

We sincerely thank the reviewer for investing their time and care to provide this detailed and constructive review. Their comments have helped us identify several points requiring clarification and will contribute 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), and in blue are the authors' responses.

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 will be revised to remove references to synergistic effects (l. 23 and 492) from data interpretation.

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, data points and their error bars will be slightly dodged to improve clarity where data points overlap.

We agree with the reviewer that Figure 4 reflects temporal variability driven by weathering dynamics, plant uptake and soil moisture. As these factors fall outside the manuscript's main scope and hypotheses, they are not discussed in depth. Following the reviewer's suggestion, Figure 4 will therefore be moved to the supplementary materials, limiting the need to address potential explanations for the observed trends in the discussion.

On the other hand, Figure 8 (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 will be revised to better reflect this balance.

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.

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 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, the correspondingly lower temperatures during this period potentially curb weathering rates (Dupla, 2025). As an example, a recent EW experiment found olivine weathering rates at 4°C two orders of magnitude lower than at 19°C (Pogge von Strandmann et al., 2022). The manuscript will be revised to reflect on potential leaching and weathering dynamics with changing temperatures and rainfall inputs.

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 will be addressed in the revised manuscript.

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 will help sharpen the manuscript's focus. Upon revision, the manuscript will be edited to more accurately reflect the modest magnitude of the observed effects and to better emphasize the nutrient-rich context of the soil used. We will adopt the reviewer's suggestion and revise 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 a following comment.

Note that in the Tables in Supplementary material N is given in %, not in mg/g (otherwise N:P ratios 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 S17, 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 S9). Figure S9 presents N and P content data (both in mg g^{-1} dry plant matter), with N content around $\sim 10 \text{ mg g}^{-1}$ plant ($\sim 1\%$), and approximately 1 mg g^{-1} plant for P content. The resulting N:P ratio is therefore 7.7 ± 0.7 (mean \pm 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 will incorporate these insights.

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 will therefore revise 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.

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 will further clarify 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 will 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 & Janssens, 2011).

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 will be revised to reflect this nuance and avoid overstatements.

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 1-2 m g⁻¹ soil (e.g., Sochorová et al., 2016), the upper end of the reported distribution can reach 100 m cm⁻³ (Miller et al., 1995). The revised manuscript will reflect both figures accordingly, with added emphasis on typical values.

In l. 138 PO₄⁻ should be PO₄³⁻.

Thank you for spotting this error; it will be corrected.

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