Editor's comments

We thank the editor for her insightful suggestions which have helped improve this manuscript's abstract. We edited the abstract to follow her suggestions. In blue are the authors' responses, and in *italic* are the manuscript excerpts.

I have a few minor comments for the abstract myself:

- to increase readability even further, please make clear in L18 that the sentence "Basalt application, but not AMF, improved soil pH, ..." is about the results.

Thank you for this comment, the sentence has been edited to *"In our experiment, basalt application, but not AMF, improved soil pH…"* (L18).

- agree with reviewer 1 that the abstract will benefit from reporting on quantitative results (even if the point of the ms is to investigate whether crop responses to enhanced weathering exist beyond the context of nutrient-poor and/or acidic soils)

In line with the editor's and the reviewers' comment, the abstract has been improved by adding quantitative results on basalt effects on soil pH, CEC, base saturation, exchangeable Ca and Mg, and pore water and corn Ni.

- L23: 'specific environmental conditions' are mentioned, but it is not clear what these are compared to this study. It probably has to do with my previous point about the context of nutrient-poor etc soils, which was also made by reviewer 2 and to which you replied in a clear manner.

Thank you for making this suggestion, which helped improve the abstract. The section has been edited to clarify the soil conditions: *"These results suggest that risks associated with heavy metal contamination are not generic, but may depend on local soil conditions. In our experiment, elevated background Ni concentrations likely contributed to pore water Ni concentrations exceeding the environmental quality standards of the EU and Australia even in the control treatments."* (L25).

- the last sentence of the abstract also relates to this context specificity; if the suggestions of reviewer 1 are followed for the abstract, the last sentence will be more clear (perhaps adapt a bit for coherence)

The last sentence has been adapted following reviewer's suggestions, and it now reads *"Unlike several studies conducted in more acidic soils however, these chemical enhancements did not increase maize growth here, suggesting limited short-term agronomic benefits of basalt under the fertile soil studied here."* (L30).

Reviewer number 1's comments

We thank Xavier Dupla for the time and care he devoted to reviewing our manuscript; his feedback greatly strengthened it. Overall, his comments centered on adding quantitative data to support the changes we observed, thereby complementing the statistical significance already reported, and we believe the manuscript benefitted substantially from these revisions.

Throughout this document, in black are the reviewer's comments, in blue are the authors' responses, and in *italics* are the excerpts from the revised manuscript. Line numbers refer to the numbers in the clean, revised manuscript.

Abstract

Overall excellent, easy to read, clear and well structured. Too bad AMF did not lead to mind-blowing results but this needs to be said as much as positive results. Here are some line-to-line comments.

We thank the reviewer for this positive general assessment. We fully agree that less striking results deserve to be communicated with the same rigor and transparency as positive ones, and have aimed to do so throughout the manuscript.

l.15. Which soil type was it? Please add WRB name with an information on the soil pH.; l. 15. Please add application rate

We thank the reviewer for pointing out the missing information on soil type (Plaggic anthrosol) and application rate (50 ton ha^{-1}); this has been added to the revised manuscript (L15-16).

l.18-22. The result section would benefit from quantitative results, please add them.

Quantitative results for the changes in soil pH, cation exchange capacity, base saturation, exchangeable Ca and Mg, soil pore water and corn Ni content have been added to the revised abstract (L19-20, L24-25).

l.25. precise soil "chemical" fertility.

Throughout the abstract and manuscript, we now specified that we assessed soil chemical fertility, an important distinction raised by the reviewer.

l. 27. The reference to more acidic soils will make fully sense when the soil pH is mentioned earlier.

We fully agree with the reviewer's comment, and the suggested revisions including

starting soil pH greatly improved the abstract (L15).

Introduction

Another great section both clear, succinct and informative. The progression towards the research hypotheses reads very well. Please find what could be clarified below:

l.40-46. this is a little bit one-sided. Please also include the potential limits or risks (e.g., heavy metal accumulation, impact on infiltration rates, etc. For infiltration rates, see for instance the recent 2026 study from Akortey et al. in Nutrient Cycling in Agroecosystems. https://link.springer.com/article/10.1007/s10705-025-10452-2

We thank the reviewer for this comment which helped provide a more balanced overview of both risks and potential co-benefits of enhanced weathering (EW). The text has been revised to reflect on potential risks of EW (e.g., loss of soil organic matter due to pH increase, reduction of water infiltration rates, heavy metal accumulation).

L53:" On the other hand, EW may also introduce undesirable side effects. These include the accumulation of heavy metals and reduced water infiltration, potentially resulting from the occlusion of (macro)pores by rock amendments which limit preferential flow pathways (Akortey et al., 2026; Dupla et al., 2023). Additionally, increases in soil pH may stimulate microbial activity, potentially accelerating organic matter decomposition and leading to net losses of soil organic carbon (SOC; Wang and Kuzyakov, 2024). Such SOC losses could not only reduce soil fertility, but also partly offset the carbon sequestration benefits of EW, as gains in inorganic carbon storage may occur at the expense of organic carbon stocks (Buss et al., 2024; Lei et al., 2025; Steinwidder et al., 2025). Consequently, these processes could diminish both the agronomic and CDR benefits anticipated from EW. However, the mechanisms governing these co-benefits and risks remain insufficiently constrained, particularly the biotic interactions that may modulate EW effects in soils."

l.50. the evidence is accumulating now on the fact that basalt will not be a major P source. You could however replace it with Silicon for which we have solid data.

In line with the reviewer's comment, phosphorus has been removed from the list of essential nutrients supplied by basalt, and the release of the beneficial element silicon has been included instead.

L64: "As an igneous rock rich in magnesium and iron silicates, it can also contain essential macronutrients such as calcium and potassium, along with important micronutrients and beneficial elements such as silicon (Conceição et al., 2022)."

l. 52 safer but heavy metal accumulation is still a risk (especially given your results on

Ni) especially given that basalt covers a range of mineral compositions that can go from very safe to concerning in terms of heavy metals. Please mention this.

The reviewer raises an important point, which we thank him for. The text has been revised to clarify that basalts can vary widely in mineralogy, and therefore in heavy metal accumulation risk, ranging from safe to concerning.

L65: "Compared to other (ultra)mafic silicate rocks like dunite, basalt contains lower concentrations of toxic heavy metals such as nickel and chromium, making it a safer option for agricultural use (Amann et al., 2020; Swoboda et al., 2022). Nevertheless, such risks are not negligible, given the substantial heterogeneity in basalt's elemental composition, which may lead to heavy metal concentrations spanning from safe to potentially concerning levels (Dupla et al., 2023)."

l.54-55 to be more objective I would add : "although local conditions, *feedstock selection and application rates* can impact these outcomes".

We fully agree with the reviewer and have revised the manuscript accordingly.

L71: "Basalt application may thus improve soil chemical fertility while minimizing contamination risks, although local soil conditions, as well as feedstock selection and application rates, can impact these outcomes."

l.56-64. Not sure about this so please do what you want with this. I am not a microbiologist but as this section was on mycorrhizal fungi, I was expecting to see a mention of ectomycorrhizal fungi. Even if it is just to say that their role is negligible for most crops, it could make your choice to focus on AMF look even more robust.

Ectomycorrhizal symbioses typically form between the fine feeder roots of woody perennial species and a specific range of soil fungi (Smith and Read, 2010), and are therefore largely irrelevant to annual crops. We thank the reviewer for raising this point, and added a few sentences to the manuscript clarifying the negligible role of ectomycorrhizal fungi in annual crop symbioses.

L75: "Among soil biota, mycorrhizal symbionts, broadly classified in ectomycorrhiza and arbuscular mycorrhizal fungi (AMF), play a central role in plant nutrition and soil processes. Ectomycorrhizal associations typically occur between the fine feeder roots of woody perennial species and a relatively specific group of soil fungi (Smith and Read, 2010), and are therefore largely irrelevant for most annual cropping systems. In contrast, approximately 80% of terrestrial plant species, including many important crops such as maize, wheat, and soybean, engage in a symbiotic relationship with AMF via their roots systems (Verbruggen and Toby Kiers, 2010; Wilkes, 2021)."

l.59. AMF acronym has just been introduced above (l. 56). Delete full name.

The full name has been removed.

l. 61. Add reference (100 m per cm -3).

The original reference appeared in the following sentence; the appropriate source has now been placed next to the data after unit conversion. The sentence has been further edited to satisfy reviewer 2's requests.

L80: "AMF extend beyond the root surface through extensive extraradical hyphal networks, with hyphal lengths commonly on the order of 0.5 to 5 m g soil^{-1} (Johnson et al., 2003; Sochorová et al., 2016), although substantially higher values (around 70 m of hyphae per g of soil^{-1}) have occasionally been reported (Miller et al., 1995)."

l.64. Exudation and enmeshment make sense but what do you mean exactly by turnover?

We thank the reviewer for pointing out this small imprecision; the text has been clarified to specify that the authors mean hyphal turnover (L88).

l.68. please clarify: by absorbing which nutrients?

We thank the reviewer for this insightful question. The text has been clarified to indicate that acidification of the soil surroundings is mostly due to ammonium uptake (Neumann and George, 2010; Taylor et al., 2009).

L93: "In addition, during ammonium uptake AMF release protons that acidify the soil surroundings, thus aiding the natural weathering process (Neumann and George, 2010; Taylor et al., 2009)."

l.73-74. contrary to the previous elements, this claim is less mechanistic and therefore harder to understand. Do you mean that heavy metal bioavailability is improved via their impact on pH (acidification) and via organic ligand release?

The main mechanism by which AMF affect (and specifically, decrease) heavy metal bioavailability is glomalin secretion. The reviewer is therefore correct: AMF excrete glomalin, which can chelate heavy metals and thereby reduce their mobility (Gonzalez-Chavez et al., 2004; Riaz et al., 2021). At the same time, the AMF-driven soil acidification might ultimately increase heavy metal bioavailability. We thank the reviewer for this insight, which helped sharpen the manuscript, and we have revised the text to be more specific on this point.

L97: "[…] Finally, AMF may influence the bioavailability and plant uptake of heavy metals through acidification and glomalin-mediated chelation (Gonzalez-Chavez et al., 2004; Riaz et al., 2021)."

l.76. the Verbruggen 2021 reference could lead readers to think that that Verbruggen et al. were the only one to do an ERW experiment with AMF before you. Please rephrase

to clarify.

We agree that this sentence could confuse readers and have rephrased it to clarify that this research gap was simply highlighted by Verbruggen et al. (2021) (L101).

l.79. Precise soil "chemical" fertility.

This has been corrected throughout the manuscript.

l. 80. So we have a problem here: ERW increase the pH but AMF lowers it… please rephrase to explain why you think one effect will be more important than the other one.

We thank the reviewer for catching this apparent inconsistency. To clarify, we do not hypothesize that the basalt effect on heavy metal availability outweighs that of AMF; rather, the acidifying effect of AMF could counteract the expected decrease in heavy metal availability caused by basalt-driven pH increases. As mentioned above, AMF may also modulate the transport of nutrients and heavy metals between soil and plant (e.g., through glomalin secretion; Gonzalez-Chavez et al., 2004; Riaz et al., 2021). The hypothesis has been revised to reflect this added nuance.

L102: "We hypothesized that i) AMF increases basalt weathering rates, and thus higher dissolved organic carbon (DIC) and alkalinity concentrations in the pore water and base cations in the soil-water complex are expected; ii) basalt application improves soil chemical fertility and therefore plant growth and nutrient uptake, and that this effect is modulated by AMF; iii) basalt addition does not increase heavy metal availability due to pH buffering, while AMF may counteract this effect through rhizosphere acidification and further affect heavy metal dynamics by altering nutrient and metal transport between soil and plant; iv) AMF growth responds to basalt application as a result of changes in soil nutrients and chemical fertility."

Methods

Another excellent section. Some moderate to minor points need to be clarified. See below. One element that I could not find (and that could help to interprete some of your results like the Ni increase even in the control) was the mineralogical and elemental profile of the soil itself. Do you have this information?

We thank Xavier Dupla for his positive assessment. We agree that including the mineralogical and elemental composition of the soil would provide useful context for readers. Unfortunately, we do not possess mineralogical data, but we do have some information on the elemental profile, which was added in a table to the supplementary materials (Table S1; also presented below as Table A). Details on the XRF method have been added to the table caption.

Table A: Soil elemental composition according to XRF. Measurements were carried out on milled samples using a WD-XRF spectrometer (Bruker S8 Tiger 4K).

Formula	Concentration (%)
SiO_2	83.13
Al_2O_3	8.70
Fe_2O_3	2.24
K_2O	1.95
Na_2O	1.09
P_2O_5	0.72
TiO_2	0.70
MgO	0.61
CaO	0.43

l.85. How were the pots distributed spatially to limit sun/shade exposure differences?

The mesocosms were located in an area free of trees, so all pots received sunlight at the same time; treatments were randomized across the experimental area to minimize spatial effects. This information has been added to the revised manuscript.

L119: "The experimental treatments were randomized within the facility in order to minimize differences in sun exposure, and mesocosms were wrapped in reflective foil to avoid excessive heating."

l.85. Just to be sure, the pots were never irrigated even during the "high temperatures" that you mention in l.178?

The mesocosms were, in fact, irrigated with rainwater stored in underground tanks, in addition to receiving natural rainfall directly. The text has been revised to clarify this point.

L161: "To prevent severe drought stress during dry spells, mesocosms were irrigated with rainwater collected in underground tanks. All mesocosms received

the same amount of water (285 mm).”

l. 88 give details on the exclusion mat (thickness, material, etc.).

Information about the exclusion mat (black woven polypropylene, weight = 100g m^{-2}; purchased at BRICO Belgium) was added to the revised manuscript (L117).

l.92. please give full WRB name including qualifiers. This is very important for reproducibility. If you need to be convinced of it, please check https://pubs.acs.org/doi/10.1021/acs.est.9b03050

We fully agree on the importance of including standardized soil names. This information (Plaggic anthrosol) was added to the revised manuscript (L127).

l.93. pH H2O? which soil : solution ratio?

The soil:water ratio used was 1:2.5; this information has been added to the method section (L129 and L193).

L129: "(mean pH ± SE = 5.6 ± 0.3; soil : water = 1:2.5, see further for detailed method).”

L193: "Topsoil pH was measured by collecting, pooling and air-drying three subsamples per mesocosm. Subsequently, 4 g of soil was dissolved in 10 mL deionized water (soil : water = 1:2.5) and shaken before pH measurement.”

l.94. how were the SOC and CEC measured? Method and machine. Try to provide as much details for these and for XRD and XRF, and amorphous phases, as you have done for BET.

We fully agree on the importance of adding as much detail as possible concerning SOC, CEC, XRD and XRF measurements, and have improved the manuscript accordingly. SOC was determined by loss-on-ignition at 550 °C for four hours. Soil potential CEC and base saturation were determined via 1 M ammonium acetate (NH_4OAc) displacement at pH 7.0, using ICP-OES (iCAP 6300 Duo; Thermo Fisher Scientific).

A recent feedstock XRD analysis was performed by Qminerals (Bruker D8 Advance, Cu Kα radiation at 30 mA and 40 kV, 2θ from 5° to 70°). For this, an internal standard (10 wt% Al_2O_3) was added during sample preparation to determine amorphous phase content, and samples were milled in ethanol for 10 minutes. XRF data were provided by RPBL, who did not possess further details on this analysis as it was carried out by a third party. These details have been added to the revised manuscript.

L128: "The initial soil was slightly acidic (mean pH ± SE = 5.6 ± 0.3; soil : water

= 1:2.5, see further for detailed method), with an organic carbon content of 0.8 ± 0.05% (mean ± SE; determined by loss-on-ignition at 550 °C for 4 h) and a potential cation exchange capacity (CEC) of 3.9 ± 0.6 meq 100 g soil-1 (mean ± SE; see further for method).”

L191: "Measurements of soil chemical fertility (here defined as soil pH, CEC and base saturation) were performed on soil samples collected before basalt application and on topsoil samples (0-20 cm) collected at the end of the experiment (12 September 2022). Topsoil pH was measured by collecting, pooling and air-drying three subsamples per mesocosm. Subsequently, 4 g of soil was dissolved in 10 mL deionized water (soil:water = 1:2.5) and shaken before pH measurement. Soil potential CEC and base saturation were determined by 1M ammonium acetate (NH_4OAc) displacement at pH 7.0 using ICP-OES (iCAP6300 Duo; Thermo Fisher Scientific; Brown, 1943).”

L139: "Basalt mineralogy was obtained through XRD analysis on samples milled in ethanol for 10 minutes (Bruker D8 Advance, Cu Kα radiation at 30 mA and 40 kV, 2θ from 5° to 70°). An internal standard (10 wt% Al_2O_3) was added during sample preparation to determine amorphous phase content. Basalt elemental composition (XRF) was provided by RPBL.”

L95:please add exact mine location coordinates as RPBL has several mines.

The coordinates of the mine (50°41.4833' N 7°19.1198' E; RPBL – Werk Hühnerberg, Willmerother Str. 50, 53639 Königswinter-Oberpleis) have been added to the revised manuscript (L134).

Table 1.

What are these “vs” mentions?

We thank the reviewer for spotting this editing error. The mineralogy table has been revised to reflect newest XRD data: since submitting the manuscript, a more accurate XRD analysis of feedstock mineralogy was performed by Qminerals, and therefore Table 1 has been revised to contain the most precise quantification.

How was the amorphous phase measured?

As mentioned in a previous comment, this information has been added to the revised manuscript table caption (Table 1).

Note that this level of precision (2 digits) is not realistic.

We thank the reviewer for this insightful comment. Indeed, two decimal places is not a realistic level of precision. Qminerals has confirmed an average precision of 0.2%, with an accuracy of 0.3% for non-layer silicates and 1.2% for layer silicates. The XRD data has been revised to reflect this more realistic accuracy level.

Revised Table 1: Left: basalt mineral phases obtained via XRD analysis on samples milled in ethanol for 10 minutes (Bruker D8 Advance, Cu Kα radiation at 30 mA and 40 kV, 2θ from 5° to 70°). An internal standard (10 wt% Al_2O_3) was added during sample preparation to determine amorphous phase content. Right: elemental concentrations obtained via XRF analysis, provided by RPBL.

Mineral	**Mineral Formula**	**Basalt**		**Basalt**
		%		%
SILICATES			Si	20.8
K-feldspar	$KAlSi_3O_8$	6.7	Mg	7.8
Plagioclase	$(Na,Ca)(Si,Al)_4O_8$	7.5	Fe	4.1
Clinopyroxene	$(Ca,Na)(Mg,Fe,Al,Ti)(Si,Al)_2O_6$	28.8	Al	3.0
Olivine	$(Mg,Fe)_2SiO_4$	13.6	Ca	7.7
Nepheline	$(Na,K)AlSiO_4$	4.2	Na	1.0
Sodalite	$Na_8Al_6Si_6O_{24}Cl_2$	1.4	Ti	1.4
Analcime	$NaAlSi_2O_6•(H_2O)$	1.9	P	0.1
2:1 layer silicates	$(K,H_3O)(Al,Mg,Fe)_2(Si,Al)_4O_{10}[(OH)_2,H_2O]$	9.1	K	0.3
		73.3	Mn	0.2
CARBONATES				
Calcite	$CaCO_3$	0.1		
OTHER/AMORPHOUS				
Amorphous		26.6		

l.95-96. please explain why you only applied it to the top 20cm (to mimic basalt incorporation through tillage I assume).

The reviewer is correct; the text has been edited to clarify the rationale.

L132: "To mimic amendment incorporation through ploughing, the top 20 cm of the basalt treatments (n = 10 mesocosms) received pasteurized soil with a mixture of

0.98 kg (equivalent to 50 ton ha^{-1}) of freshly mined Eifel Basalt (Table 1; Rheinischen Provinzial-Basalt- un Lavawerke (RPBL) […]"

l.107. how was the inoculation done? Add details and doses.

Dry inoculum (40 g) was suspended in 200 mL of deionized water in a glass beaker and homogenized. Seeds were then added, mixed thoroughly to ensure even coating, after which the seeds were sown. These details have been added to the revised manuscript.

L147: "For half of the treatments (five control and five basalt mesocosms), seeds were inoculated with AMF (Rhizophagus irregularis; Symplanta) prior to sowing. Dry inoculum (40 g) was suspended in 200 ml of deionized water in a glass beaker and thoroughly homogenized. Seeds were subsequently added to the suspension and mixed thoroughly to ensure a uniform coating of the inoculum on the seed surface. The remaining 10 mesocosms (five control and five basalt treatments) received a sterilized inoculum (Table 2), obtained by suspending 40 g of dry inoculum in 200 ml of deionized water in a glass bottle and autoclaving the inoculum at 121 °C for 25 minutes."

Fig. 1. Heteroscedaticity is clearly visible on these plots. Please either mention that you took it into account or refer to the statistical section as this figure appears much earlier than the stats section.

We thank the reviewer for this comment. Indeed, variance was not equal across treatments; however, a square-root transformation satisfied the model assumptions of normality and homoscedasticity, and a non-parametric test (Kruskal-Wallis) yielded the same results as the ANOVA test. The transformation applied has been detailed in the figure caption.

Figure 1 caption: "[…] Compliance with model assumptions of normality and homoscedasticity was ensured by square-root transformation."

L.129. Precise soil "chemical" fertility.

This has been corrected throughout the manuscript.

l. 135. Precise the probe that was used for pH.

This information has been added to the revised manuscript (L179).

l.142. Were samples acidified prior to ICP-OES analysis?

Yes, samples were acidified with 10 ml of 65% nitric acid (HNO_3). This detail has been added to the manuscript.

L185: "Elemental analyses were performed by inductively coupled plasma-optical

emission spectrometry (ICP-OES; DSOI SpectroGreen, SPECTRO) in the Helmholtz Laboratory for the Geochemistry of the Earth Surface (HELGES) on samples filtered through a disposable 0.2 µm PET-20/25 membrane filter (CHROMAFIL, Macherey-Nagel) and acidified with 10 ml 65% nitric acid (HNO_3), using international water reference material SLRS-6 (NRC-CNRC) as quality control."

l. 151. Please add details on CEC measurement (to be consistent with the other parameters that you described).

This comment has been addressed together with the previous comment on the CEC method.

l.156. the fact that you waited 9 days before the initial sampling implied that some of the most reactive fraction may have already dissolved. I imagine that you did not wait for 9 days on purpose so maybe you could justify how you ensured that the initial measured was still robust (e.g., by comparing the control and basalt treatments).

The reviewer's point is valid: weathering rates are typically highest immediately after feedstock application. Our weathering rate calculations therefore missed the initial (day 0-9) cation weathering, as shown by a newly added supplementary figure presenting Ca, Mg, K, and Na contents in each sequentially extracted pool at day 9 and day 113 (Figure S11; also below as figure B). In this figure, cation contents at day 9 are sometimes higher in basalt treatments than in the control treatments (especially for Ca, and for Mg in most pools), a pattern that can be explained by either of two non-exclusive possibilities: that the sequential extraction method partially extracted un-weathered primary minerals, or that some weathering had already occurred by day 9.

As a result, weathering rates have potentially been underestimated. That said, this manuscript's central hypothesis concerns whether AMF can enhance weathering rates, rather than estimating absolute weathering rates values. Since fungal hyphae take time to develop, as also shown by pore water DIC dynamics (Fig. 3), AMF-driven silicate weathering is unlikely to have occurred between day 0 and 9. Our method and estimation therefore remain valid for addressing our central hypothesis; however, they warrant a more cautious interpretation when considered as standalone estimates of weathering rates.

This potential underestimation of weathering rates, and the manuscript's focus on the AMF effect on weathering rather than on weathering rates *per se*, have been more strongly emphasized in both the method and discussion sections.

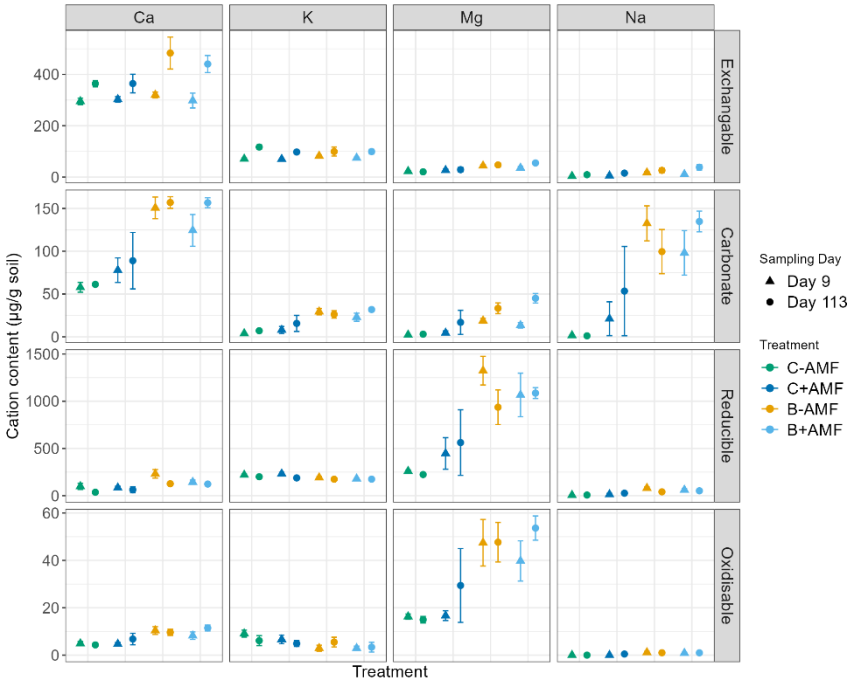


Figure B: average Ca, K, Mg and Na content in the four sequentially extracted soil pools at day 9 (triangles) and 113 (circles) for each treatment. Error bars represent the standard error on the mean. Several small error bars are not visible due to overlap with data points.

L244: "Because no soil samples were collected immediately after amendment (day 0), samples obtained at day 9 were used as the initial soil + basalt mixture. As weathering rates are highest immediately after rock application, this approach likely underestimates the true weathering rates. Nevertheless, the day 9 measurements still provide an appropriate baseline for testing our central hypothesis, namely whether AMF enhance basalt weathering rates. Although some weathering occurred before the first sampling event (Fig. S11), AMF-driven weathering is unlikely to have contributed substantially during this period because fungal hyphae require time to establish. This interpretation is supported by the pore water DIC dynamics (Fig. 3), which indicate that AMF activity increased progressively throughout the experiment rather than immediately after inoculation. Consequently, while the use of day 9 samples likely results in conservative estimates of absolute weathering rates, it does not bias our assessment of the relative effect of AMF on weathering."

L410: "Because the first soil samples were collected nine days after basalt application, these values likely underestimate absolute weathering rates, as mineral

dissolution is generally fastest shortly after amendment. However, this limitation is unlikely to affect the comparison between AMF treatments: AMF colonization develops gradually following inoculation, making a substantial AMF contribution to weathering during the first nine days unlikely. Thus, while the absence of day-0 measurements introduces uncertainty into the absolute weathering-rate estimates, it should not bias our conclusion that AMF did not measurably enhance basalt weathering."

l.177-178. Precise what you mean by high temperatures and minimal volumes.

Thank you for flagging this imprecision. The high summer temperature (mean summer temperature = 19.6 °C, compared to the 17.9 °C average between 1991 and 2020; Royal Meteorological Institute, 2022) and low leachate volume values (0.7 ± 0.01 l, mean ± se over the entire experimental duration) have been specified in the text.

L222: "During the experiment, high temperatures (mean summer temperature = 19.6 °C, compared to the 17.9 °C average between 1991 and 2020; Royal Meteorological Institute, 2022) were registered, and leaching occurred only at the beginning of the experiment and in minimal volumes (mean ± SE = 0.7 ± 0.1 l; Table S10), as most of the water in the mesocosms was lost through evapotranspiration driven by plant uptake and atmospheric evaporative demand."

l.183. this is confusing to me. I understand that you did

> [additional cations from basalt] = [cation]basalt treatment (with or without AMF) –[cation]control without AMF treatment

But these cation concentrations were measured in leaves right? If so you could do two things: 1) just add "To determine the amount of each cation released from the applied basalt and taken up by plants, ***leaf*** cation concentrations in the C-AMF treatment were subtracted" and 2) more importantly, move the plant measurement above the weathering rate section. That way the reader will not be surprised when you talk about cation concentrations in leaves. We will have already red that you did that and how you did that.

We thank the reviewer for this comment, which helped clarify the calculations in the manuscript. We adopted the reviewer's first suggestion and specified that, to estimate cation uptake by plants, **leaf** cation concentrations in the control treatment were subtracted from those in the other treatments (L229).

Regarding the second suggestion (i.e., moving the plant measurements above the weathering rate section) we agree this would improve the section's flow, but it would also disrupt the overall logical structure of the method section, which currently follows soil > weathering > plants > AMF. We instead resolved this by

clarifying in the text that plant measurements appear later.

L229: "To determine the amount of each cation released from the applied basalt and taken up by plants (ΔCat_{plant}), leaf cation concentrations in the C-AMF treatment ($Cat_{C_AMF(leaf)}$; see section 2.5 for method) were subtracted from cation concentrations in basalt-applied treatments ($Cat_{treatment(leaf)}$; Eq. 1)."

l.186. you know what I think now of this day 9 results interpreted as the initial data. Again if you can demonstrate that the most reactive particles did not weather before day 9, that would be a real plus.

Unfortunately, we are unable to demonstrate that, as no measurement was taken of the un-weathered feedstock + soil mixture at day 0. In fact, the additional Figure B (above, and in the revised supplementary material as Fig. S11) shows that likely some weathering has already taken place, given the often higher values in basalt mesocosms compared to control ones at day 9.

On the other hand, as also stated above, this study was designed primarily to determine whether AMF inoculation significantly alters feedstock weathering, rather than to characterize absolute weathering dynamics. Given that fungal colonization develops gradually following inoculation, we contend that a day-0 baseline, while it would add completeness to the dataset, would not alter the manuscript's central finding: that AMF inoculation did not measurably enhance feedstock weathering. As previously mentioned, the discussion has been expanded to reflect on this (L410).

l.175-196. All these steps would be much clearer with a few equations rather than sentences.

We thank the reviewer for this valuable suggestion; the equations used to estimate weathering rates have been added to the manuscript (L234, 240, 265).

l.215. specify the digestion method.

The text was amended with detailed information on the digestion method. Dried, ground plant samples were digested with 70% HNO_3 (60°C for 30 min) and 30% H_2O_2, then heated to 120 °C for 90 minutes. After cooling and filtering, concentrations were measured by ICP-OES (Thermo Scientific, iCAP 6300 Duo). Calibration standards (CPAChem multi-element standard) and internal standards (Yttrium, Merck) were prepared the same way, with matching HNO_3 and H_2O_2 concentrations.

L285: "Plant samples were analysed for nutrients (C, Ca, Fe, K, N, Mg, P) and heavy metals (Al, Cd, Cr, Ni, Pb, V and Zn). Dried, homogenized plant material was subjected to acid digestion with 70% HNO_3 at 60 °C for 30 min, followed by the addition of 30% hydrogen peroxide (H_2O_2) and further heating at 120 °C for 90 minutes. After cooling and filtering, elemental concentrations were measured by ICP-OES (Thermo Scientific, iCAP 6300 Duo). Quantification was based on multi-element calibration standards

(CPAChem), with yttrium (Merck) used as an internal standard. A certified reference material (Hay Powder BCR129, CPAChem) was processed alongside the samples using identical digestion procedures. Quality control checks were conducted every 20 measurements to ensure analytical precision and accuracy. Plant Ni, Cr and Mg results are reported in the main text, while concentrations of all other elements are reported in the supplements (Fig. S12). Biogenic Si was determined by digestion of a 0.03 g plant sample with 25 ml 0.5 M sodium hydroxide (NaOH) and then analysed with a continuous flow analyser (SAN++® Advanced Series, Skalar) following Demaster (1981); and Saccone et al. (2007).”

l.231. this is the last time that you talk about mesh bags and the only place where you mention Fig.S11. Consider removing this if not used in the main study.

While we agree that the mesh bag results are not central to the manuscript's main message, we consider the mesh bag data (i.e., hyphal length) highly relevant, as they demonstrate successful AMF colonization, and we therefore propose to retain them in the main text. The detailed mesh bag analysis figure (*ex* Fig. S11), however, adds little further information and was accordingly removed from the supplementary material.

l.234. the packages are independent from Rstudio which is just an interface (that you do not need to mention).

Thank you for catching this oversight; the revised manuscript now cites the R programming language and the packages used (L309).

l.238. which approach did you use for data transformation? Box-Cox perhaps?

The transformations applied (square-root and base-10 logarithm) are now specified in the text (L313).

Results

Overall this section is good but several figures need to be reworked to be easier to read. Also, I would like to thank you for having added all your data online as separate csv files. Thank you very much for your transparency.

We thank the reviewer for this positive general assessment, as well as for acknowledging the effort put into making our data openly available. We agree that several figures would benefit from improvements in readability and have reworked them accordingly in the revised manuscript (see below for details).

l.258. These sentences remain qualitative. Adding either % of change or absolute differences for each change you mention would make these sentences more informative.

We agree that the section would benefit from quantitative data on the observed changes in soil chemical fertility; these have been added during revision.

L332: "At the end of the experiment, basalt significantly increased soil pH (+0.71 ± 0.14), CEC (+0.43 ± 0.18 meq 100 g^{-1}), and base saturation (+13.6 ± 3.5%; Fig. 2). The influence of basalt on soil pH, CEC, and base saturation was not affected by AMF. Similarly, pore water pH (+0.41 ± 0.12) and alkalinity (+0.26 ± 0.08 meq l^{-1}) also increased with basalt, again without influence of AMF (Fig. 3). All reported values represent mean basalt effects ± SE. Concerning DIC, our data showed significant two- and three-way interactions between basalt and time and between basalt, AMF, and time respectively. Basalt increased pore water DIC concentrations throughout the experiment, with DIC rising on average 1.60 mg l^{-1} more than in the control between days 10 and 102. This effect became more pronounced in the presence of AMF: DIC concentrations in the B+AMF treatment increased by 8.42 mg l^{-1} more than in the control over the same period (day 10 – 102; Fig. 3)."

l.269-270 often either use limit of quantification or LOQ. Given that you do not use the term limit of quantification that often, I would suggest sticking to words and limiting acronyms.

Thank you for this helpful point; in the revised manuscript we removed the abbreviation and used the full term instead.

l.271. please correct "only in the first half of the growing season". Ni remained significantly higher after 75 days out of 100 days for B-AMF

The text has been revised to reflect this nuance, which the reviewer rightly highlighted.

L347: "[…] Nonetheless, basalt increased Ni concentration until around day 75 (Fig. 4)."

Fig.3 and Fig.4: This figure is very hard to read. Two ideas: 1) add some transparency to

the error bar so that we can better see the general trend with the dots and 2) connect the dots and (more controversial and debatable scientifically) and make the dots smaller. If you were to do option 2, you could also decrease the dots' size to improve "digestability".

We thank the reviewer for this valuable and constructive feedback. To also address Reviewer #2's feedback, Figure 4 was removed from the main text of the manuscript and moved into the supplementary materials. To improve readability of Figure 3 and (*ex*) Figure 4, the dots and error bars have been dodged and slightly reduced in size to better visualize trends in overlapping datapoints and corresponding standard errors.

L.278. same remark on qualitative vs quantitative info.

Quantitative data was added to this results section as well.

L354: "Basalt application increased exchangeable Ca and Mg in the 0-20 (Ca = +87.5 ± 37.7; Mg = +11.5 ± 5.9 mg kg soil^{-1}) and 20-40 (Ca = +72.6 ± 11.3; Mg = +36.3 ± 2.0 mg kg soil^{-1}) cm layers but not in the 40-60 cm layer (Fig. 5; Table S8-9). Additionally, basalt increased exchangeable Na in the 20-40 cm layer (+20.3 ± 2.0 mg kg soil^{-1}), and in the 0-20 cm layer a post-hoc pairwise comparison revealed a significant Na increase between C-AMF and B+AMF treatments (+21.9 mg kg soil^{-1}). On the other hand, exchangeable K, Fe and Si concentrations were not affected by basalt application (Fig. 5; Table S8). Concentrations of exchangeable Ni and Cr were below the limit of quantification in all layers, and the same was true for Al in the 40-60 cm layer (Table S14), hence precluding formal statistical analysis of these elements in the exchangeable pool of those layers. This was not the case for exchangeable Al in the 0-20 and 20-40 cm layers, which was not affected by basalt application, and exchangeable Zn, which decreased in the 0-20 and 20-40 cm layers (-0.4 ± 0.1 and -0.6 ± 0.1 mg kg soil^{-1} respectively; Fig. 6; Table S8-9). AMF did not affect cation concentrations in the sequentially extracted soil pools."

Fig. 6 : Mean soil nutrient **contents**
Fig. 6 and Fig 7. In the graphs and legends, change the term concentration that is used for solutions (mg/L) to content that is used for solids (mg/kg).

We thank the reviewer for catching this oversight, noted here and in the previous comment; the figures were corrected using the appropriate terminology (now Fig. 5-6).

l.295. I suspect that the amorphous phase is as much (if not much much more) responsible for the release of base cations. Please include it in your sentence.

An updated XRD measurement (performed by Qminerals, the earlier one was provided by RPBL) revised the amount of amorphous phases in the applied feedstock's mineralogy from 42.1% to 26.6%, thus decreasing their contribution to

weathering products. We agree, however, that given the high weathering rates typical of amorphous phases, they may be just as (or more) responsible for base cation release as the crystalline phases. The relevant sentence has been adapted accordingly.

L371: "[…] This corresponds to the weathering of 37.5% ± 3.7 and 33.2% ± 4.0 of Ca-, Mg-, Na- and K-bearing minerals in the applied basalt (including both crystalline and amorphous phases), in the presence and absence of AMF respectively, over the 113-day experimental period."

Fig.9. This figure is too packed. Please split it in three rows. By doing this, you may not need the dashed axes. Also axis labels are not always in the same direction. Avoid having significance letters over data (Ni Corn).

We thank the reviewer for this detailed and constructive feedback; the readability of *ex* Fig. 9 (now Fig. 8) was improved as suggested.

Discussion

Despite the high number of results that needed to be discussed, this section reads very well and is easy to follow. In a few cases, the explanations would benefit from being more mechanistic. See my comments and questions below.

We thank the reviewer for this encouraging general comment on the discussion section. We agree that some explanations would benefit from a more mechanistic framing, and have addressed this in our responses to the specific comments below.

l.323-330. how do these weathering rates compared with lab/theoretical weathering rates?

Weathering rates obtained in laboratory or flask experiments have been shown to be one to five orders of magnitude higher than those measured at field scale (White and Brantley, 2003), with mesoscale experiments yielding intermediate values (Malmström et al., 2000). At the same time, as the reviewer correctly noted in an earlier comment on heavy metal content, feedstock mineralogy can vary substantially across basalts, leading to differences in weathering rates of up to a factor of six (Lewis et al., 2021).

For this reason, we chose to compare our mesocosm weathering rates with those reported in similar experiments using the same basalt and a comparable method of weathering rates estimation, rather than with laboratory or theoretical weathering rates. Nevertheless, we recognize the value of situating our results relative to laboratory-derived weathering rates and, more broadly, of reflecting on how weathering rates vary across spatial scales. A comparison with lab-based basalt weathering rates, how dissolution rates change with scale, and a discussion of how differences in mineralogy among basalts may drive differences in weathering rates has been added to the manuscript.

L432: "Despite these comparatively high dissolution rates, weathering rates estimated from cation mass balance remained approximately three orders of magnitude lower than those predicted from laboratory-derived dissolution kinetics (−7.0 ± 0.08 log_{10} mol charge equivalents m^{-2} s^{-1} for the amended feedstock; S21). Such discrepancy is consistent with previous observations that laboratory-derived weathering rates commonly exceed field-derived rates by one to five orders of magnitude (White and Brantley, 2003), while mesocosm experiments typically yield intermediate values (Malmström et al., 2000). Such differences highlight the strong influence of experimental scale on estimated weathering rates. However, variability in weathering rates cannot be attributed to experimental scale alone, as mineral dissolution rates are also strongly affected by feedstock properties and environmental conditions (Palandri and Kharaka, 2004). Even among basalt feedstocks, dissolution rates may vary by up to a factor six due to differences in mineralogy, glass content, particle size distribution, and elemental composition (Lewis et al., 2021). Likewise, environmental conditions such as soil type, temperature and water availability can substantially influence weathering dynamics (Dupla, 2025). Consequently, weathering rates reported across studies should be compared with caution, particularly when experiments differ simultaneously in scale, feedstock composition and environmental conditions."

l.331. specify the magnitude of this increase

The magnitude of the increase in leachate DOC with pasteurization (which increased by roughly 158%) has been added to the manuscript (L429).

l.331-332. There are two issues here: first the study you mention does not say that DOC increases weathering rates. Second the mechanistic link between more DOC and higher weathering rates is missing. How can higher DOC trigger higher weathering rates? An instinctive answer could be by boosting microbial and therefore organic acid production but this does not work because you sterilized the soil and the only present microorganism, AMF, does not have a significant effect on weathering rates. So if the explanation is not biotic, we have to look into geochemical explanations: could the added DOC have increased the load of cations bound to OM and therefore kept the solution further from equilibrium ? (which would keep weathering rates high). Other explanations are plausible but as such this paragraph is not convincing.

We thank the reviewer for highlighting the lack of clarity in this section. The cited study used chemical data and a reactive transport model to examine the role of low-molecular-weight organic acids (a component of DOC) in basalt weathering. Their findings suggest that organic acids can sustain far-from-equilibrium conditions that drive rapid elemental loss, and potentially weathering rates as well.

The mechanistic link between DOC and weathering rates lies in DOC's composition: low-molecular weight organic acids (e.g., acetic acid, oxalic acid, oxaloacetic acid, salicylic acid) are an important component of DOC (Blume et al., 2010), and these

organic acids can drive silicate weathering. At the same time, DOC is both a substrate (e.g., polysaccharids) and a product (e.g., enzymes) of microbial activity (Blume et al., 2010), and it can also increase upon pasteurization as microbial necromass leaches and some soil organic matter becomes thermally destabilized. As such, elevated DOC may reflect increased SOM mineralization (and therefore greater microbial DOC production), while also directly contributing to weathering.

Regarding the potential abiotic explanation, the reviewer proposes that DOC acts as a sink for weathered cations, thereby sustaining far-from-equilibrium conditions. If this were the case, we would expect a corresponding increase in pore water cation concentrations alongside the observed increase in DOC. However, this pattern was observed only for Mg. Taken together with our sequential extraction data, this result suggests that weathered cations were rapidly incorporated into solid soil pools rather than remaining in the aqueous phase.

On the pasteurization process specifically: we want to clarify that the soil was pasteurized, rather than sterilized, meaning that most, but not all, microbes were killed. Some microbial activity therefore persisted in the mesocosms, albeit at a reduced level. The meaning of pasteurization, as well as the mechanistic links between DOC and mineral weathering, have been better highlighted in the revised manuscript.

L424: "The comparatively high weathering rates observed here may be related to the pasteurization process: heating the soil at 80 °C for four hours likely generated substantial microbial necromass and increased dissolved organic carbon (DOC) concentrations. In particular, low-molecular weight organic acids, which are an important component of DOC, have been shown to sustain far-from-equilibrium conditions that drive rapid elemental loss, and potentially weathering rates as well (Perez-Fodich and Derry, 2019). Indeed, pore water DOC concentrations in pasteurized mesocosms were on average 158% higher (+22.8 ± 3.5 mg l^{-1}) than those observed in comparable unpasteurized systems (Fig. S20), consistent with enhanced DOC availability contributing to the higher weathering rates observed here."

l.334-338. but you observed that the symbiosis happened right? I am not a microbiologist so sorry in advance if my question is stupid but were there any sign of a "poor" symbiosis or colonization?

The reviewer is correct that the symbiosis was successfully established. The average hyphal length in the AMF treatments was 2.69 ± 0.43 m g of $soil^{-1}$ (mean ± SE), which falls within the range of typically reported AMF hyphal lengths (0.5–5 m g $soil^{-1}$; L81). Similarly, root colonization by AMF averaged 17.4% ± 3.9 (mean ± SE), which is within the range reported for maize grown under varying phosphorus addition levels (3-45%; Duan et al., 2009; Galvez et al., 2001; Gosling et al., 2013;

Liu et al., 2000). In addition, AMF root colonization is known to vary across host plant developmental stages (Abbott and Robson, 1991; Gosling et al., 2013), and it is therefore possible that our sampling did not coincide with peak colonization. Taken together, the measured hyphal lengths and root colonization rates indicate that AMF successfully colonized the host plants and do not provide evidence of poor establishment of the symbiosis.

l.335. you have the data to assess the validity of this hypothesis. How do the bioavailable nutrient contents of your soil compare with contents in non-pasteurized soils?

We thank the reviewer for this comment, which adds robustness to our discussion. We examined pore water NPK, Ca, Mg, Si, and pH in pasteurized versus non-pasteurized soils (from a parallel experiment using the same, albeit non-pasteurized soil) and found no systematic increase associated with pasteurization. The manuscript has been revised to indicate that fertilizer application alone, rather than fertilization combined with pasteurization, is at least partly responsible for the observed lack of an AMF effect on weathering rates.

L401: "In contrast with our hypothesis, AMF did not significantly increase basalt weathering rates. This limited response likely reflects the relatively nutrient-rich conditions of the experiment. The soil used was relatively fertile and only moderately acidic (initial pH = 5.6), and all mesocosms received NPK fertilizer (96-10-39.5 kg ha^{-1}). Although P and K application rates were slightly below Flemish recommendations (Departement Landbouw & Visserij, 2016; Vlaamse Land Maatschappij, 2026), no visible symptoms of nutrient deficiency were observed in the plants. Hence, P availability was likely sufficient, reducing the incentive for AMF-mediated P acquisition. Under such conditions, the potential for AMF to stimulate mineral dissolution through biological weathering could be reduced. These findings suggest that AMF-induced enhancement of silicate weathering may be limited in fertilized soils where plant P demand is largely satisfied."

l.336. The NPK doses that you applied were low when compared with agronomic standards for corn ("normal" N doses for corn are 50 to 100% higher, P up to 6 times higher, and K up to 4 times higher). If even at very low NPK doses, AMF did not improve weathering rates, then there is little hope that AMF will have an impact in most conventional systems. This needs to be discussed more openly.

While we agree that the applied NPK fertilizer rates were below typical agronomic standards, we believe that nutrient limitation was unlikely in our experiment. The applied N rate is within the range of commonly used application amounts in Belgium and was recommended by the Flanders Research Institute for Agriculture, Fisheries and Food (ILVO). While P and K doses were indeed slightly lower than Flemish standards, the soil was fertile to begin with, which likely compensated for the reduced

external inputs. This is further supported by the absence of any visible signs of nutrient deficiency in the maize plants throughout the duration of the experiment. We therefore maintain that the experimental conditions did not reflect a nutrient-limited system, as also now stated in the discussion. While we acknowledge this is a relevant point, we do not believe our results support any conclusions about AMF responses under nutrient-limited conditions. A reflection on the nutrient limitation of the system has been added to the revised manuscript.

L560: "This outcome may be partly explained by the adequate P availability in our experiment. Arbuscular mycorrhizal symbiosis is established by the allocation of photosynthetic C from the plant to the fungus, and an important driver of this symbiosis is low bioavailability of soil P (Leake and Read, 2017). In the present study, P availability was likely enhanced by fertilizer addition, reducing the plant's dependence on AMF-mediated nutrient acquisition. Consequently, plants may have allocated less C to the symbiosis, limiting fungal development and potentially explaining why basalt amendment did not increase AMF colonization or hyphal length."

L596: "In our experiment, plant N:P ratios (around 7.7; Fig. S12) suggest that crop growth remained primarily constrained by N rather than P availability. Because basalt amendments supply base cations and, to a lesser extent, P, but do not contribute to biologically available N, their potential to alleviate the dominant nutrient limitation in our system was inherently limited."

L.351. specify : as the pore water pH **of the basalt-treated pots** was > 6.5

The text has been revised to specify that we refer to basalt-amended mesocosms (L456).

l.353-354. this happens even below pH 6.5. Maybe more correctly, mention the fact that at this pH, dissolved CO2 **predominantly** speciated into bicarbonate.

We agree with the reviewer on this point; the manuscript has been revised accordingly.

L456: "As the pore water pH of the basalt-treated mesocosms in our experiment was > 6.5 in the second half of the experiment (Fig. 3), the dissolved H_2CO_3 likely predominantly speciated into (bi)carbonate ions within the soil solution, leading to elevated DIC."

l.360. how much lower?

Basalt treatment with AMF showed, on average lower (-11.9 mg l^{-1}) pore water DOC than basalt treatment without AMF in the middle of the growing season. This figure has been added in the revised manuscript (L465).

l.363-367. to speak of stocks, you must cross content with biomass. If you did not measure aerial biomass, then it is a argument that is difficult to make.

The reviewer is correct: we measured both above- and below-ground biomass (L274), which allowed us to estimate Ca and Mg stocks from their content in each plant organ.

l.395. which is consistent with adsorption lyotropic series.

The reviewer is correct, and the revised manuscript has been amended to reflect this point (L499).

l.425-435. please add some quantitative information on the range of this increase so that the benefits can be better evaluated.

Increases in cations in the reducible and oxidizable pools have been added to the text.

L527: "Most of the agriculturally relevant (micro)nutrients present in the basalt, namely Ca, Mg and Fe, increased in the reducible pool in the 20-40 cm layer. The net basalt effect amounted to +116.0 ± 9.3 mg kg^{-1} for Ca, +1200 ± 71.6 mg kg^{-1} for Mg, and +668.3 ± 109.2 mg kg^{-1} for Fe. Likewise, basalt increase oxidizable Ca and Si by 4.5 ± 0.7 mg kg^{-1} and 27.6 ± 9.5 mg kg^{-1}, respectively (Fig. S6; Table S8)."

l.446-451. Another reason may be the sterilization of the soil. Many microbial processes are pH-dependent. You saw a pH increase but could not see the microbe-induced benefits associated with it.

We thank the reviewer for this valuable contribution. Although we did not sterilize the soil but rather pasteurized it, a substantial portion of the soil microbial community was eliminated by the process. We attempted to mitigate this by adding a microbial wash (meaning, a mycorrhizal inoculum stripped of fungal spores, though potentially retaining smaller microorganisms); however, this is an imperfect method, and the soil would also have been recolonized over time. Nevertheless, it is possible that the microbially mediated improvements in crop yield and quality often associated with increased pH were not observed in our experiment because of the lower starting soil microbial diversity. This additional potential explanation for the observed lack of agronomic co-benefits has been added to the manuscript.

L577: "Finally, soil pasteurization likely removed a substantial fraction of the indigenous microbial community, potentially limiting the extent to which basalt-induced improvements in soil chemistry could translate into plant responses. Many beneficial effects of increased soil pH on plant growth and nutrition are mediated by soil microorganisms through processes such as organic matter decomposition and consequent nutrient mineralization, solubilization, and uptake (Richardson et al., 2009; Van Der Heijden et al., 2008). Moreover, soil pH is a major determinant of

microbial community composition and activity (Fierer and Jackson, 2006; Lauber et al., 2009), meaning that microbial responses to pH change represent an important pathway through which basalt can influence nutrient cycling and crop performance. By substantially reducing microbial abundance and diversity, pasteurization may therefore have disrupted these microbial pathways, weakening the link between basalt-induced increases in pH and improvements in nutrient availability. Consequently, the reduced microbial community may have contributed to the limited crop responses observed despite clear changes in soil chemical properties."

l.473. this is a very interesting results that shows that even "natural" soils can lead to "illegal" heavy metals concentrations. Do you know what was the initial total content in Ni in your soil? An already elevated content could be an explanation here.

We agree that already-elevated background Ni in soil can lead to higher Ni content following feedstock amendment, and that including this data would strengthen the manuscript. However, we unfortunately do not possess information on the soil's Ni content at the start of the experiment.

l.472-475. please give the references of the 4 regulations that you cite.

The text has been amended to include references for the cited regulations and corrected thresholds.

L607: "During the 113-day experimental period, average pore water Ni concentrations exceeded the freshwater Environmental Quality Standards (EQS) of both the EU (4 µg l^{-1}) and Australia (0.3–10 µg l^{-1}, depending on water pH and hardness) in both the control and basalt treatments (7.1 ± 1.7 and 19.7 ± 4.9 µg l^{-1}, respectively) (Anzg, 2024; European Commission, 2006). This may be partly due to background Ni already present in the soil. Nevertheless, the observed concentrations remained below the regulatory limits set by other jurisdictions, such as the United States (52 and 470 µg l^{-1} for chronic and acute exposure, respectively) and Canada (3–150 µg l^{-1}, depending on water hardness) (B.C. Ministry of Water, 2024; Canadian Council of Ministers of the Environment, 1987; Epa, 1995)."

l.488-490. Another argument where having initial elemental soil contents would be helpful.

Again, we fully agree that reporting the initial soil Cr content would strengthen the manuscript, but unfortunately we do not have that information.

Reviewer number 2's comments

We sincerely thank the reviewer for investing their time and care to provide this detailed and constructive review. Their comments helped us identify several points requiring clarification and contributed to a more balanced and rigorous framing of the manuscript.

Throughout the document, in black are the reviewer's comments (separated into different paragraphs to address each comment separately), in blue are the authors' responses, and in *italics* are the excerpts from the revised manuscript. Line numbers refer to the numbers in the clean, revised manuscript.

The ms describes a factorial experiment where the impact of basalt and arbuscular mycorrhizal fungi on soil chemical properties and plant performance was assessed. The results show that basalt had very little influence (apart from increasing soil pH, CEC and base saturation as main effects) and even less influence on plant performance (biomass, nutrient mass fractions), whereas AMF did even have less impact on soil chemistry and plant performance. Admittedly, the ms reports somewhat more significant effects, but with the large number of tests applied setting a significance level at $p < 0.05$ (l. 235) may well be too liberal. It may then be unwarranted to claim synergistic effects (l. 23, 492) – apart from the fact that synergy has not been operationalised here (the authors should look at Larimer et al., Symbiosis 51: 139-148. 2010, where an explicit test for synergy has been proposed), the only parameter with a significant interaction term is DIC; and here the authors explain the problems with the use of DIC as a proxy in section 4.1.1. My reading of that section is that the DIC assessment is complicated and cannot be used as sole proxy for weathering rates.

We thank the reviewer for their valuable opinion. While we acknowledge that the large number of statistical tests applied could justify a more conservative significance threshold (e.g., $p < 0.01$), we maintain that this would not substantially alter the manuscript's core message: despite improvements in soil chemical fertility following feedstock application, these improvements did not translate into appreciable gains in plant yield or quality.

The reviewer's interpretation of Section 4.1.1 is correct: pore water DIC is indeed the only (key) parameter showing a significant basalt x AMF interaction. In light of the reviewer's comment on synergistic effects and their operationalization, the manuscript was revised to remove references to synergistic effects from data interpretation (L28, L446).

The paper contains many figures. In my view some of these are superfluous (e.g. Fig. 8 can be omitted if the t-value, degrees of freedom and corresponding p-value are moved to the text). Other figures are difficult to interpret. Fig. 4 (Fe in pore water) shows 4, 3, 2 and 1 data point for the first four assessments over time. I suppose this figure implies overlapping circles, but one cannot see the data points that overlap, unless one studies the supplementary material. (Personally, I am not convinced of the

importance of such figures that show changes of metals in pore water over time; one would predict weathering going on over the course of the experiment, and concentrations in pore water are a functioning of continuous weathering, possible uptake (hardly different for most nutrients between the different treatments) and likely soil moisture (if no neutral tracer is used to correct for that). For most nutrient there is a peak in the middle period and a steep decline towards the end – but the discussion does not engage with potential explanations for this curvilinear relationship. However, the experiment was apparently executed under conditions of no rainfall excess, as there was very little leaching reported (l. 178).

We thank the reviewer for their close attention to detail and constructive suggestions for improving the manuscript's data visualization and flow. Throughout the manuscript figures, data points and their error bars have been slightly dodged to improve clarity where data points overlap.

We agree with the reviewer's suggestion regarding Figure 4, which has therefore been moved to the supplementary materials, thereby limiting the need to address potential explanations for the observed trends in the discussion.

On the other hand, *ex* Figure 8 (now Fig. 7; weathering rates with/without AMF) directly addresses one of the manuscript's central hypotheses, namely whether AMF enhance feedstock weathering rates, and we therefore consider it a key component of the manuscript. For this reason, we respectfully disagree with the reviewer's suggestion to remove it, and would rather keep it in the main text.

It is also somewhat inconsistent to provide six data points over the course of the experiment and then using the discussion to suggest that the experiment may have lasted too short for an assessment of basalt application.

We acknowledge the inconsistency between the duration of our experiment (113 days) and the considerably longer timeframe that would be needed to fully assess the effects of basalt amendment, given that only ~35% of the applied basalt weathered by the end of the experiment. However, extending the experimental duration was not feasible in this setup, as the pasteurized, AMF-free mesocosms would eventually have become colonized by AMF spores, compromising the AMF-free treatments. The manuscript has been revised to better reflect this balance.

L568: "In addition, the short experimental duration of our trial (113 days) might have constrained the potential for AMF to influence crop yield and quality. Indeed, a meta-analysis by Qin et al. (2022) identified experimental duration as the most important factor limiting the influence of AMF on crop growth, followed by soil texture, with a smaller AMF effect on sandy soils (>50% sand). Given both the short experimental duration of the present experiment and texture of the used soil (sandy loam; 69.5% sand, 28.1% silt, 1.8% clay), it is likely that both factors played a role in the observed lack of AMF effect on plant biomass in our study. Although a longer experimental duration might have allowed both basalt- and AMF-mediated responses to develop

more fully, extending the study was not feasible because the pasteurized, AMF-free mesocosms would eventually have been recolonized by AMF spores, thereby compromising the integrity of the AMF-free treatment."

While the basalt weathered rapidly, one may wonder what would happen under conditions of higher rainfall – with very little plant acquisition of the released cations, one may envisage leaching under field conditions outside the growing season.

We agree with the reviewer that it would have been valuable to consider conditions outside the growing season; however, as mentioned previously, this was not feasible in our experimental setup, since the AMF-free mesocosms would eventually have been colonized by AMF. We also agree that higher rainfall could influence the balance between weathering, plant uptake and leaching. However, because these processes were not investigated experimentally and their net effect depends on several interacting factors (e.g., rainfall, temperature, plant activity, secondary mineral formation), we feel that a discussion of such scenarios would be overly speculative for inclusion in the manuscript. Nevertheless, we have elaborated on these considerations below in response to the reviewer's comment.

During the experiment, leachate losses were limited due to low rainfall combined with active plant growth and hence high transpiration rates. Consequently, cation export through leaching was low and cations were largely retained in the soil or plants through adsorption, secondary mineral formation and plant uptake. Outside the growing season, rainfall is generally higher at the experimental location, and soil water losses are driven primarily by evaporation rather than evapotranspiration, thereby potentially resulting in leachate formation. One might expect this greater water availability to drive higher weathering rates, and therefore potential flushing of reaction products in the leachates. On the other hand, such conditions would likely coincide with lower temperatures outside the summer growing season. Temperature strongly constrains silicate weathering rates (Dupla, 2025): for example, Pogge Von Strandmann et al. (2022) reported olivine weathering rates at 4 °C that were approximately two orders of magnitude lower than those measured at 19 °C. The comparatively high weathering rates observed here therefore likely reflect the combined effects of elevated DOC concentrations due to pasteurization and favorable summer temperatures.

An issue of potential concern (especially with long-term application) is the fact that basalt releases more Na than K, that Na availability increased and K availability decreased in the soil, and that leaf K mass fractions declined after basalt application. This may lead in the end to an unfavourable K:Na balance that could constrain plant performance. Unfortunately, we cannot assess this potential problem as there are no data on leaf Na (only on corn Na). But a somewhat less optimistic perspective on Na could be useful.

We agree with the reviewer that the observed increase in pore water Na raises questions for future plant performance and K:Na ratio, and we thank them for their

valuable insight. On the other hand, within the scope of this experiment, we did not observe consistent adverse effects of basalt on plant growth, biomass or quality, aside from a few exceptions. Still, as we did not analyse plant Na content, our picture of this dynamic remains partial.

In general, we recognize Na-K antagonism as a potential concern over longer timescales, particularly with repeated basalt application. This nuance has been addressed in the revised manuscript.

L553: "Furthermore, Na availability increased in both the soil exchangeable complex and the pore water, while leaf K concentration declined following basalt amendment. Although these changes were not associated with consistent negative effects on plant growth or biomass, they raise questions regarding the long-term consequences for plant nutrition, particularly shifts in the K:Na ratio that could ultimately constrain plant performance (Maathuis and Amtmann, 1999). Even though leaf Na concentrations were not measured in this experiment, we recognize Na-K antagonism as a potential concern over longer timescales, especially under repeated basalt applications."

My main problem with the ms is its framing. (A title like "Limited effect of basalt amendment and mycorrhizal inoculation on soil chemical properties and maize growth on a nutrient-rich soil" does more justice to the results). I think this issue can be easily remedied by looking more at what the data actually show (very little effect of basalt and AMF) and less to general statements that have been made.

We thank the reviewer for this constructive assessment, which helped sharpen the manuscript's focus. Upon revision, the manuscript was edited to more accurately reflect the modest magnitude of the observed effects and to better emphasize the nutrient-rich context of the soil used (L32, 108, 401-408, 523, 550). We adopted the reviewer's suggestion and revised the title accordingly to reflect these points.

The concept of context-dependency is mentioned thrice (l. 28, 465, 504), but without specification of the context, the concept remains vague. In this experiment that soil was heavily fertilised and P was adequately available, to judge from leaf N:P (around 8, indicating that despite the high amounts of N added, the system was still N limited; high P availability is also consistent with lack of response to AMF).

We sincerely thank the reviewer for the time taken to thoroughly review both the main text and supplementary materials. We agree with the reviewer that P was adequately available and the system was rather N- than P-limited, albeit without visible signs of nutrient deficiency as mentioned above. The reviewer's comment on context dependency will be addressed in the following comment.

However, the ms frames the study in use of basalt under very different conditions (nutrient-depleted soils, high acidity) and thereby provides a too optimistic perspective of the use of basalt as soil enhancer in agro-ecosystems of northwestern Europe.

We thank the reviewer for this valuable perspective, and we agree that our original framing insufficiently emphasized the specific experimental context of this study (a fertilized temperate sandy loam soil with relatively high nutrient availability and only moderate acidity). We have therefore revised the text to better clarify this context and to avoid overgeneralization of basalt-associated agronomic benefits such as mineral-associated organic matter formation and nutrient replenishment (L15, L401-408, L516-519, L523, L536-540, L550-558, L595-603, L636).

While the agronomic co-benefits of enhanced weathering are frequently emphasized in the literature, studies conducted on fertile temperate soils remain comparatively rare. Specifically, crop responses to enhanced weathering originate from nutrient-poor and/or acidic soils, whereas the present study was conducted under comparatively fertile conditions with fertilizer addition and relatively high P availability. We clarified that the limited effects of basalt and AMF observed here likely reflect these experimental conditions and therefore highlight the context dependency of enhanced weathering outcomes across agroecosystems. We also stress that reporting such constrained responses is important for defining the environmental conditions under which enhanced weathering is most likely to deliver agronomic co-benefits (Dieleman and Janssens, 2011).

Note that in the Tables in Supplementary material N is given in %, not in mg/g (otherwise N:P rations would make no sense at all) as the Info to that table suggests; and leaf K contains an evident outlier for pot 18, which could have affected the analysis on plant K.

Regarding the units in the supplementary table referenced for N content: the reviewer may be referring to Table S19, which reports p- and F-values from the ANOVA tests rather than N content, as the supplementary material does not contain a table with N content/concentration, but rather a figure displaying N content (Figure S12). Figure S12 presents N and P content data (both in mg g^{-1} dry plant matter), with N content around ~10 mg g^{-1} plant (~1%), and approximately 1 mg g^{-1} plant for P content. The resulting N:P ratio is therefore 7.7 ± 0.7 (mean ± se), indicating N limitation rather than P limitation, in agreement with the reviewer. The leaf K outlier was removed from the dataset prior to statistical analysis, in order to avoid skewed results. The revised manuscript incorporates these insights.

L595: "The contrasting responses reported across soils and regions emphasize the context dependency of EW outcomes. In our experiment, plant N:P ratios (around 7.7; Fig. S12) suggest that crop growth remained primarily constrained by N rather than P availability. Because basalt amendments supply base cations and, to a lesser extent, P, but do not contribute to biologically available N, their potential to alleviate the dominant nutrient limitation in our system was inherently limited. In contrast, EW may deliver greater agronomic benefits in highly weathered tropical soils, where soil acidity, low base saturation, and depletion of rock-derived nutrients are more common and

where basalt-derived nutrient inputs can more directly alleviate nutrient constraints (Haque et al., 2025; Swoboda et al., 2022).”

The authors also make sweeping statements that go well beyond basalt weathering, e.g. the link to MaOM (l. 44), a mechanism that works for poorly crystalline material but not for basalt. Also the statements in l. 385 (importance of remedying low soil pH or nutrient depletion as a consequence of intensive agriculture) do not seem to be relevant in relation to the experimental conditions used here. Equally l. 434-436 seem overstated on the basis of the data presented here.

While this falls outside the manuscript’s primary focus, we would like to clarify that it is mostly the secondary, clay-sized minerals, as well as divalent metal cations formed and/or liberated upon weathering of primary minerals that provide substrate for SOM stabilization via organo-mineral interactions (Kleber et al., 2015; Slessarev et al., 2021; Wu et al., 2023). The text has been revised to reflect this nuance and avoid the overstatements mentioned by the reviewer.

L46: “Moreover, weathering of silicate minerals releases divalent metal cations and promotes the formation of secondary clay-sized minerals, both of which can facilitate the stabilization of soil organic matter through organo-mineral interactions (Kleber et al., 2015; Slessarev et al., 2021; Wu et al., 2023), potentially improving soil fertility (Oldfield et al., 2019; Woomer et al., 1994).”

L536: “Although the total stock of cations in the oxidizable fraction was relatively small compared to other soil pools (Fig. S6), the observed increases in Ca and Si following basalt application are consistent with pathways that may contribute to organic matter stabilization. More specifically, such processes have been linked to improved soil structure (Buss et al., 2023; Steinwidder et al., 2025) and mineral-associated organic matter (MAOM) formation (Niron et al., 2024) in EW experiments, although these properties were not directly assessed in the present study.”

As a mycorrhizal researcher I noted the incorrect claim that hyphal length of AMF can be up to 100 m /cm3. This is a value for ectomycorrhizal fungi, but hyphal length density for AMF is normally an order of magnitude less – as the data by the authors confirm much lower hyphal densities as well.

We thank the reviewer for their insight. While typical hyphal length values are indeed closer to 0.5-5 m g soil^{-1} (e.g., Sochorová et al., 2016), the upper end of the reported distribution can reach 70 m g soil^{-1} (Miller et al., 1995). The revised manuscript reflects both figures accordingly, with emphasis on typical values.

L80: “AMF extend beyond the root surface through extensive extraradical hyphal networks, with hyphal lengths commonly on the order of 0.5 to 5 m g soil^{-1} (Johnson et al., 2003; Sochorová et al., 2016), although substantially higher values (around 70 m of hyphae per g of soil^{-1}) have occasionally been reported (Miller et al., 1995).”

In l. 138 PO_4^- should be PO_4^{3-}.

Thank you for spotting this error; it has been corrected.

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