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:

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

l. 15. Please add application rate

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

l.25. precise soil "chemical" fertility.

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

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

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.

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.

l.54-55 to be more objective I would add : "although local conditions, *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.

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

l. 61. Add reference.

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

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

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?

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.

l.79. Precise soil "chemical" fertility.

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.

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?

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

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

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

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

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

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.

l.96. please add exact mine location coordinates as RPBL has several mines.

Table 1.

What are these "vs" mentions?

How was the amorphous phase measured?

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

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

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

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.

L.129. Precise soil "chemical" fertility.

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

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

l.146. precise that you measured soil "chemical" fertility.

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

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).

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

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

$$[\text{additional cations from basalt}] = [\text{cation}]_{\text{basalt treatment (with or without AMF)}} - [\text{cation}]_{\text{control without AMF treatment}}$$

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.

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.

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

l.215. specify the digestion method.

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.

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

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

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.

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

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.

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

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".

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

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).

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.

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).

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.

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

l.331. specify the magnitude of this increase

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.

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?

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?

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.

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

l.353-354. this happens even below pH 6.5. Maybe more correctly, mention the fact that at this pH, dissolved CO_2 **predominantly** speciated into bicarbonate.

l.360. how much lower?

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.

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

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

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.

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.

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

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