

Reviewer 1

Generally I enjoyed reading this study. The Results section is quite dense as you document each analyte in turn and I wonder whether this could be better summarised. There is a lack of experimental detail in places, particularly about things like the earthworm species, unless I missed it, and a significant lack of quality control data to accompany your analytical results. I think, given your data you could add in a mass balance to quantify the extent of basalt weathering and whether this is accelerated by earthworms.

We thank the reviewer for these comments. We agree that the Results section was overly detailed in places due to the analyte-by-analyte description. We will therefore streamline the Results section, particularly the sections describing porewater chemistry and plant nutrient and metal concentrations. There we will revise the text by summarizing overarching response patterns where possible while retaining tissue- and growing-season-specific results relevant to the Discussion. We will also add further experimental details, including information on the earthworm species, and expanded the description of analytical quality control, as detailed in our responses to the specific comments below. We address the reviewer's suggestion to perform an elemental mass balance in detail in response to the specific comment on this topic (see last comment of R1 below).

Changes that will be made to streamline the result section:

L275-278: "Porewater nutrient responses differed among elements. N increased with basalt but was unaffected by earthworms, whereas P decreased with both basalt and earthworms, with no basalt × earthworm interaction for either element (Fig. 4). In contrast, Ca, Mg, and Na showed significant basalt × earthworm interactions, with earthworms reinforcing the basalt-induced increases in their porewater concentrations (Fig. 4, Fig. S2, Method S1). Despite the increases in Ca and Mg, the Ca/Mg ratio decreased with basalt, particularly in 2023 (Fig. 5)."

Line 402-405: "Statistical inference for porewater Cr, Ni, and Mn was limited by concentrations below LOQ (Cr and Ni) or violation of model assumptions (Mn). Nonetheless, Ni tended to increase and Mn to decrease with basalt (Fig. 7, method S1). In contrast, Al and Zn, for which concentrations were above LOQ, decreased significantly with basalt and were not significantly affected by earthworms (Fig. 7, Fig. S2)."

Line 580-584: "Responses of the other plant nutrients were more tissue- and growing-season-specific. Basalt altered Fe concentrations differently among plant parts, decreasing leaf Fe in 2022 while increasing root Fe in both years and stem and tassel Fe in 2023. Effects on P and K were less consistent: basalt increased leaf P but decreased tassel P in 2022, while earthworms enhanced leaf P under basalt addition in 2023. Stem K decreased with basalt in 2023 only in the absence of earthworms. Effects on plant C and N concentrations were similarly dependent on plant part and growing season (Fig. 12, Tables S3–S4)."

Line 704-708: "Basalt and earthworms also altered plant nutrient ratios, although responses differed among plant parts and growing seasons (Fig. 13, Tables S3–S4). Earthworms decreased C/N ratios in tassels in 2022 and leaves in 2023, while basalt decreased stem C/N. Basalt generally decreased N/P ratios in 2022. This effect disappeared in 2023 while a negative

earthworm effect emerged for leaves and stem. Last, Ca/Mg decreased with basalt across all plant parts in 2022 and in leaves, stems, and roots in 2023. Some tissue-specific basalt × earthworm interactions occurred in 2023, particularly for stem and corn Ca/Mg ratios.”

Line 748-757; “Basalt effects on plant metal concentrations differed among plant parts and growing seasons and were generally more pronounced in 2022 (Fig. 14-15, Tables S5-S6). In 2022, basalt decreased leaf Al, corn and tassel Cd, leaf Ni, and Zn concentrations in all plant parts, while increasing tassel Cr and Ni and root Al, V, and Ni concentrations. In 2023, effects on Al, Cr, Cd, and V were no longer apparent, whereas the increase in root Ni persisted. Zn responses also differed between growing seasons, with concentrations decreasing in all plant parts in 2022, but increasing in roots and stems in 2023. Earthworms did not significantly affect these metal concentrations.

Basalt × earthworm interactions were found for Pb concentrations in roots in 2022 and in leaves and corn in 2023 (Fig. 14-15, Tables S5-S6). In 2022, basalt increased root Pb concentrations only in the absence of earthworms. In 2023, basalt increased leaf Pb concentrations in the presence of earthworms but decreased them in their absence, while corn Pb concentrations were highest in the treatment with basalt and earthworms.”

Title (and throughout): don't use the term "heavy metal", it is unscientific - see <https://publications.iupac.org/pac/2002/pdf/7405x0793.pdf>

We replaced 'heavy metals' by 'metals' through the manuscript

Abstract

This needs to be far more quantitative - we don't know the soil pH or organic matter content, the duration of the experiments, whether differences between treatments were statistically significant, what concentrations in plant biomass actually were.

We thank the reviewer for this comment. In the revised manuscript, we will include the pH and organic matter of the soil, duration of the experiment and the increase in porewater chemistry and nutrients:

Line 16-20: “Here, we conducted a full-factorial experiment to test the combined effects of basalt and earthworms on soil chemistry, and growth, nutrients and heavy metal concentrations of *Zea mays* for two growing seasons (1 and a half year) using a sandy loam soil w(pH = 5.59, organic carbon = 0.83%).”

Basalt significantly increased porewater pH by 0.3-0.8 units, and alkalinity by 30-60, % compared to the control treatment. Concentrations of major base cations increased as well: Ca by 40-80%, Mg by 50-120%, Na by 30-70% and Si by 60-150%, depending on treatments and growing season.”

Introduction

2nd para - there are instructions left in the text "Click or tap here to enter text" - this needs proof reading properly.

We thank the reviewer for noticing. These add-ins will be deleted.

Line 31 - I think the statement that there is good evidence for EW benefiting plant growth in the tropics but weak evidence in temperate regions is not accurate. There are lots of published experiments in temperate conditions as well. Perhaps the authors mean field trials (though I don't think this paper is about a field trial either). The statement seems at odds with line 41 where the well documented benefits of EW are acknowledged.

Line 33 - I not that the first author is citing a very limited number of papers here but 2 out of 3 are their own. There's nothing wrong with self citation but it is important to acknowledge others work in the field as well.

The text will be elaborated as follows:

“The benefits of EW for crop growth are demonstrated in tropical regions (Swoboda et al., 2022), while evidence in temperate climates remains inconsistent. A few EW experiments in temperate climates reported increased crop growth for basalt and dunite application alongside changes in plant elemental composition, including increased Ca, Mg, and Si uptake (Rijnders et al., 2023, 2024; Skov et al., 2024). Other studies reported no biomass response to EW application (Te Pas et al., 2026, Rineau et al., 2026), or even decreased plant biomass (Anthony et al., 2025). These inconsistent findings suggest that not only climatic region, but also other factors such as soil properties play an important role.”

<https://bg.copernicus.org/articles/23/2261/2026/>

Methodology

Line 63 - justify this choice of amendment level - is it typical of what is advised when EW is being done at scale in the field?

We thank the reviewer for this comment. The application rate of 50 t ha⁻¹ is well within the range used in other EW experiments (Suhroff et al., 2026), and we have added this justification and reference to the Methods. Changes made:

Line 64: “The upper 20 cm was filled with the same soil, either unamended in the control treatment (16 replicates) or amended with 50 ton ha⁻¹ of basalt (Table S1), similar to what is regularly used in experiments (Surhof et al., 2026), mixed into the topsoil layer (16 replicates)”

Table 1 - caption needs to state how many replicates were used for each measurement and what the uncertainty is - std err or std dev. Also methods used to determine these parameters need to be cited and, for chemical measurements quality control data should be given.

We thank the reviewer for this comment. The following changes were made:

- In the heading of the table “The methods used to analyse these characteristics can be found under 2.3: Soil measurements and chemical analysis (n=6 for pH, organic C, cation exchange capacity, n=3 for texture). ” will be added

- Line 160-162: pH was measured by collecting and pooling three subsamples per mesocosm. Subsequently, 4 g of soil was dissolved in 10 mL deionized water and shaken before analysis using a Metrohm 914 pH/Conductometer meter.

Line 76 - how old and what growth stage were the maize plants at when added?

We thank the reviewer for this comment and will clarify this in the revised manuscript.

Changes made on Line 97: "Two maize seeds, inoculated with spores of arbuscular mycorrhizal fungi..."

Line 80 - how does this fertilisation rate compare to what is typically used in the field?

Line 101-103: "The typical fertilization rate depends among soil types, but is in general lower compared to conventional agricultural practices in Flanders (Odeurs, W. & Bries, J., 2023). Given that dwarf maize is used in this study, the applied amount should be sufficient."
<https://edepot.wur.nl/274577>

Line 84 - at destructive sampling what was earthworm recovery like and was there a mass change? (Line 89 - at the end of the experiment were the remaining mesocosms destructively sampled as well as the plants being harvested?)

We thank the reviewer for these questions. We will provide the requested information in the revised manuscript:

Line 109-110: "At the first harvest, destructive soil sampling, using a soil auger, was conducted on four mesocosms per treatment. The soil, plants and earthworms were collected as described below."

Line 126-127: "At the end of the growing seasons, earthworms were collected by carefully search through the soil."

Line 87 - first mention of the earthworm additions - needs far more detail - species, mass added, how this compares to typical field densities, source of earthworms, how their identity was confirmed etc.

We agree that this needs more clarification and will add the following information:

Line 104-107: The treatments with earthworms received 20 individuals (ten (*Aporrectodea caliginosa*) and ten *Aporrectodea rosea*), collected nearby the Drie Eiken campus of the University of Antwerp. Earthworm density was selected based on average earthworm density in agricultural soil in Europe, taking into account that only two species of endogenic earthworms are added (Dinter et al., 2013; Rutgers et al., 2016).

Line 101 - how were the cores taken? Were the mesocosms excavated and, when a certain depth was taken a core was taken or something else?

We will add the requested information:

Commented [SV1]: Latijnse namen in italics

Line 147-148: After each growing season, soil samples were collected using a soil auger. Samples were first collected from the top layer, after which this layer was removed while carefully searching for earthworms. This procedure was then repeated for the second and third soil layers.

Line 106 - can you justify this assumption by citing some studies which have tested it?

This assumption was based on previous maize experiments using a similar mesocosm system, in which root biomass was sampled both directly beneath plants and between plants to account for its spatial distribution (Verlinden et al., 2018; Ven et al., 2019). Although the mesocosm dimensions and sampling design differed and the 50/25/25% weighting therefore remains an approximation, the same procedure was applied consistently across all treatments, minimizing the risk of systematic bias between treatments.

We will add the following information in the manuscript:

“For each soil layer, it was assumed that the core from the center of the mesocosm represented 50% of the root distribution, while each of the two cores beneath the plants contributed 25%. The spatial sampling approach was based on previous maize experiments in mesocosms, in which root biomass was sampled both beneath and between plants to obtain a spatially representative estimate of root biomass (Verlinden et al., 2018; Ven et al., 2019).”

Line 113 - chemical analysis of biomass needs quality control - accuracy and precision

Line 143-144: “using certified standards (CPAChem) as quality controls every 20 samples and Hay Powder BCR129 as Reference Material (CPAChem). The recovery for all elements fall between 90% and 105%.” added

Line 119 - similarly for soil analysis

Line 170-174: For pore water, we will add the following in the manuscript: “Measurements on the ICP-OES broadly followed the protocol of Schuessler et al 2016. Repeated measurements of in-house matrix-matched reference materials were used to verify lack of drift throughout the analytical sessions and yielded repeatabilities typically <2-5% RSD, depending on the element. Accuracy was verified through analysis of certified reference materials, including SLRS-6 and USGS-T187 river waters and BHVO-2 digests, and yielded results within 5% of certified values for the major elements”

Line 152 - why wasn't presence / absence of earthworms and basalt included in your model for explaining differences in plant biomass?

We thank the reviewer for this consideration. The effect of basalt and earthworms on plant biomass was tested (see line 140 and Table 3 of the original manuscript). No significant effect was detected and we therefore added the analysis described in line 152 of the submitted manuscript was to investigate whether variation in plant biomass could instead be explained by other underlying factors.

Fig 1 - state what the error bars represent and give a value for n, i.e. number of reps.

In the revised manuscript, we will include that the error bars represent the standard deviation across mesocosms. We will also include the number of mesocosms per treatment.

Line 167 - list the exceptions

We thank the reviewer for this comment. The exception was actually in mesocosms that were not part of the treatments discussed in this manuscript. No casts were found in the mesocosms that were used for this manuscript. We deleted the “with a few exceptions”.

Line 168 - perhaps the detail is in the SI but here give average no. of earthworms recovered and average weight change between start and end of experiment. Initial mass of earthworms should have been added to the methods section

We thank the reviewer for this comment. Unfortunately, we did not weigh the worms at the beginning. We randomly added 10 individuals of the two species of worms to each pot, but the weight was not written down.

The number and dry weight of the earthworms at the end of the growing season is given in the SI (table S2a and S2b).

Line 197 - was this increase significantly greater?

It was not significant, but there was a tendency of higher pH. We therefore used the wording ‘tended to be higher’ to indicate the observed direction of the response without implying a significant treatment effect.

Fig 2, 3 (all figures) - the “*” are not needed as you show the p values. - typically these sorts of subscripts are used to show significant differences between treatments. Need to state what the error bars represent and always ensure number of replicates (n) is given (sometimes it is but e.g. Fig. 8 it isn't).

We thank the reviewer for notifying. Changes made:

- The number of replicates and the representation of the error bars is added for all figures.
- * is deleted

Line 351 - go back to the Methods section and state how limits of quantification were determined and list what these were for all analytes in the SI.

In the revised manuscript, we will add the following:

“Measurements on the ICP-OES broadly followed the protocol of Schuessler et al 2016. Repeated measurements of in-house matrix-matched reference materials were used to verify lack of drift throughout the analytical sessions and yielded repeatabilities typically <2-5% RSD, depending on the element. Accuracy was verified through analysis of certified reference materials, including SLRS-6 and USGS-T187 river waters and BHVO-2 digests, and yielded results within 5% of certified values for the major elements.”

Typically if concentrations are below quantification you can set the concentrations at limit of detection divided by the square root of 2 - did you do this and then carry out your statistical

analysis for e.g. Ni. (Croghan, C. & Egeghy, P. P. 2003. Methods of Dealing with Values Below the Limit of Detection Using Sas. 602 *Southern SAS User Group*, 22, 24.)

We thank the reviewer for the suggestion. We used the following when concentrations were below the detection limit: detection limit/2, according to [10.1897/03-365](#). The assumptions were still not met after we did this, hence a correct statistical analysis was not possible.

Line 404 - it's probably worth reminding the reader here which variables were included in the PCA but didn't appear in PC1 or PC2, e.g. pH

We thank the reviewer for this comment. The components that were not part of PC1 or PC2 were added:

Line 513-514: Factors that were included in the PCA, but were not significant contributors to differences in biomass, were Al, Fe, S, pH, and Zn.

Fig 9, 10, 11, 12, i.e. all box plots - need to state what the various whiskers, tails, horizontal lines represent in the caption

We will add the following information to all box-plots: "Boxes represent the interquartile range (25th–75th percentiles), horizontal lines within boxes indicate the median, and whiskers extend to the most extreme observations within 1.5 x the interquartile range. Points beyond the whiskers represent potential outliers."

Discussion

Line 723 - careful with the word "synergistic" - do you mean that the effect of basalt and earthworms was greater than the sum of the individual effects, or was the effect additive, i.e. the effect of basalt and earthworms was equal to the sum of the individual effects, or was the effect of the two, less than the sum of the individual effects. Synergism is a technical term and should be used correctly.

Line 749: "Synergistic" deleted, changed by "positive relationship between"

Line 725 - it is highly unlikely that simply burrow breaks down silicate minerals - what evidence is there for this and do you mean chemically or physically? If you mean chemically then yes moving basalt to a different environment may accelerate weathering but there is no reason why burrowing and resultant displacement of silicate minerals (or basalt grains) would result in their physical breakdown.

There is a little evidence in the literature of earthworms reducing mineral size - the paper of Suzuki et al is rather unconvincing, I've yet to meet an earthworm ecologist who believes the results, people think the results are an experimental artefact of either preferential feeding or retention of particles in the earthworm gut. The Carpenter et al papers on earthworms and mineral weathering do hint at some grain size reductions, but not consistently.

We agree with the reviewer and will make the following changes on line 749-751: "The burrowing activity of earthworms can break down the silicate materials by moving the materials throughout the soil, while also increasing soil porosity and water infiltration (Six et al., 2004)."

Line 747 - but I don't think you've told us which ecotype (or species) of earthworm were used in your study.

We thank the reviewer for this comment. The earthworm species are now added in the materials and method section.

Line 758 - first mention of watering of the mesocosms - this detail should be mentioned in the methods. Thinking about it, it would also be useful to document the leachate volumes collected over time, it would be interesting to see if there was more leaching in the earthworm treatments, so please incorporate this into your Results section.

The leachates can be consulted in the supplementary information (Figure S12). As there was no significant difference among treatments, we decided not to add these to the result section.

Line 762 - 765 - I don't follow the logic of why, just because respiration has increased in your mesocosms you think earthworms would migrate to these areas in the field. You could argue that an increase in CO₂ (which has a negative impact on earthworms) would decrease migration into such areas.

We thank the reviewer for this question. It is not the increased CO₂ that attracts the earthworms, but the increased microbial activity, as there are generally more nutrients available due to microbial breakdown of organic matter. We will clarify this in the revised manuscript:

Line 782-785: In our mesocosms, however, lateral movement toward treated plots was not possible, preventing such potential responses. Soil respiration increased during the first year following basalt application (Boito et al., 2025), consistent with increased biological activity and organic matter decomposition. Increased microbial activity and associated decomposition products can provide food resources for earthworms and may therefore attract them to such areas. Under field conditions, where earthworms can migrate between treated and untreated plots, this mechanism could contribute to responses similar to those reported by Dupla et al. (2024).

Line 800 - 805 - more sustainable agricultural practises will also likely elevate soil organic matter content

We agree with the reviewer that more sustainable agricultural practices may also increase soil organic matter content. However, evaluating the effects of different agricultural management practices was beyond the scope of the present study, and we therefore prefer not to expand the Discussion in this direction.

Line 880 - but in nutrient poor or degraded soils how likely is it that there will be a thriving earthworm population?

We thank the reviewer for this reflection. The sentence will be modified as follows:

line 913-916: Thus, while effects may be stronger in nutrient-poor or degraded soils, their magnitude will depend on whether earthworm populations are present or able to re-establish under such conditions.

The main thing missing from this study is, I think, an attempt at mass balance. You have pore water data, leachate data, exchange site data and plant composition and biomass data so you could attempt a mass balance similar to that of Kelland et al (2020) *Global Change Biology* 1 – 19. [Doi.org/10.1111/gcb.15089](https://doi.org/10.1111/gcb.15089) to determine a) basalt weathering rates and b) whether earthworms increase these or not.

We agree that a complete elemental mass balance would be valuable for quantifying basalt dissolution over time and assessing whether earthworms altered dissolution rates. However, quantifying basalt dissolution or weathering rates was not an objective of this study. Rather, repeated porewater measurements within the basalt-amended topsoil were used to assess temporal changes in nutrient availability and weathering-related element dynamics. Although several relevant elemental pools and fluxes were measured, these measurements do not provide a complete and sufficiently constrained basalt-derived elemental budget from which dissolution rates can be robustly calculated. In fact, porewater and leachate base cation concentrations as well as plant base cation uptake remained negligible (< than 0.3% of added base cations, Steinwider et al. 2026). While exchangeable base cations have been used to for weathering rate calculations, Steinwider et al. 2026 also illustrate that substantial fractions of released cations were retained in different soil pools, thereby highlighting the uncertainty associated with closing a complete elemental mass balance. We have therefore clarified this limitation and restricted our interpretation to whether the observed elemental responses are consistent with enhanced basalt weathering.

Line 782-785: “Porewater concentrations reflect the net balance between mineral dissolution and subsequent retention in soil pools, plant uptake, and downward transport, and therefore cannot be directly translated into quantitative basalt dissolution rates. Thus, while the observed porewater responses are consistent with enhanced basalt weathering in the presence of earthworms, changes in the partitioning of released elements may also have contributed to these patterns.”

Reviewer 2

The authors report the results of an experiment to investigate the potential impacts of basalt and earthworms, and their interaction, on plant nutrient availability and heavy metals in soils. Overall, results show an increase in pH and alkalinity in pore water induced by basalt application, with earthworms slightly enhancing these effects. The study presents an interesting dataset addressing key knowledge gaps in enhanced weathering. However, there are methodological questions that need to be addressed.

Following the application of basalt, the dynamics of alkalinity are not easy to track, as they involve not only dissolved species in pore water, but also species held on the cation exchange complex, in turn affecting the outflux through leaching or the uptake by plants. The authors appear to have focused mainly on concentrations in pore water and in plants. However, I am wondering whether this is sufficient to fully interpret the results, given that the full mass balance for each element probably cannot be resolved, especially without an estimate of how much basalt has dissolved over time.

We thank the reviewer for this important comment. We agree that porewater concentrations alone cannot provide a complete picture of basalt weathering, as released elements may subsequently be retained on soil exchange sites or in other soil pools, taken up by plants, or transported downward and lost through leaching. Consequently, porewater concentrations reflect the net outcome of several interacting processes and cannot be directly translated into quantitative basalt dissolution or weathering rates.

Although several relevant elemental pools and fluxes were measured in this experiment, including porewater, plant biomass and composition, leachate, and exchangeable soil pools, these measurements do not provide a complete and sufficiently constrained basalt-derived elemental budget from which dissolution rates can be robustly calculated. A comprehensive elemental mass-balance analysis aimed at quantifying basalt dissolution rates would require additional assumptions and analyses and is beyond the scope of the present paper, which focuses on nutrient and metal availability and their response to earthworms. Complementary analyses from parts of the same experiment showed that substantial fractions of released cations were retained in different soil pools and also highlighted the uncertainty associated with closing a complete elemental mass balance (Steinwider et al., 2025).

We have therefore clarified this limitation in the Discussion and restricted our interpretation to whether the observed elemental responses are consistent with enhanced basalt weathering:

Line 782-785: "Porewater concentrations reflect the net balance between mineral dissolution and subsequent retention in soil pools, plant uptake, and downward transport, and therefore cannot be directly translated into quantitative basalt dissolution rates. Thus, while the observed porewater responses are consistent with enhanced basalt weathering in the presence of earthworms, changes in the partitioning of released elements may also have contributed to these patterns."

A similar question arises regarding the dynamics of heavy metals. These have a complex speciation, of which dissolved forms are only one component, highly dependent on pH. Would it

not be essential to measure heavy metal content more comprehensively (with different extraction methods) to understand their fate once released from the basalt? Also, it is not clear whether background concentrations in the basalt and in the soil were measured.

We thank the reviewer for this comment. We agree that determining the full fate of metals released from basalt would require characterization of different soil metal pools and their speciation. However, such a comprehensive assessment was beyond the scope of the present study, which focuses primarily on metal availability in porewater and metal concentrations in plant tissues. Porewater measurements were used to assess the dissolved and potentially plant-available fraction, rather than to quantify the total amount of metals released from basalt or their distribution among soil pools.

The initial soil was not analysed. However, we have metals analysed starting from day 21 of the experiment. Here, we also have control treatments without basalt. We always compare our treatments with the control treatments. So even though we do not have the absolute starting concentrations in our soil, differences among treatments provide insight into potential changes in heavy metal concentrations.

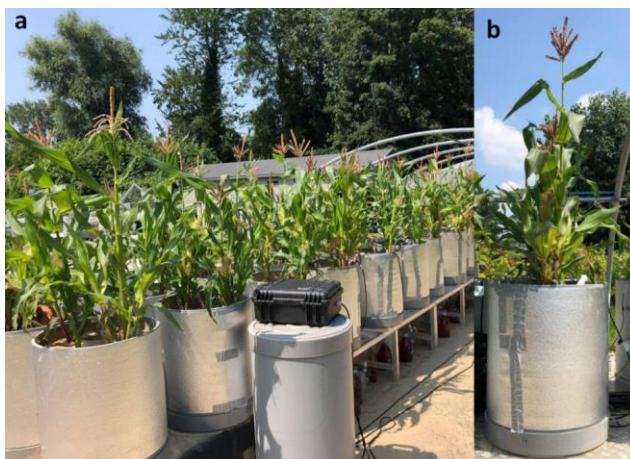
The background concentrations of the basalt can be found in table S1.

Lastly, various methodological details are missing. For example, the manuscript is missing the basic characterization of the soil. How was the soil collected (undisturbed or disturbed samples)? What about the bulk density and texture? What about the physical and chemical properties of the basalt? It would also be helpful to see a diagram and/or picture of the experimental setup.

Thank you for your comment. The following changes were made to clarify:

Line 109-110: “At the first harvest, destructive soil sampling, using a soil auger, was conducted on four mesocosms per treatment.”

A figure is added of the mesocosm experiment: Figure 1:



The basalt composition is shown in table S1. I added a picture of the experimental set-up.

Below are some additional minor points.

The introduction could be expanded to reflect the evolving literature on enhanced weathering and better connect the knowledge gaps.

We thank the reviewer for this suggestion. We will expand the Introduction to better reflect the rapidly growing experimental literature on enhanced weathering. In particular, we will highlight that although EW can increase soil pH, nutrient availability, and crop performance, responses vary substantially depending on soil properties, feedstock characteristics, application rates, and environmental conditions, and crop biomass and yield responses are not consistently positive across studies (Suhrrhoff et al., 2026). This broader context helps clarify the remaining knowledge gap concerning biological controls on EW, particularly the largely unexplored role of earthworms. The introduction will be elaborated as follows:

“The experimental evidence base for EW has expanded considerably in recent years. While rock amendments frequently increase soil pH and can improve nutrient availability and crop performance, responses vary substantially among studies depending on factors such as initial soil properties, feedstock characteristics, application rate, and environmental conditions (Suhrrhoff et al., 2026). Crop responses are similarly variable, with positive, neutral, and negative biomass or yield responses reported across experiments (Suhrrhoff et al., 2026).

In tropical regions, positive effects of silicate rock applications on crop growth and nutrient supply have been found to prevail (Swoboda et al., 2022), while evidence for temperate climates remains inconclusive. A few EW experiments in temperate climates reported increased crop growth for basalt and dunite application alongside changes in plant elemental composition, including increased Ca, Mg, and Si uptake (Rijnders et al., 2023, 2024; Skov et al., 2024). Other studies reported no biomass response to silicate application (Te Pas et al., 2026; Rineau et al., 2026), or even decreased plant biomass (Anthony et al., 2025). These inconsistent findings suggest that not only climatic region, but also other factors like soil properties play a role.”

Lines 30-35. Please remove the placeholder text (“Click or tap here...”).

We thank the reviewer for noticing and will remove these placeholders.

Lines 86-87. Mention here what is a typical population for this soil, for context.

Line 104-107: added “The treatments with earthworms received 20 individuals (ten *Aporrectodea caliginosa* and ten *Aporrectodea rosea*), collected nearby the Drie Eiken campus of the University of Antwerp. Earthworm density was selected based on average earthworm density in agricultural soil in Europe, taking into account that only two species of endogenic earthworms are added (Dinter et al., 2013; Rutgers et al., 2016). “

Lines 108-109. But should it not be relatively straightforward to obtain actual root distribution?

We thank the reviewer for this comment. Determining the actual root distribution would require recovering and separating roots from the entire soil volume of each mesocosm, which was not feasible given the number of mesocosms and soil volume involved. We therefore estimated root biomass from spatially distributed subsamples taken at different soil depths and positions within each mesocosm. This spatial sampling approach was based on previous maize mesocosm experiments, in which root biomass was sampled both beneath and between plants to obtain a spatially representative estimate of root biomass (Verlinden et al., 2018; Ven et al., 2019). While the subsequent extrapolation of root distribution remains an approximation, the same sampling and calculation procedure was applied consistently across all treatments, minimizing the risk of systematic bias between treatments. We have clarified this approach and added the relevant references to the Methods.

Lines 725-728. I would expand on this comparison.

We will expand this comparison as follows:

Revised text:

Our results differ from those of Calogiuri et al. (2025), who found no positive effect of surviving earthworms on weathering indicators in eight-week artificial organo-mineral column experiments. Calogiuri et al., (2025) reported that surviving earthworms decreased several indicators, including DIC, TIC, Ca, and Si. They suggested that the relatively short experimental duration may have limited the detection of earthworm-induced weathering and that increased retention of weathering products could have contributed to the observed decreases. In contrast, earthworm mortality increased most weathering indicators, which was attributed to microbial processes associated with decomposition of earthworm biomass. Differences from our results may therefore partly reflect the substantially longer duration and more soil-realistic conditions of our experiment, in which earthworm effects could develop over two growing seasons in a planted soil system.

Line 846. Were Cr and V measured in the basalt?

Yes, Cr and V were measured in basalt, see table S1. These were present in basalt, but in minor concentrations (367 and 242 ppm, respectively).