corrected

Comment#8: Figure S6 : what are the R-squared and the number of samples? Typo: "top" instead of "sop"? p-value = 3.8.10-9
Corrected

Comment#9: One minor comment: it looks like there is a missing read character in Figure 1. Can you see all the fluxes other than plants and soils?
Indeed, the basalt treatment should appear in red for all fluxes, which was not the case. This is now corrected.

Reviewer #2

In their manuscript “Enhanced weathering leads to substantial C accrual on crop macrocosms” the authors present a mesocosm experiment (called “ecotron”) on carbon sequestration in soil cores upon application of finely ground basalt (<1mm). The soil cores were chosen and prepared to mimic the conversion of marginal lands to agricultural lands. The experiment was carefully set up. We particularly appreciate the measurement of all three major greenhouse gases (CO_2, CH_4, and N_2O). The full control of the atmospheric gas composition in the ecotron facility makes it possible to achieve a full carbon mass balance of the system. The inclusion of uncertainty and sensitivity analyses on plant roots and dissolved inorganic carbon (DIC) leaching via bootstrapping adds robustness to the results. In addition, the discussion around the potential stabilization of root exudates by weathering products and the subsequent formation of mineral-associated organic matter (MAOM) is valuable. The manuscript is well structured overall; we have a few suggestions for restructuring the text to enhance the story line. But several sections should be strengthened before publication. For example, the introduction section is very short and contains sloppy wording on core concepts (see detailed comments below). The discussion section lacks detail on the potential changes in plant metabolism that could explain observations. This study definitely merits publication but the comments below need careful consideration.

Comment#10: Bring introduction section in line with state of the art: The introduction section does not capture the state of the art of ERW research For instance, the absence of DIC leaching to deeper water layers, the complications introduced by interactions between the organic and inorganic carbon dynamics in natural soils (e.g. impact of weathering on SOM stabilization, soil organic carbon decomposition and plant C inputs), the difficult to quantify role of secondary mineral formation (PIC, clays, Fe/Al (hydr)oxides) are all well established and merit consideration. In this sense it lacks some recent references (e.g. Vicca et al (2022) Vienne et al. (2024), Steinwidder et al. (2025), …). A more complete and up to date description of the state of the art will strengthen the introduction, with as additional benefit to further support the relevance of the underlying study.

We thank the reviewer for this useful suggestion. We added the following text in the introduction: “Assessing the net impact of enhanced silicate weathering on soil carbon budgets is for example complicated by the tight coupling between organic and inorganic carbon dynamics. On the one hand, silicate dissolution contributes to inorganic C sequestration through carbonate precipitation and organo-mineral associations, such as stimulation of mineral -associated organic matter (Xu et al., 2024). On the other, it indirectly influences the largest terrestrial C pool—soil organic carbon—via changes in nutrient availability, microbial activity, litter quality, and aggregate formation. These processes can either promote stabilization of organic matter or accelerate its mineralization, with outcomes that vary across timescales and soil types (Vicca et al., 2022). In addition, secondary mineral formation (e.g. clays, Fe/Al (hydr)oxides, or pedogenic carbonates) can reduce DIC availability for export while providing new sorption surfaces that affect SOC stabilization. While enhanced weathering is typically framed in terms of inorganic CO_2 sequestration through pedogenic carbonate formation or leaching of dissolved weathering products, these fluxes can be relatively minor compared to other soil C processes. A substantial fraction of released base cations may instead be retained on exchange sites or incorporated into secondary minerals such as clays or Fe/Al (hydr)oxides, thereby bypassing direct CO_2 uptake. Moreover, organic carbon fluxes often exceed inorganic fluxes by orders of magnitude, with soil organic matter decomposition potentially releasing far more CO_2 than is sequestered via mineral weathering. At the same time, silicate amendments can enhance plant productivity and promote the transfer of organic C into more stable soil fractions through aggregation and mineral associations (Vienne et al., 2024). These contrasting effects illustrate the difficulty of isolating a single mechanism and highlight the need for integrative

approaches that jointly consider inorganic sinks, organic matter dynamics, and secondary mineral formation when evaluating the true CO_2 removal potential of enhanced weathering (Steinwidder et al., 2025). Together, these processes illustrate that reliable estimates of carbon dioxide removal (CDR) through enhanced weathering must account for both inorganic and organic C pools, as well as the role of secondary mineral phases, rather than focusing solely on weathering rates."

Comment#11: Context of marginal land application: It should be stressed that basalt was applied on artificially marginalized land that has been subjected to fertilization and cropping in the mesocosm setup. This is crucial to contextualize the results, as the implications may differ from intensively managed agricultural systems. Positioning the work in this context, in line with and related literature (e.g. https://doi.org/10.3389/fclim.2022.928403), could highlight the potential of enhanced weathering (EW) to increase food production and carbon sequestration on marginal soils, particularly in the Global South.
Indeed, it's an interesting point. This work doesn't support the hypothesis that EW can improve food production, though, as crop yield was not affected in our results. However we agree that the increase in soil TOC is potentially relevant for soil health in general. We added the following text in the introduction:
"To address these limitations, we conducted an EW amendment experiment on an artificially marginalized land that has been subjected to fertilization and cropping, within an ecotron—a controlled, closed-environment facility that enables real-time monitoring of ecosystem-level carbon fluxes (Roy et al., 2021)."
And in the discussion:
"This is especially significant given that our experimental macrocosms were established on marginal soils, where increases in SOC are typically associated with improvements in soil health, such as reduced erosion, enhanced water retention, and greater nutrient storage capacity. These results confirm that enhanced weathering—a potentially low-cost strategy—could strengthen both carbon sequestration and soil health on marginal lands (Ivan A Janssens et al., 2022), particularly in the Global South.

Comment#12: Discuss importance of biological processes: The comprehensive mass balance results show that increases in soil carbon are the primary driver of carbon sequestration. However, the manuscript does not fully articulate the logical implication that this increase is likely due to enhanced CO_2 uptake through photosynthesis, followed by subsequent release as root exudates (that can be stabilized in MAOM) (see e.g. https://www.science.org/doi/10.1126/sciadv.abd3176). In our opinion the discussion should be elaborated, and emphasize that biological processes, rather than geochemical mechanisms (DIC + PIC increases), appear to be the dominant contributors to the observed CO_2 uptake. Specifically, the fertilization effect of basalt (enhanced nutrient availability) in driving the observed changes in net ecosystem exchange (NEE) and CO_2 fluxes after basalt addition deserves stronger emphasis.
We respectfully disagree with that comment. We actually dedicate a full discussion section to address the potential role of biological processes in the observed sequestration (section 4.3). We also don't think that we can conclude to any fertilization effect of basalt as we found no significant difference in plant biomass in the two treatments. However we agree that we did not completely articulate that increase in root exudates or microbial activity would be initiated by enhanced plant photosynthesis. We therefore added the following text in the discussion:
"Although root exudates alone are unlikely to fully account for the magnitude of carbon storage observed, their role in MAOM formation represents a significant component of the coupled organic–inorganic processes underlying ERW-driven sequestration. An alternative explanation is that basalt

treatment indirectly stimulated microbial biomass production, through a combination of increased root exudation and improvements in soil physical and biochemical conditions. This stimulation could initially lead to additional soil CO_2 emissions, which aligns with the patterns observed in our experiment;"

Comment#13: Incomplete characterization of t0 : Methodologically, the characterization of t0 is confusing. In our opinion, the experiment starts at the beginning of the basalt amendment. However, soil pH and soil organic carbon are given at the time the soil cores were taken from the field. Between this time and the actual start of the experiment, the mesocosms were subjected to cropping and fertilization. Concerning the soil characteristics after the transition period, the authors only briefly mention that "the pH was 7 in all units". This is a high and atypical value for agricultural soil, and no information on vertical variation in pH is provided. No mention of soil carbon at the start of the experiment is given. It would strongly strengthen this paper if soil pH and organic carbon were available at t0 of the experiment. This is particularly relevant when interpreting the experimental results in terms of real agricultural systems. Including pH data after application would further strengthen the dataset and provide valuable context regarding soil chemistry changes. If soil samples were taken at the end of the experiment and are still present, this could be an interesting addition to the dataset.
Unfortunately the soil samples taken at the beginning of the experiment have been used for other purposes and we cannot run these measurements again. We would just like to clarify that soil pH was neutral at the beginning of the experiment as we used 10 t/ha equivalent of CaCO3 to correct for soil acidity at the beginning of the previous growing season. We added this information in the M&M.

Comment#14: We further have the following detailed comments and suggestions:
Abstract:
Mention that you monitored all GHGs in deep mesocosms and emphasize that 10 ton basalt ha-1 was added to degraded soils first. Emphasize that this experiment was not done on a typical rich agricultural soil.
Corrected: "We monitored all greenhouse gases in deep mesocosms representative of marginal soil conditions and applied 10 t ha^{-1} of basalt at the start of the experiment."

Comment#15: L30 "It involves the application of silicate rock powder to soils, where it is expected to react with CO_2 released from soil respiration, forming stable carbonate Ions". While this is an intuitive way of understanding the weathering process, it is more correct to rephrase it along the lines "Dissolution of silicate minerals enhances the alkalinity of the pore water, resulting at a shift of the carbonate system towards carbonate and bicarbonate, leading to higher dissolved inorganic carbon when the water is equilibrated with the atmosphere".
Corrected.

Comment#16: L33: 1.5 tons ha-1 sequestration; please specify that this is ton C ha-1 and not ton CO2 ha-1. Also add the timescale after which this C sequestration took place, 150 days = 1 growing season I suppose?
We specify in the same sentence that we refer to "measured carbon flux into the soil", hence we think it is already clear that the results are expressed in ton C ha-1 and not ton CO2 ha-1. We added that the results were obtained during one growing season.

Comment#17. Change electrochemical transformations into “geochemical C sequestration” or specify that DIC or PIC increases could not explain the observed C sequestration. Throughout the MS: change electrochemical into geochemical.
Corrected.

Intro:
Comment#18: L39 – The first sentence does not make sense. The relative contribution of agriculture to GHG emissions, does not “highlight the urgent need for scalable mitigation strategies.” If the authors want to stress the urgent need for mitigation, they can support and strengthen that argument with recent reports and publications on risks involved with the current emission pathways, literature on tipping points, etc ...
We deleted this sentence.

Comment#19: L41 Please be more specific. ERW should not be framed as a major mitigation strategy, but as a negative emission technology and a CDR method, specifically proposed to achieve a net negative GHG balance in the near future.
We corrected this sentence accordingly: “Enhanced rock weathering (EW) has emerged as a promising negative emissions strategy and carbon dioxide removal (CDR) approach, specifically aimed at achieving a net negative greenhouse gas balance in the near future and reducing carbon emissions from the agricultural sector”

Comment#20: L42 “where it reacts with CO2 etc...” -> see comment on abstract. Please rephrase accordingly.
We rephrased the sentence as follows: “The method involves applying silicate rock dust to soils, where the dissolution of silicate minerals increases the alkalinity of pore water, shifting the carbonate system toward carbonate and bicarbonate ions and leading to higher dissolved inorganic carbon when the water equilibrates with the atmosphere.”.

Comment#21: L42 – L50 This core part of the introduction section does not capture the state of the art of ERW research. (see general remark)
See reply to comment#10.

Comment#22: As mentioned above, the authors should introduce and motivate the relevance of specifically studying ERW on marginal soils that are converted to agriculture and clearly mention the artificial “marginalization” and conversion in the mesocosm setting.
See first part of the reply to comment#11 for what concerns the introduction.

Methods:
Comment#23: 2.1 Ecotron facility
Remove “(as Heading 1)” in the section title
Thanks for spotting this. Corrected.

Comment#24: L61 adapt to “[...] in gas-tight enclosure. High-frequency measurements allow [...]”
Corrected.

Comment#25: L62-L64 -> Move to introduction section
Corrected

Comment#26: L62 “this enables to study the actual dynamics of the process of EW ...” this oversells the system bit. Please be more precise, e.g. along the following lines: “This enables us to close the carbon mass balance in the system and to study the fate of released cations, while the large mesocosms [...]”
Corrected.

Comment#27: Lacking from the “ecotron”-part of the methods section is the way CO2 is regulated in the mesocosm -> L126-127 can be moved here.
Corrected.

2.2 Macrocosm
Comment#28: L83 “Six large macrocosms” -> Six large soil cores. In our opinion, it becomes a macrocosm once those cores are incorporated in the full setup.
Corrected.

Comment#29: L90: Agricultural treatments to mimic the conversion of heathland to crop fields. Please mention which treatments these were? Only the foliar Si, and NPK fertilization? Can you add details on how much NPK was applied?
We used NPK fertilization at the same level in all units and Si foliar spraying in three of them. We added 100 N 80P 120K, with NH_4NO_3, $NH_4H_2PO_4$ and KNO_3. This is now mentioned in the manuscript.

Comment#30: L92: How deep was the basalt plowed in?
20cm. We added this information in the manuscript.

Comment#31: L93: Is there a more detailed basalt particle size distribution than just <1 mm particle size? More specific information on the particle size and the specific surface area (e.g. BET surface area) would greatly benefit comparison to other experimental studies.
Unfortunately we couldn’t retrieve more information on the particle size distribution and the batch used in the experiment is now gone.

2.3 Climate simulations
Comment#32: L101-106 Move to Ecotron section. The current section should focus on the climate scenarios that were simulated, and how.
Corrected.

Comment#33: L100-101 and L109-110 convey are identical in message – remove one of both.
We removed the first one.

2.4 Plant biomass
Comment#34: L115: How were roots exactly measured? How much cores/m2 … for the entire 1.5m depth?
We did not use soil cores. As written in that section, we sampled entire plants (shoots + roots, by gently pulling them out of the soil – note that we comment also on that in the discussion to explain that we may have left some roots in the soil and we tried to correct for it in the bootstrap analysis) and weighted each organ separately.

2.5 CO2-C net flux
Comment#35: L126-127 move to the Ecotron section of the methods.

See reply to comment#27.

Comment#36: L133 “Data gaps were filled using a moving average function (ALMA function from the TTR package in R)” Please be more precise on the gap filling. Perhaps data was (linearly?) interpolated before running the moving average filter?
Indeed. We clarified this sentence.

2.7 Rainwater C flux
Comment#37: L150: Rainwater C flux: DOC in rainwater was measured, is inorganic C in rain water negligible? Or what are typical values please mention why this is not measured.
Indeed, we did not measure inorganic C in rainwater, because all of the volume was used for TOC/TN measurements. We clarified this in this section “Note that, since the entire sample volume was used for TOC/TN measurements, we could not measure the inorganic carbon input from rainwater. However, as the rainfall amounts were the same for each unit—following the same climate projections—this should not confound the results.”

Comment#38: Non-purgeable C or DOC, how was this measured, device, precision …?
Corrected.

Results:
Comment#39: L259: The largest input flux, by far, came from net CO2 exchange (366 to 457 gC/m2 depending on the treatment) ==> not clear which one is basalt and which one the control from the text, please rephrase.
Corrected: “366 to 457 gC/m^2 in control and basalt treatments, respectively”

Comment#40: L261: Not clear: were there significant differences in CH4 and N2O emitted after basalt amendment. Please add details.
We do not cover N20 in this paper as we focus on the C cycle. We just wanted to mention that the macrocosms were C sinks whatever the treatment, though the treatment had no significant impact on the methane balance of the system. We clarified this in the text: “The crop system was a net CH_4 sink throughout the experiment, but the treatment had no effect on methane balance of the macrocosm.”

Comment#41: L264, Figure 1: add the amount of days the growing season lasted. Add red / black everywhere for basalt/control to improve interpretation of this figure.
Corrected.

Comment#42: Figure 2: please add error bars for the cumulative NEE differences around the red and black line.
The standard error is already added to the graph, it is just barely visible. We clarified this in the text.

Comment#43: L320: Table 1: should be a figure for visual interpretation or just move to the supplement.
We are not sure about that – this paper has already been submitted to several journals and we found that reviewers were mostly critical on these soil water analyses, while it was presented as a supplementary figure. We would prefer to keep them as a table in the main text, because we think they are crucial to convey the message that there is lots of variability, and that the extent of the gaps might explain that we may have missed some carbonate peaks.

Comment#44: Colors are not explained.
Corrected: Orange cells: concentrations above 5mg/L; red cells: concentrations above 10mg/L.

Comment#44: L342: I think the evidence for microbial activity is a strong result and may be included in the main text, potentially as an alternative for Table 1.
Corrected.

Discussion:
Comment#45: see general remark about discussing role of photosynthesis, root exudates and MAOM.
See replies to comments#1, #2 and #12.

Comment#46: L374: You're mentioning that the C sequestration is higher than the theoretically achievable inroganic C sequestration. Based on the EW potential of the rock, we assume the Renforth (2019) formula was used to calculate this. If not already cited, please cite this work in the methods somewhere.
Indeed. We clarified this in the M&M section 2.1.

Comment#47: L363: Enhanced CO_2 levels are known to stimulate both weathering rates and CO_2 uptake via electrochemically-driven reactions (Amann et al., 2022). ==> change to geochemically.
Corrected.

Comment#48: L357: Alternatively, carbon sequestration rates nearing 2 tons per hectare were realized within a single year using equal amendment quantities (10 tons per hectare), though this involved utilizing a more chemically reactive substance—olivine—as opposed to basalt (Dietzen et al., 2019).
==> We believe Dietzen et al. 2018, measured an increase in soil respiration in the incubation experiment (Effectiveness of enhanced mineral weathering as a carbon sequestration tool and alternative to agricultural lime: An incubation experiment). A "CDR potential" is estimated if all Mg of the exchangeable pool were to leach out as Mg and charge balance with HCO3-. I think this is the value you refer to with "C-sequestration". Please nuance that no proof of inorganic C sequestration was found in the referenced Dietzen et al, 2018 study.
That is true indeed, I did not pay enough attention reading that paper, thanks for the suggestion. We corrected the sentence accordingly.

Comment#48: L366: Secondly, we also measured significantly higher microbial activity in basalt-amended soils, mirroring findings by Li et al. (2020), who reported a 33% increase in microbial activity following enriched rock-dust applications.
Please add some recent literature to emphasize this point. The simulation work of Klemme et al. (2022) also suggests increases in SOM decomposition after basalt amendment and pH increase. Steinwidder et al. (2025) also makes this point. Mention that for C sequestration in Kelland et al. (2020), only inorganic C was considered and that the reported sequestration was a modelled value.
L379: On the lack of substantial DIC / Ca / Mg leaching, others are finding similar results (e.g. Steinwidder et al. (2025), Vienne et al. (2024), Amann et al. (2020)).
Corrected. We added these references in the discussion to strengthen the points on increase in microbial activity, on comparison with reported sequestration in Kelland et al., 2020, and lack of ERW evidence in other papers. We also modified the concluding remarks of this section accordingly

("Therefore, although our data provide no direct evidence for ERW-mediated reactions, methodological limitations prevent us from drawing definitive conclusions regarding their occurrence. At the same time, we cannot exclude the possibility that some carbon was sequestered through pathways other than carbonate formation.").

Comment#49: It is possible that processes such as cation exchange produce protons and degas DIC, please comment on this as a possible mechanism for the non-observation of a DIC increase in leachates.
In that case, we indeed would observe no DIC in the soil water samples, but wouldn't it lead to soil CO2 emissions and substantially decrease the C sequestration?

Comment#50: L405: Also mention the work of Noah Sokol on MAOM and EW, there is at least 1 field study that discusses this. Other relevant recent work:
https://doi.org/10.1016/j.scitotenv.2025.180179.
We added these references to section 4.3 in the discussion.

Reviewer#3

This manuscript rigorously investigates the efficacy of enhanced weathering (EW) as a climate change mitigation strategy, with a focus on carbon (C) accrual in crop macrocosms under projected future climate conditions. A key strength is its employment of a large-scale ecotron facility (4.7 m^3 macrocosms, 1.5 m deep soil), which overcomes inherent limitations of prior field (uncontrolled C fluxes) and mesocosm (shallow soil profiles) studies by enabling comprehensive quantification of ecosystem-level C fluxes. The study demonstrates that EW enhances soil C flux and promotes C sequestration exceeding levels attributable solely to electrochemical processes, via both carbonate precipitation and enhanced biogeochemical activities. These findings provide novel evidence that EW drives substantial C sequestration in agricultural systems while challenging the traditional paradigm that EW-dependent C sequestration relies primarily on carbonate formation, thereby advancing understanding of EW's mechanisms and scalability and offering critical insights for developing degradation-specific soil C management strategies. Nevertheless, several points require clarification to improve the manuscript's clarity, logical coherence, and academic impact.

Comment#51: Lines 33-34: Caution is warranted regarding the conclusion that "EW treatment resulted in an almost three-fold enhancement of measured carbon flux into the soil, achieving rates up to 1.5 tons per hectare". As noted in the manuscript, basalt weathering does not increase soil inorganic carbon levels but may elevate soil CO_2 concentrations. This observation raises critical mechanistic and quantitative questions: following the documented influx of substantial CO_2 into the soil, does this gas primarily store in soil pores? Given that free CO_2 in soil pores is inherently unstable and susceptible to re-emission to the atmosphere, it is imperative to quantify the fraction of this CO_2 that can be stably/ long-term retained in the soil under EW conditions. This question could be addressed by comparing short-term and long-term differences in soil CO_2 levels under EW Such quantification is essential for accurately delineating the actual CO_2 sequestration efficiency of EW, as only stably retained carbon contributes to long-term climate mitigation value-distinguishing transient soil CO_2 accumulation from durable carbon sinks.

If we understand your comment correctly, you are suggesting that CO_2 may have been sequestered primarily as a gaseous pool, rather than in DIC, DOC, MAOM, POM, or particulate inorganic C. This is an interesting idea, but unfortunately we are unable to test it directly in our study, as the macrocosms were dismantled at the end of the experiment in 2023 and we do not have measurements of soil CO_2 concentrations. We would, however, like to note that the soil gaseous CO_2 pool is generally considered negligible compared with pools such as SOC or DOC, which are orders of magnitude larger. In addition, this pool is highly transient due to respiration and diffusion. If the treatment had substantially increased soil CO_2, this would likely have been reflected in the NEE measurements, and we would not have observed the increase in C sequestration. It is therefore possible that there was some misinterpretation of our results, and we are grateful for the opportunity to clarify this point.

Comment#52: In the Introduction section of the manuscript the elaboration on core scientific questions remains insufficient. While the experimental methodology underpins part of the work's innovative merit, it is imperative to supplement a dedicated paragraph that explicitly clarifies the scientific rationale for investigating EW-induced effects on soil carbon from the perspective of carbon fluxes. Specifically, this part should elaborate on the fundamental distinctions between this flux-based perspective and direct soil carbon evaluation, as well as the significance of expanding to a flux-centric framework when assessing EW's role in soil carbon sequestration.

We agree with the reviewer that the rationale of the study was not clearly laid out. We tried to correct the introduction accordingly, by deepening the literature context, and providing a proper

research gap (“These contrasting effects illustrate the difficulty of isolating a single mechanism and highlight the need for integrative approaches that jointly consider inorganic sinks, organic matter dynamics, and secondary mineral formation when evaluating the true CO_2 removal potential of enhanced weathering (Steinwidder et al., 2025). Together, these processes illustrate that reliable estimates of carbon dioxide removal (CDR) through enhanced weathering must account for both inorganic and organic C pools, as well as the role of secondary mineral phases, rather than focusing solely on weathering rates.”). We hope it addresses the reviewer’s comment. This reply is also connected to the improvements in the introduction section from comment#10.

Comment#53: It is recommended to incorporate an experimental design figure into the supplementary materials to facilitate readers’ comprehensive understanding the crop ecosystem within a macro-scale and the carbon flux monitoring system.
We added an experimental figure design into the “macrocosm details” box.

Comment#54: Line 244 The manuscript mentions a “different bootstrapping approach” for gappy DIC/Ca/Mg data, but does not detail how it differs from the standard bootstrapping used for soil C flux.
Thank you for pointing this out. We clarify how this bootstrap procedure differs from the initial one in the following sentence (“We generated empirical distributions of the concentration of the element (iC, Ca or Mg) in soil solution at the unit level (in order to keep the structure induced by depth and date). We then ran a generalized linear model with treatment, depth and their interaction as fixed variables and unit and date as random variables, and computed the average estimate and p-value of the effect of treatment across all iterations (n = 1,000).”), but we are happy to further emphasize this distinction if you feel it would improve clarity.

Comment#55: Result 3.1 Climate condition should be moved to Section 2.3 Climate simulations of the Material and Methods .
Corrected (see also comment #32).

Comment#56: The results section incorporates the discussion content, such as most of the content in 3.3. The results section should only present the results and move the explanations of the results into the Discussion section.
Corrected. Note that we added a new section in the material and methods to better introduce the Mg and Ca measurements.

Comment#57: Silicate weathering releases Ca^{2+} and Mg^{2+} (prerequisites for carbonate formation), yet no treatment differences in these cations were detected. The manuscript should clarify whether cation concentrations were measured in leachates (not just pore water) and discuss if plant uptake or adsorption to soil colloids explains the absence of leaching-key to ruling out carbonate formation
We understand that the reviewer means by “leaching” the Ca and Mg that would have left the macrocosm from the lower boundary of the lysimeter. Indeed our measurements were done on pore water only, not on leaching; we now clarify it in the manuscript. Indeed, we agree that the mechanisms suggested by the reviewer may be responsible for the absence of these elements in the soil solution; we added these elements to the discussion.

Comment#58: Line 30 Enhanced weathering (EW)” and Line 345 “enhanced rock weathering (ERW)” are used interchangeably. Standardize to one term to avoid confusion.
Corrected. We now use EW throughout the manuscript.

Comment#59: The width of the table should be the same as the width of the page window.
Corrected.

Comment#60: Notable formatting inconsistencies are present across both the main text and supplementary materials of the manuscript. A comprehensive review and adjustment of the text, figures, and supplementary content are therefore recommended to ensure adherence to academic formatting standards.
We assume that this comment is an introduction to the following ones. If not, we would appreciate it if the reviewer could clarify their comment, as we were unable to identify any formatting inconsistencies apart from what is listed below.

Comment#61: Line 450 Extraneous spacing precedes the reference list
We assume the reviewer refers here to the page jump. It is now removed.

Comment#62: Table S1 lacks the horizontal lines at the top and bottom.
Corrected.

Comment#63: The image resolution is insufficient for clear interpretation of details.
We assume the reviewer refers to Figure S1. It is unfortunately very difficult to embed such a complex figure on a word file with a proper resolution. However we generated the file as a pdf and will provide it to the editorial team if the paper is accepted for publication.

Comment#64: The y-axis font sizes in Figures S4 and S7 are excessively small, impeding readability.
Corrected.

Comment#65: The line spacing in the legend of Figure S5 within the manuscript is notably irregular and fails to align with the consistent design style adopted for other figure legends throughout the work.
Corrected.

Comment#66: The Discussion section of the manuscript inadequately integrates the isotopic research findings when elaborating on the impacts of EW on carbon flux and soil carbon.
Corrected (we added the following sentence in the discussion:" Isotope evidence also showed that the EW treatment did not directly interact with the atmospheric C pool instead of the CO2 coming from soil respiration. ").

Reviewer#4

This paper reports results of a study of enhanced silicate weathering (EW) at an ecotron (enclosed macrocosm) facility, producing a dataset of C cycling in this system that is novel in this field (similar previous work has either been carried out in open-system field settings (e.g. Kantola et al. 2023), or mesocosms (e.g. Vienne et al., 2024) that are not enclosed). This allows for whole-system determination of fluxes under tightly controlled conditions that is crucial in order to address key outstanding questions in the field of EW – what is the short-to-medium-term fate of soil organic carbon when silicate fertilisers are added? What is the balance of inorganic carbon stored as SIC or DIC in pore water/leachate? And what is the overall GHG budget in these systems?

Comment#67: The study is valuable in this regard, but significant improvements must be made to the data presentation and interrogation here before this manuscript is fit for publication, to the extent that I would suggest a rejection and resubmission, rather than major revisions. Most glaringly, while the headline of soil C sequestration in the basalt-treated macrocosms is reported, this figure (calculated from difference in fluxes) is at odds with all of the data collected from the chemical pools in question (SOC, SIC, porewater DIC and DOC) – which in fact seem to show that the soil C pool is actually much greater in the control macrocosms rather than the basalt-treated macrocosms (driven by SOC, see Table 2). This discrepancy is not addressed at all, even though the authors do note in the discussion that the sink of C that their system-wide balance suggests cannot be traced by inorganic C formation. Figure 1 is therefore very misleading in this regard.

[…]

It seems to me that the big question here is how to reconcile the CO2 flux data with the other datasets the authors have generated. This must be addressed before this study should be resubmitted for publication – and my sense is that properly reconciling this discrepancy will result in a complete overhaul and re-write of the discussion, conclusions and headline takeaways.

We understand the reviewer to be suggesting a discrepancy between the reported carbon flux calculations and the chemical pool measurements, which appear to indicate greater carbon sequestration in the controls. However, we believe this is a misinterpretation of our results.

-First, this table shows the size of the C pool at the end of the experiment: **we assessed their absolute size rather than any temporal change**. Since we could not measure these pools at the start of the experiment, it is possible that the control plots already had higher pool sizes initially.

-Second, there is actually **no significant difference in POC pool size**. While the controls do show slightly higher values, the variability is large, as reflected in the standard errors. To the reviewer's credit, this point is not explicitly stated in the manuscript—it was included in an earlier version submitted to another journal but was removed following a previous reviewer's suggestion (in hindsight, we should have retained it).

-Finally, and most importantly, as noted in the manuscript, given the relative size of these pools compared to the fluxes, along with the inherent variability of soil C content, **any increase in pool size would have fallen within the range of measurement uncertainty**.

For these three reasons there is no discrepancy between the pool size at the end of the experiment and the measured C fluxes.

We addressed this comment by adding this paragraph in the results section where the results of Table 2 are described:

“The results presented in Table 2 show the size of the C pools at the end of the experiment. Because pool sizes were not measured at the beginning of the experiment, it is possible that control plots already contained higher C pools initially. We did not observe statistically significant differences in POC pool size between the treatments. Although mean values appear higher in the controls, the variability is considerable, as indicated by the large standard errors. The relative size of these pools (3713 ± 320 g/m² in the basalt treatment and 5086 ± 921 g/m² in the control; see Table 2), compared to the measured fluxes, together with the inherent variability of soil C content, means that increase in pool size due to the measured C sequestration fell within the range of measurement uncertainty.”

As well as in the discussion:

"While the missing initial TOC data limits our ability to quantify the absolute change in the soil C pool, we emphasize that the NEE flux measurements provide a more sensitive indicator of short-term sequestration. This is because the magnitude of carbon sequestration observed, while substantial (up to 1.5 tons per hectare over one season), is relatively small compared to the total size of the soil C pool, meaning any corresponding increase in pool size would have fallen within the range of measurement uncertainty. Consequently, the flux data offers the most robust evidence for enhanced sequestration."

Comment#68: Note that there are several fluxes that are missing in their analyses. It seems to me that leachate was only analysed for DOC, not DIC. This may be significant (though previous studies, such as Kelland et al. 2020, which is cited by the authors, find that DIC export in leachate is negligible in mesocosm EW experiments).

It is true that we did not analyse the leachates for DIC, due to technical limitations related to the design of the leachate sampling system. Nevertheless, DIC would first need to be formed near the surface (as the basalt was applied to the top 20 cm) and should therefore have been detectable in the upper soil layers before reaching the lower lysimeter boundary. We did not find any evidence of this. But we agree that this deserves some clarification, that we think relates a lot to comment#1 (reviewer1). We addressed these two comments the same way.

However, and most importantly, the central conclusion of our study remains unchanged: the extent of C sequestration observed is significantly greater than what is theoretically possible through complete weathering of the added basalt, as calculated with the Renforth formula. In this context, whether or not some DIC was missed in the leachate is a secondary consideration.

Comment#69: The “Extra mechanisms of C sequestration” touched upon by the authors in the discussion are invoked because additional effects on top of weathering of mafic minerals charge-balancing cations with bicarbonate are needed to explain the difference in measured C fluxes. As such the authors focus on the recalcitrance of organic C here; but this ignores the fact that a change in proportion of OM as MAOM should not mean that SOC change cannot be measured in the soil. Furthermore, an explanation for greater C storage in soils as a result of EW is at odds with the observation (Figure S9) that microbial activity was higher in the basalt-treated macrocosms. This means more active respiration of SOC and should result in a greater CO2 flux out of the soil, as well as a reduction in SOC stock (which is in fact what the authors observe).

We agree with the reviewer. This was also pointed out by two other reviewers. We corrected this as explained in replies to comments#2 and #12.

Line-by-line comments:

Comment#70: Line 88: Note that in the “marginalization” process, topsoil was removed. This makes the system deplete in TOC in the upper portion of the soil, which may be something to come back to in the discussion on measured C fluxes.

Indeed. We added these sentences in the discussion “It is important to note that these results were obtained on soil with initially low SOC, as the experimental site was established by converting heathland into cropland through removal of topsoil and vegetation. This initial condition likely influenced the processes observed. In particular, because the soil was far from C saturation, its capacity for additional C sequestration was relatively high. For example, mineral surfaces available for association with newly formed organic matter were more likely to be unoccupied, creating conditions that favored stabilization of fresh C inputs. Thus, the experiment was conducted under circumstances representing a situation with one of the highest potentials for C sequestration.” See also answer to comment#11.

Comment#71: Line 90: Future climate scenario (2070-2075) – what does this entail? (In terms of temperature, precipitation). [EDIT: I see this is given in Line 108;

Indeed, see line 108.

Comment#72: Line 91-92: How much foliar Si and NPK fertiliser was applied?

We appreciate the reviewer’s comment. However, we believe this point has already been addressed in Rineau et al. (2024), where the amendment was applied to the plants rather than the soil, on the growing season preceding this experiment. For this reason, we consider it outside the scope of the present manuscript.

Comment#73: Line 92: To what depth was the basalt incorporated?

20cm. We added this information in the manuscript. See also response to comment#30.

Comment#74: Line 93: Is there a particle size distribution for the basalt that you added? How was the basalt ground (e.g. ball mill?). Do you have data on the specific surface area of the basalt? These are all of consequence as the surface area distribution of the material is a key determinant of the reactivity.

Unfortunately we couldn’t retrieve more information on the particle size distribution and the batch used in the experiment is now gone. See also reply to comment#31.

Comment#75: Line 95: Rephrase “…contents, and a maximum potential C removal capacity of 529 kg…”

We corrected accordingly.

Comment#76: Line 95: How was this C removal potential calculated?

Using Renforth formula. This is clarified in the new version of the manuscript. See also reply to comment#46.

Comment#77: Line 135: I’m not sure I follow this line of reasoning. As I read this now, this is trying to say the following: CO2 within the chambers was regularly measured and converted into fluxes. Following measurement (or continuously, I’m not sure I follow this), CO2 was either injected into the

chambers or scrubbed from the atmosphere of the chambers in order to maintain the "future atmosphere" pCO2 of ambient + 221ppm. This process of amending the CO2 concentration in the chamber can interfere with C cycling within the chamber, as ambient CO2 concentration within the chamber does not replicate timescales of atmospheric mixing perfectly (?). Thus, to estimate the effect that this might have on calculated C fluxes, you assumed that the largest likely swing in CO2 concentration within the chamber as a result of amending CO2 is 400ppm, which – if completely taken up by plants and converted to biomass – yields 50mg of C, or 1g over the duration of experiment.

If this isn't quite accurate, I would sharpen the language used to make it clearer when exactly the CO2 amendment occurred relative to the measurement, and what you mean by the figure of 1g C during the experiment.

Thank you for this comment. We indeed realize that our original explanation may not have been sufficiently clear. What we intended to convey is that there may have been some confusion between three distinct aspects: (i) the CO_2 control system, (ii) our model to estimate net ecosystem exchange, and (iii) potential disturbances caused by disruption of chamber seal integrity. Based on our observations, CO_2 concentrations inside the chambers could increase from ~400 to ~800 ppm during a prolonged intervention. To provide a conservative estimate, we assumed that the entire excess of 400 ppm was not scrubbed by the Ecotron control system but instead fully assimilated through plant photosynthesis (which is unlikely in reality). Using chamber volume, average temperature, and pressure, this would correspond to approximately 50 mg of C fixed in plant biomass per intervention. Multiplying this by the number of interventions gives our estimate of about 1 g of potential error in C sequestration.

We suggest to replace l134-138 by the following text: "Interventions in the chambers can slightly disturb the system's carbon balance, as CO_2 levels in the chamber equilibrate with those of the main corridor. During long interventions, CO_2 concentrations inside the chambers were observed to increase from ~400 to ~800 ppm. For a conservative estimate of potential bias, we assumed that the entire excess of 400 ppm was not removed by the Ecotron control system but instead fully assimilated through photosynthesis, although this scenario is unlikely. Based on chamber volume, average temperature, and pressure, this would correspond to ~50 mg C fixed per intervention. Scaled to the total number of interventions, this results in an estimated maximum error of ~1 g C in sequestration".

Comment#78: Line 143: what is meant by "in the absence of a macrocosm"?

It literally means when no macrocosm was in the chamber. We wanted to verify that there was no other methane source outside of the macrocosm itself.

Comment#79: Line 145-146: What additional tests were performed to verify that microbial activity did not contribute to CH4 fluxes?

We temporarily sealed the drainage system, monitored CH_4 levels, and compared them to pre-sealing measurements. These details were not included in the manuscript, as they fall outside the main focus of this study, but we can provide this information if the reviewer considers it relevant.

Comment#80: Line 160: Does leachate C flux only relate to organic C? What about inorganic C (i.e. bicarbonate and carbonate alkalinity), which is likely to be a large flux of C?

See replies to comments#68 and #49. We did not detect any DIC in the 5 soil layers above the bottom of the lysimeter. We clarified that the leachate indeed relates to organic C and that inorganic C is ruled out by higher depth measurements.

Comment#81: Line 187-188: What instrument/analyser did you use for DIC and DOC measurements?

We clarified this in the manuscript.

Comment#82: Line 188-190: I would improve the wording of this for clarity. You measure concentration of Soil Organic Carbon and Soil Inorganic Carbon at the end of the experiment and then assess the difference between control and basalt-treated macrocosms to determine whether there has been an effect of the treatment on the formation and storage of SIC and SOC (I think this phrasing works a bit better).. I would not describe this as Particulate Inorganic or Organic Carbon, as this generally implies suspended load in solution (which is not something you have looked at, as far as I understand).

Indeed, the suggested wording is clearer. We corrected these sentences accordingly.

Comment#83; Line 193: Give the make and model of the analyser.

We corrected accordingly.

Comment#84: 2.12: "soil carbon" here refers to SOC+SIC, doesn't it, given the analytical procedure? I would make it clear in this section if these two C reservoirs were considered separately in the isotope mass-balance, or whether there were data suggesting it is possible to ignore the contribution of SIC.

Soil C indeed refers here to soil carbon in a broad sense (organic and inorganic). We corrected accordingly.

Comment#85: Line 244, 295: Replace "gappy" with "sparse", or change to "the dataset had gaps". I wouldn't use the word "gappy".

We corrected accordingly.

Comment#86: 3.1: Are climatic conditions allowed to vary within the chambers then? In Line 65, you say "environmental conditions including air temperature, precipitation, relative humidity, CO_2 concentration, and wind speed were precisely controlled". This section implies that this is not the case. If so, I would explicitly state in Line 65 that environmental conditions (with the exception of CO2 concentration?) were allowed to vary with ambient conditions.

We may not fully understand this comment. Climate conditions in the experiment were controlled and followed a predefined climate scenario based on a climate model. In this section, we describe the outcome of this scenario in the Ecotron units.

Comment#87: Figure 1: Should some of these values be in red font? E.g. CO2 net flux (there are two values in black).

Indeed. We corrected accordingly. See also response to comments#9 and #41.

Comment#88: Line 272-274: But important to state that this underestimate does not affect the EW treatment more than the control.

Thank you for pointing this out, we added a sentence to clarify this: "; note that this affected the two treatments equally.".

Comment#89: Line 285: I understand that this is at a whole-system level; but this is not observed in the soil samples you measure (in fact the opposite). Surely it's better to amend this figure to show the actual measured values of soil C, and then note the discrepancy in C fluxes?

We understand that this comment refers to the values shown in Table 2. The POC values are not significantly higher in the control treatment (see the high standard error) and the sequestration rates are too low and integrated in a too short time to be reflected in the POC: they are lower than the noise. See also reply to comment#67.

Comment#90: Figure 2: Define NEE in the figure caption.

We corrected accordingly.

Comment#91: 3.3: Specify here whether the "inorganic carbon" that is being talked about is "dissolved inorganic carbon in soil pore water", or "soil inorganic carbon" – i.e. solid-phase carbonates. Note that the title "soil carbonates" implies the latter, when I think reading the paragraph suggests to me that the former is meant.

In this paragraph, we indeed meant dissolved inorganic carbon in soil pore water. We corrected accordingly.

Comment#92: Line 295: "the" is in bold where it shouldn't be.

We corrected accordingly.

Comment#93: Line 295: I don't understand what the figures quoted in brackets here are referring to – there seems to be a unit missing, and if this is a difference between treatments then it should be stated as such.

These are the difference in estimates of linear model after bootstrapping (hence expressed in mg/l). We clarified this.

Comment#94: Line 304: Again, include units here.

We corrected accordingly.

Comment#95: Line 305: "Carbonates might have initially formed in the soil but later degasseed [spelling mistake] as CO_2 during sample storage prior to analysis". How do the authors suggest this could have happened? Were samples heated or acidified prior to analysis? If dissolved, C would not have degassed but would be added to DIC.

The sentence with the spelling mistake got deleted from adjusting the text to another comment. We think that as the soil pore water samples underwent at least twice manipulations where they were exposed to low CO2 partial pressure in the headspace: when stored into the collection bottle for up to three weeks, when filtered and aliquoted to 20ml vials for storage. We cannot rule out that the carbonates dissolved in the water degassed at least partly as CO2 during these manipulations. We clarified by adding this sentence: "The soil pore water samples were subjected to at least two manipulations during which they were exposed to a low CO_2 partial pressure in the headspace: first, during storage in the collection bottles for up to three weeks, and second, during filtration and aliquoting into 20 mL vials. Under these conditions, dissolved carbonates may have partially degassed as CO_2."

Comment#96: Line 308-309: Again, include units here.

We corrected accordingly.

Comment#97: Figure S7: “effect was not significant at any depth” – to what confidence interval? Visually it does look like concentrations of Ca in solution were higher in basalt-treated samples as compared with control samples. I think what would be instructive here is to do a simple back-of-the-envelope calculation to look at effect size needed for mass-balance: assuming that all the difference in C flux between basalt and control is the result of DIC stored in soil porewater, and assuming that all of this DIC is in the form of bicarbonate charge-balanced by Ca and Mg ions, then how much of a change in the concentration of Ca in soil porewater would you expect to see? Is this change larger than the confidence interval?

Concentrations of Ca and Mg tended to be higher in the basalt treatment, but variability was substantial. A generalized linear model including treatment, depth, and their interaction as fixed effects, with unit and date as random effects, indicated that concentrations of both cations were not significantly affected by treatment at any level ($p > 0.05$), even after bootstrapping to account for the sparse dataset.

For a the back of the envelope calculation: assuming 150 g C m^{-2} of additional sequestered C in a 1.5 m deep soil column, with 10% volumetric water content in the top 60 cm and 30% in the bottom 90 cm, the total water volume would be approximately 330 L (acknowledging that water content varies throughout the growing season). At the same time, 380 L of leachate were collected over the season, corresponding to ~704 mg/L Ca equivalent. If we conservatively assume that C sequestration occurred at a constant rate over the growing season (rather than the observed peak in May–June), this would translate into an average difference of ~88 mg/L across 8 sampling dates. This value is about four times higher than the maximum concentrations measured in pore water and roughly 18 times higher than the seasonal average. Even accounting for the possibility that some of the reaction also releases Mg, the measured concentrations are still, on average, about one order of magnitude below the expected values.

Comment#98: Table 1: What does the colour-coding mean here? There are also no units for the concentration given here. I’m not sure that this data needs to be in the main text rather than the supplement; it is more instructive visually to show box-and-whisker plots (e.g. Figure S7).

As noted in our response to comment #43, we are happy to include this as a supplementary figure if desired. However, given that several reviewers raised questions regarding the pore water analyses, we believe it is important to present the full data structure and variability. A figure alone could give the misleading impression of a continuous, stable dataset. For information here are two possible figures:

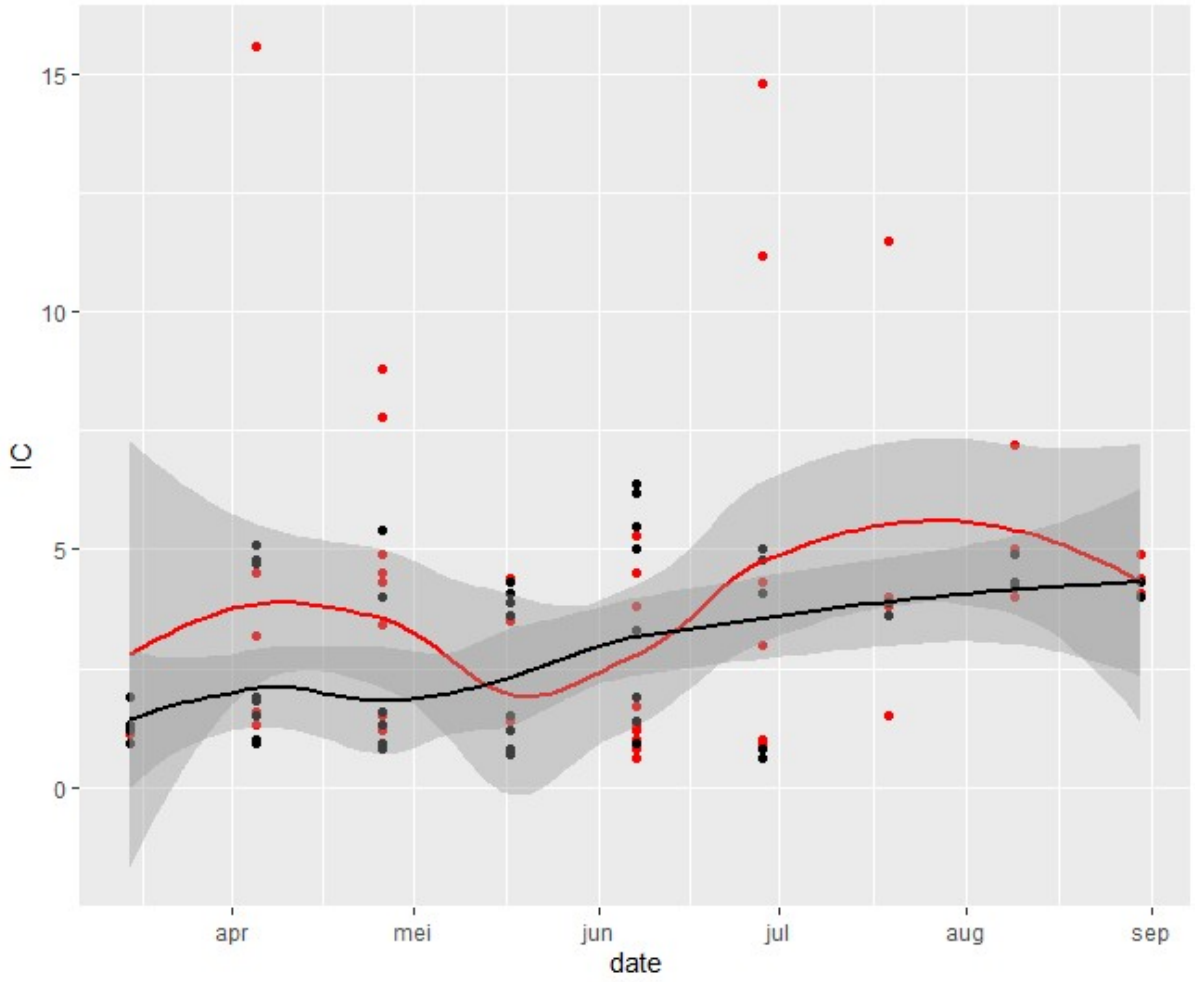

Figure SX. Effect of treatment on inorganic C in soil solution (mg/l). Black: control, Red: basalt treatment. Grey area: 95% confidence interval. We performed a t-test to test the effect of basalt amendment on inorganic C in soil solution at every date, which returned no significant result.

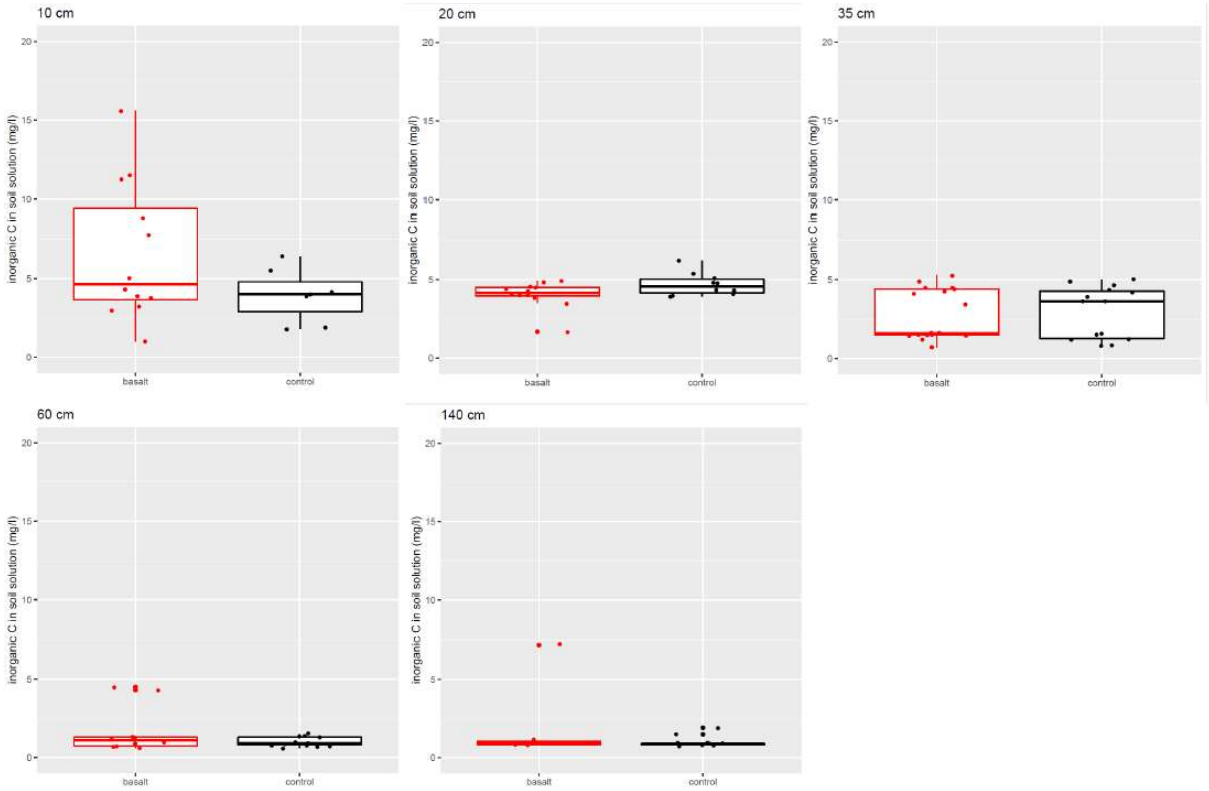

Figure SX. Effect of basalt on inorganic carbon concentration of soil water samples taken by suction cups in function of depth. The suction cups were installed at 5 different depths, with 3 replicates per depth, and set at a tension of -150 HPa until 35cm deep and -50 HPa below. Soil water was sampled every 3 weeks and analysed for TOC and inorganic C. Values are pooled for all dates after basalt amendment until just before harvest (30/08/2022). The effect of basalt treatment has been tested by a mixed model with treatment as a fixed effect and unit as a random effect, and was not significant at any depth.

Comment#99: Table 2: I think the word “particulate” in this context is not necessary and is in fact misleading, as this does not refer to suspended OC in solution (which is the context in which it is usually used).

We corrected accordingly.

Comment#100: Line 330: ‘wt%’ cannot be described as a rate. Reword to clarify what you mean here.

Indeed, we meant amounts, not rates. We clarified accordingly.

Comment#101: 3.5: I would carefully rephrase the writing here to be much more precise. Rather than “rule out the possibility that the basalt directly reacted with CO_2 from the atmospheric pool rather than with CO_2 derived from soil respiration”, I would phrase this as “test to what extent atmospheric C was incorporated into soil inorganic C during the lifetime of the experiment” – note that this does not have to be mediated by basalt dissolution. Figure S2 does not allow you to observe a “shift”, given that you do not test SIC before and after incubation in the atmosphere with negative d13C signature; though it does allow you to observe a difference (or lack of difference) between the SIC C isotope signature between the two treatments, suggesting (as you say) that direct atmospheric CO2 fixation by basalt was negligible, as would be expected given the location of the basalt in the topsoil and the difference in CO2 concentrations in topsoils vs in atmosphere.

Thank you for the suggestion. We replaced this sentence by “Although Figure S2 does not allow us to observe temporal shifts in SIC $\delta^{13}C$ (since we did not measure SIC before and after incubation under an isotopically distinct atmosphere), it does allow comparison between treatments. The lack of a meaningful difference in SIC $\delta^{13}C$ between the basalt and control treatments suggests that direct incorporation of atmospheric CO_2 into soil inorganic carbon via basalt was negligible. This result is consistent with expectations, given the position of basalt in the topsoil and the difference between soil and atmospheric CO_2 concentrations”.

Comment#102: Line 347: Yes, on a macrocosm-wide budget level – but crucially, are you able to measure the change in the actual soil C stock itself? Table 2 suggests the opposite trend! This is an important caveat that should be made here.

No, as the observed increase is within the measurement uncertainty of SOC measurements. See replies to comments#67 and #89.