We thank Xavier Dupla for the time and care he devoted to reviewing our manuscript; his feedback will greatly strengthen 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 will benefit substantially from these revisions.

Throughout the document, in black are the reviewer's comments (separated into different paragraphs to address each comment separately), and in blue are the authors' responses.

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 will be added to the revised manuscript.

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 will be added to the revised abstract.

l.25. precise soil "chemical" fertility.

Throughout the abstract and manuscript, we will specify that we assessed 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 will greatly improve the abstract.

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, once implemented, will help provide a more balanced overview of both risks and potential co-benefits of enhanced weathering (EW). We will revise the text to reflect also on potential risks of EW (e.g., loss of soil organic matter due to pH increase, reduction of water infiltration rates, heavy metal accumulation).

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 will be removed from the list of essential nutrients supplied by basalt, and the release of the beneficial element silicon will be included instead.

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 will be revised to clarify that basalts can vary widely in mineralogy, and therefore in heavy metal accumulation risk, ranging from safe to concerning.

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 will revise the manuscript accordingly.

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 & Read, 2010), and are therefore largely irrelevant to annual crops. We thank the reviewer for raising this point, and will add a few sentences to the manuscript clarifying the negligible role of ectomycorrhizal fungi in annual crop symbioses.

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

The full name will be removed.

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

The original reference appeared in the following sentence; the source will be placed next to the data.

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 will be clarified to specify that the authors mean hyphal turnover.

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

We thank the reviewer for this insightful question. The text will be clarified to indicate that acidification of the soil surroundings is mostly due to ammonium uptake (Neumann & 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 partly correct: AMF excrete glomalin, which can chelate heavy metals and thereby reduce their mobility. At the same time, the AMF-driven soil acidification might ultimately increase heavy metal bioavailability. We thank the reviewer for this insight, which will help sharpen the mechanistic explanation, and we will revise the text to be more specific on this point.

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 will rephrase it to clarify that this research gap was simply highlighted by (Verbruggen et al., 2021).

l.79. Precise soil "chemical" fertility.

This will be 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. 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 will be revised to reflect this added nuance.

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 data on the elemental profile, which will be added in a table to the supplementary materials (and is also presented below, Table A). Details on the XRF method have been requested to RPBL.

Table A: Soil elemental composition according to XRF.

Formula	Concentration (%)
SiO2	83.13
Al2O3	8.70
Fe2O3	2.24
K2O	1.95
Na2O	1.09
P2O5	0.72
TiO2	0.70
MgO	0.61
CaO	0.43
SO3	0.12

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 will be added to the revised manuscript.

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 will be revised to clarify that watering was partly "artificial", along with the mm applied.

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) will be added to the revised manuscript.

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 will be added to the revised manuscript.

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

The soil:water ratio used was 1:2.5 (see below for the detailed method); this information will be added to the method section.

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.

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°). An internal standard (10 wt% Al_2O_3) was added during sample preparation to determine amorphouse phase content, and samples were milled in ethanol for 10 minutes. XRF data were provided by RPBL, who have been contacted to retrieve this information.

Qminerals has been contacted to obtain further details on the determination method for amorphous phase content. These and the above-mentioned details will be added to the revised manuscript.

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

Unfortunately, we do not currently possess this information; we have contacted RPBL in order to obtain it.

Table 1.

What are these “vs” mentions?

We thank the reviewer for spotting this editing error. The mineralogy table will be revised to reflect XRD data as below. Since submitting the manuscript, a more accurate XRD analysis of feedstock mineralogy was performed (by Qminerals), and therefore Table 1 will be revised to contain the most precise quantification.

How was the amorphous phase measured?

As the XRD analysis was outsourced and as mentioned in a previous comment, we are in the process of acquiring this information, which will be added to the revised manuscript table.

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 will be revised to reflect this more realistic accuracy level.

Revised Table 1: Minerological composition of the applied feedstock measured with XRD, and elemental content measured by XRF.

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	$NaAlSi_2O_6 \bullet (H_2O)$	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 will be edited to clarify the rationale.

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 bottle and hand-shaken for one minute. Seeds were then added, and the bottle was hand-shaken again for one minute to ensure even coating, after which the seeds were sown. These details will be added to the revised manuscript.

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. The transformation applied will be detailed in the figure caption.

L.129. Precise soil "chemical" fertility.

This will be corrected throughout the manuscript.

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

This information will be added to the revised manuscript.

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 will be added to the manuscript.

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 (l94).

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 potentially 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 A; also below). 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 some weathering had already occurred by day 9, or that the sequential extraction method partially extracted un-weathered primary minerals.

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, AMF-driven silicate weathering was 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*, will be more strongly emphasized in both the method and discussion sections.

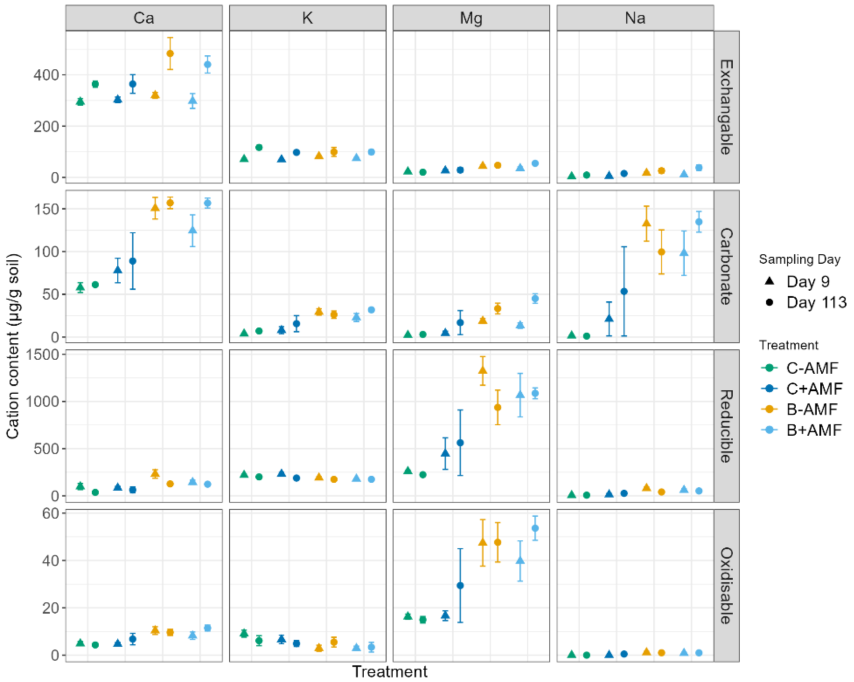

Figure A: average Ca, K, Mg and Na content in the four sequentially extracted soil pools at day 9 (triangles) and 113 (circles) for each treatment. Error bars represent the standard error on the mean. Several small error bars are not visible due to overlap with data points.

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

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

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 will help clarify the calculations in the manuscript. We will adopt the reviewer's first suggestion and specify that, to estimate cation uptake by plants, leaf cation concentrations in the control treatment were subtracted from those in the other treatments.

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 methods section, which currently follows soil > weathering > plants > AMF. We will instead resolve this by clarifying in the text that plant measurements appear later.

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 A (above, and in the revised supplementary material) shows that likely some weathering has already taken place, given the 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 will be expanded to reflect on this.

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 calculate weathering rates will be added to the manuscript.

l.215. specify the digestion method.

The text will be 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). Samples were diluted with HNO_3 (2% HNO_3:sample ratio of 19:1, TraceMetal Grade, Fisher Chemical). 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.

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 (Fig. S11), however, adds little further information and will accordingly be 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 will cite the R programming language and the packages used.

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

The transformations applied (square-root and base-10 logarithm) will be specified in the text.

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 could benefit from improvements in readability and will rework 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 this results section would benefit from quantitative data on the observed changes in soil chemical fertility; these will be added during revision.

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; the revised manuscript will remove the abbreviation and use 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 will be revised to reflect this nuance, which the reviewer rightly highlighted.

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 will be removed from the main text of the manuscript, and it will be moved into the supplementary materials. To improve readability of Figures 3 and (*ex*) Figure 4, the dots and error bars will be dodged and slightly reduced in size to better visualize trends in overlapping data, and the error bars will be made more transparent, as suggested.

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

Quantitative data on percent change will be added to this results section as well.

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 will be corrected using the appropriate terminology.

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, rather than an earlier one 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. 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 will be revised accordingly.

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 Figure 9 will be 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 & 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 basalt weathering rates obtained in laboratory experiments, how they change with scale, and a discussion of how differences in mineralogy among basalts may drive differences in weathering rates will be added to the manuscript.

l.331. specify the magnitude of this increase

The magnitude of the increase in leachate DOC with pasteurization (which increased by roughly 158 %) will be added to the main text.

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, will be better highlighted in the revised manuscript.

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; the symbiosis did occur. Given the adequate nutrient supply in our experiment, however, there may have been less physiological "need" for AMF symbiosis, which could account for the comparatively modest degree of colonization observed.

The average hyphal length in the AMF treatments was 2.69 ± 0.43 m per g of soil (mean ± se), toward the lower end of the 2-29 m g^{-1} soil range typically reported for pot experiments (Leake & Read, 2017), consistent with a reduced need for AMF-driven nutrient (particularly phosphorus) scavenging.

Our observed values of root colonization by AMF (17.4 % ±3.9; mean ± se) fall within the range reported for maize under varying phosphorus addition levels (3-45%; (Duan et al., 2009; Galvez et al., 2001; Gosling et al., 2013; Liu et al., 2000). We therefore do not consider colonization to have been poor; the shorter hyphal length is more likely attributable to the fertilized growing conditions. Finally, root colonization by AMF is known to vary over the host plant's developmental stages (Abbott & Robson, 1991; Gosling et al., 2013), so it is possible that our sampling did not capture the peak in root colonization.

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 will therefore be 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.

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 stated in several paragraphs of the discussion (Line 397-401, Line 510-515). 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 will be added to the revised manuscript.

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

The text will be revised to specify that we refer to basalt-amended mesocosms.

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 will be revised accordingly.

l.360. how much lower?

Basalt treatment with AMF showed, on average, 22.4% lower pore water DOC than basalt treatment without AMF in the middle of the growing season. This figure will be reported in the revised manuscript.

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 (lines 204-206), which allowed us to estimate Ca and Mg stocks from their concentrations in each plant organ.

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

The reviewer is correct, and the revised manuscript will reflect this point.

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

Percentage increases in cations in the reducible and oxidizable pools will be added to the text.

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 explanation for the observed lack of agronomic co-benefits will be added to the manuscript.

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 will be amended to include references for the cited regulations and corrected thresholds. 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 (11.67 ± 1.2 and 26.8 ± 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 agree that reporting the initial soil Cr content would strengthen the manuscript, but unfortunately we do not have that information.

References

Abbott, L., & Robson, A. (1991). Factors influencing the occurrence of vesicular-arbuscular mycorrhizas. *Agriculture, Ecosystems & Environment, 35*(2-3), 121-150.

Blume, H. P., Brümmer, G. W., Horn, R., Kandeler, E., Kögel-Knabner, I., Kretzschmar, R., Stahr, K., Wilke, B.-M., Thiele-Bruhn, S., & Welp, G. (2010). Scheffer/schachtschabel. *Lehrbuch der Bodenkunde, 16*(472), 10-1007.

Duan, T., Shen, Y., Facelli, E., Smith, S. E., & Nan, Z. (2009). New agricultural practices in the Loess Plateau of China do not reduce colonisation by arbuscular mycorrhizal or root invading fungi and do not carry a yield penalty. *Plant and Soil, 331*(1-2), 265-275. https://doi.org/10.1007/s11104-009-0251-3

Galvez, L., Douds Jr, D., Drinkwater, L., & Wagoner, P. (2001). Effect of tillage and farming system upon VAM fungus populations and mycorrhizas and nutrient uptake of maize. *Plant and Soil, 228*(2), 299-308.

Gonzalez-Chavez, M. C., Carrillo-Gonzalez, R., Wright, S. F., & Nichols, K. A. (2004, Aug). The role of glomalin, a protein produced by arbuscular mycorrhizal fungi, in sequestering potentially toxic elements. *Environ Pollut, 130*(3), 317-323. https://doi.org/10.1016/j.envpol.2004.01.004

Gosling, P., Mead, A., Proctor, M., Hammond, J. P., & Bending, G. D. (2013, Apr). Contrasting arbuscular mycorrhizal communities colonizing different host plants show a similar response to a soil phosphorus concentration gradient. *New Phytol, 198*(2), 546-556. https://doi.org/10.1111/nph.12169

Leake, J. R., & Read, D. J. (2017). Mycorrhizal Symbioses and Pedogenesis Throughout Earth's History. In *Mycorrhizal Mediation of Soil* (pp. 9-33). https://doi.org/10.1016/b978-0-12-804312-7.00002-4

Lewis, A. L., Sarkar, B., Wade, P., Kemp, S. J., Hodson, M. E., Taylor, L. L., Yeong, K. L., Davies, K., Nelson, P. N., Bird, M. I., Kantola, I. B., Masters, M. D., DeLucia, E., Leake, J. R., Banwart, S. A., & Beerling, D. J. (2021). Effects of mineralogy, chemistry and physical properties of basalts on carbon capture potential and plant-nutrient element release via enhanced weathering. *Applied Geochemistry, 132*. https://doi.org/10.1016/j.apgeochem.2021.105023

Liu, A., Hamel, C., Hamilton, R., Ma, B., & Smith, D. (2000). Acquisition of Cu, Zn, Mn and Fe by mycorrhizal maize (Zea mays L.) grown in soil at different P and micronutrient levels. *Mycorrhiza, 9*(6), 331-336.

Malmström, M. E., Destouni, G., Banwart, S. A., & Strömberg, B. H. (2000). Resolving the scale-dependence of mineral weathering rates. *Environmental science & technology, 34*(7), 1375-1378.

Neumann, E., & George, E. (2010). Nutrient Uptake: The Arbuscular Mycorrhiza Fungal Symbiosis as a Plant Nutrient Acquisition Strategy. In *Arbuscular Mycorrhizas: Physiology and Function* (pp. 137-167). https://doi.org/10.1007/978-90-481-9489-6_7

Riaz, M., Kamran, M., Fang, Y., Wang, Q., Cao, H., Yang, G., Deng, L., Wang, Y., Zhou, Y., Anastopoulos, I., & Wang, X. (2021, Jan 15). Arbuscular mycorrhizal fungi-induced mitigation of heavy metal phytotoxicity in metal contaminated soils: A critical review. *J Hazard Mater, 402*, 123919. https://doi.org/10.1016/j.jhazmat.2020.123919

Royal Meteorological Institute, B. (2022). *Klimatologische overzichten van 2022*. https://www.meteo.be/nl/klimaat/klimaat-van-belgie/klimatologisch-overzicht/2022/zomer

Smith, S. E., & Read, D. J. (2010). *Mycorrhizal symbiosis*. Academic press.

Taylor, L. L., Leake, J. R., Quirk, J., Hardy, K., Banwart, S. A., & Beerling, D. J. (2009, Mar). Biological weathering and the long-term carbon cycle: integrating mycorrhizal evolution and function into the current paradigm. *Geobiology, 7*(2), 171-191. https://doi.org/10.1111/j.1472-4669.2009.00194.x

Verbruggen, E., Struyf, E., & Vicca, S. (2021). Can arbuscular mycorrhizal fungi speed up carbon sequestration by enhanced weathering? *Plants, People, Planet, 3*(5), 445-453. https://doi.org/10.1002/ppp3.10179

White, A. F., & Brantley, S. L. (2003). The effect of time on the weathering of silicate minerals: why do weathering rates differ in the laboratory and field? *Chemical Geology, 202*(3-4), 479-506. https://doi.org/10.1016/j.chemgeo.2003.03.001