Dear authors,

Thank you for providing point-by-point answers to the comments made by both reviewers. As you see, both reviewers see the merit of your work, but also flagged a potential issue with the duration of the experiment, which needs to be properly acknowledged in the discussion and conclusion of the manuscript to mitigate misinterpretation of the results of this study. Additionally, both reviewers provided constructive comments to increase the clarity of the manuscript. I therefor invite you to submit a revised version of your manuscript that adequately deals with the points that were raised by the reviewers.

I am looking forward to receiving the revised version of your manuscript.

All the best,

Marijn

Dear editor,

We thank you for the invitation to submit a revised version of the manuscript. We added a paragraph in the discussion about the duration of the experiment:

Line 513-519: "While at first sight reduced toxic trace elements in the plant may be beneficial for crop production, increased porewater concentrations, potential accumulation in the plant roots, and the immobilization of these elements in the soil may pose an environmental risk in the longer term. Our short-term experiment did not allow for complete weathering of the silicate materials, and increased release of toxic trace elements may thus occur still over longer time (Dupla et al., 2023). *Furthermore, future decreases in soil pH may re-release these elements into the environment (Kicińska et al., 2022). This way, the toxic trace elements could gradually leach through the soil into the ground water, potentially posing a risk to water quality and human health (P. Li et al., 2021; Sbai et al., 2024)."*

The conclusion was also adjusted: L539-542: "We therefore conclude that, in our experiment, crops mostly benefited from silicate application, with the largest benefits observed for basalt application. We did not find concerning accumulation of toxic trace elements in this short-term experiment, but this effect requires further verification through long-term monitoring.

The other points that were raised by the reviewers are adjusted as well and all changes are listed in the response letter.

Thank you!

Kind regards,

Jet, Arthur & Sara.

In their manuscript “Effects of basalt, concrete fines, and steel slag on maize growth and heavy metal accumulation in an enhanced weathering experiment” the authors present a 100 day mesocosm experiment with maize after the application of basalt, steel slag and concrete waste. The authors evaluate the changes in weathering, plant biomass, nutrient and toxic element contents as well es plant update using a dosage approach. The authors report biomass increased with basalt but not with the other amendments. Strong pH effects of the different amendments explain most of the observed effects. Finally, no critical contents of toxic elements but also little changes in nutrients in the plant biomass. The authors conclude that there is now risk of toxic element accumulation in plants and basalt has the most positive effect on biomass. This is an important study and the experimental design (even with only one replicate per treatment, which is due to the high number of replicates) is solid. The study is an important contribution to understand the effects of silicate rich material on soil-plant systems in the framework of ERW. However, the authors miss an important limitation. This is the duration of the experiment. The authors identify that the pH effect is controlling most of the observations. The basalt has the lowest pH effect while the carbonate containing steel sag and concrete waste have stronger effects. As the authors discuss, the nutrient and toxic element availability depends highly on the pH. The authors discuss that the concrete waste and steel sag even overshoot preferential pH conditions for plants (reducing the agronomic benefit of such practices). The long-term effect of such liming effects is not clear. Therefore, the toxicity and bioavailability of the toxic elements might have been undetected given the duration and maintained pH. When the pH decreases with time, this might shift dramatically. The toxic elements might accumulate and at a given pH a much larger load will be bioavailable. This study is still a valuable contribution. But the general conclusion needs to be done with a clear statement of such limitations.

Response: we thank the reviewer for the positive assessment of our work. We agree that the duration of the experiment is a limitation that warrants discussion and we added the following paragraph to the discussion:

Line 513-519: “While at first sight reduced toxic trace elements in the plant may be beneficial for crop production, increased porewater concentrations, potential accumulation in the plant roots, and the immobilization of these elements in the soil may pose an environmental risk in the longer term. Our short-term experiment did not allow for complete weathering of the silicate materials, and increased release of toxic trace elements may thus occur still over longer time (Dupla et al., 2023). *Furthermore, future decreases in soil pH may re-release these elements into the environment (Kicińska et al., 2022). This way, the toxic trace elements could gradually leach through the soil into the ground water, potentially posing a risk to water quality and human health (P. Li et al., 2021; Sbai et al., 2024).”*

The conclusion was also adjusted:
L534-537: “We therefore conclude that, in our experiment, crops mostly benefited from silicate application, with the largest benefits observed for basalt application. We did not find concerning accumulation of toxic trace elements in this short-term experiment, but this effect requires further verification through long-term monitoring.

The authors start with the big picture of climatic effects of ERW and thus CDR potential. They never come back to this in the manuscript discussion. However, they argue that concrete waste and steel slag have much higher weathering rates given the DIC compared to basalt. As mentioned before, concrete waste and steel slag contain substantial amounts of CaCO3.

Therefore, it is a simple carbonate dissolution for these materials, which has no CDR effect (see also below). This is another strong limitation when thinking about CDR potentials of different materials. The high weathering rates of the steel slag and concrete waste could therefore miss-interpreted as strong CDR potentials. Therefore, the authors should include a critical discussion about the links of their findings to climatic effects.

Response: We thank the reviewer for raising this important point. Initially, we did not include further discussion about the CDR potential because the manuscript focusses on the plant responses. Nonetheless, we agree that this may lead to misinterpretation of our results. We therefore added the following text to the discussion:

366-375: "As expected, increases in DIC were lowest for basalt compared to concrete fines and steel slags, indicating a higher weathering rate for the latter two silicates. This was accompanied by a higher initial increase in soil and pore water pH for steel slag and concrete fines. Concrete fines used in this study contained about 18% calcite, and steel slag about 9%. Calcite weathers faster than silicate minerals (Berner et al., 1983; Lehmann et al., 2023), potentially explaining the higher increase in DIC with concrete fines and steel slags compared to basalt, as basalt does not contain calcite. Even though the CO2 removal of these silicates is not part of the current study, we want to emphasize that calcite weathering does not contribute to long-term carbon capture, and has a risk of reversal if carbonates reprecipitate downstream after leaching (Berner et al., 1983; Lehmann et al., 2023). In other words, the higher increase in weathering products with concrete fines and steel slag in our experiment does not imply a proportionate increase in CO2 removal.".

I include more details and unclear aspects in my specific comments below:

Line 24-25: it should be clear that such practices rely on the transport of the weathering products (e.g. cations and bicarbonate) to the aquatic/oceanic ecosystems where they precipitate as carbonate. This is a large uncertainty. Currently it reads like the CO2 is directly stored on site in the soil.

Response: We thank the reviewer for pointing out this unclarity and we made the following adjustment to line 25-27: "When silicates react with water and CO_2, (bi)carbonates are formed that can be transported to the ocean via leaching into the groundwater, possibly storing carbon (C) for centuries and longer (Moosdorf et al., 2014)."

Line 30: Unfortunately, it is more and more common to name the C capture by weathering "sequestration". Sequestration is in most cases considered to involve the assimilation of atmospheric CO2 by plants/organisms (Don et al 2023). I would recommend to use "atmospheric CO2 removal" here as this is also conform with CDR.

Response: We agree with the suggestion of the reviewer and made the following adjustment to line 32: "In addition to its atmospheric CO_2 removal potential, "

Line 31-37: Here the authors list in several sentence many effects that can improve plant productivity and soil functions. The use of furthermore, moreover and additionally does not make fully sense here as all effects are more or less the same. For example, CEC increase is related to the Ca and Mg, shifts pH and might shift the physical soil properties. This could be rephrased here.

Line 39-40: This can be integrated in the previous paragraph

Response: We agree and made the following adjustment to line 32-42:

"In addition to its atmospheric CO_2 removal potential, applying silicate minerals to soils holds promise for improving agricultural practices. When silicate minerals weather, protons are consumed and weathering products, such as Ca^{2+} and Mg^{2+}, are released (Kelland et al., 2020; Ramos et al., 2022). This can improve soil chemical properties such as increasing soil pH and cation exchange capacity (CEC), and improvement of soil water retention (Anda et al., 2015; Calabrese et al., 2022; Taylor et al., 2017). Soil acidification and nutrient leaching are pervasive issues in agriculture, and EW can in this way contribute to soil health and improve crop growth (Tilman et al., 2002). Additionally, even though not considered an essential plant nutrient, the process of EW releases silicon (Si), which can improve plant resistance to pests and diseases, thereby improving crop health and productivity in general (Calabrese et al., 2022; Swoboda et al., 2022). Because of these benefits, silicate rock powder has been used as a fertilizer for many years (e.g. Van Straaten, 2006), particularly in tropical regions, where the release of base cations from these rocks can significantly enhance crop productivity (e.g. Swoboda et al., 2021)."

Line 54-55: Can you provide a reference that most studies have been performed in the tropics. Most of them might not have involved the CDR aspect of ERW but rather the soil re-fertilization.

Response: We added a reference to line 55: (Swoboda et al., 2022).

Line 62-64: This is a very important point here. Thanks for making this.

Line 67-70: Can such by-products contain carbonates? If so, they would weather fast due to the fast dissolution of carbonates. The CDR effect, however, would be low. Considering the IPCC, carbonate dissolution by, e.g. liming, is net zero because of the release of all CO2. The net CO2 effect of carbonate dissolution is also presented in Hartmann et al. (2013)

Hartmann, J., West, A. J., Renforth, P., Köhler, P., De La Rocha, C. L., Wolf-Gladrow, D. A., Dürr, H. H., & Scheffran, J. (2013). Enhanced chemical weathering as a geoengineering strategy to reduce atmospheric carbon dioxide, supply nutrients, and mitigate ocean acidification: ENHANCED WEATHERING. Reviews of Geophysics, 51(2), 113–149. https://doi.org/10.1002/rog.20004

Response: We thank the reviewer for making this point. Indeed, these by-products do contain a certain amount of carbonates. More information about carbonates is added in lines 72-78:

"Concrete waste has previously been applied to soils to improve plant growth. However, how it affects plants is currently poorly understood (Ho et al., 2021). Concrete by-products are produced in large quantities because concrete is a popular product throughout the construction industry (dos Reis et al., 2020). Concrete fines also contain silicate minerals, and other cations, such as Fe, Ca, and Mg (Table 2), but almost 18% of the concrete fines used in this study is calcite ($CaCO_3$) (Table S1). Calcite dissolution does not lead to net

uptake of CO_2, because all CO_2 that is consumed during dissolution is returned to the atmosphere by precipitation of carbonates in the ocean (Liu et al., 2011). Therefore, weathering of concrete fines will probably be less efficient for carbon capture. "

Line 77-78: The three field trials are Branca et al (2014)?

Response: yes, these three field trials are discussed in Branca et al., 2014.

Line 79-80: And the fate of potential toxic elements is unknown.

Response: this is added to the text (line 85).

Line 83: I would prefer "toxic element" rather than "heavy metal" throughout the whole manuscript.

Response: we agree and replaced 'heavy metal' with 'toxic trace elements' throughout the manuscript.

Line 84-86: Also here, please provide evidence that most studies are in tropical systems. Additionally, it might be worth to mention that ERW is already considered for some countries in the temperate regions and thus a better understanding is needed. "justify further research" can be removed here.

Response: We agree and added a reference to a synthesis study (Swoboda et al., 2022), and made the following adjustments to lines 89-91:

"Most research so far has been conducted in a tropical climate (Swoboda et al., 2022), often on highly weathered and acidic soils, but EW is currently considered for application also in other climate regions and thus a better understanding is needed. "

Line 91: H1, reads like the authors expect that weathering rates increase. I assume the absolute quantities of weathering products are hypothesized to increase.

Response: We agree and made the following adjustment to line 96: HI: "Availability of weathering products will increase with increasing silicate application amount".

Line 122: I assume the five replicates for the 50 t/ha basalt treatment are selected since this is the commonly discussed application rate. The authors do not really advance here on the variability observed between the replicates. This would be helpful.

Response: Indeed, the 50 ton per ha of basalt was selected because it was commonly used in enhanced weathering experiments. We added the following in the method section (lines 121-124): "For basalt, there were five replicates of 50 ton ha^{-1} because it is commonly used in previous studies about EW (Gillman et al., 2001; Swoboda et al., 2022)."

We also agree that the variability deserves some attention, and we therefore added a paragraph in the discussion about this (lines 381-386): "The observed variability in nutrient concentrations, especially Si, for the control treatment and the 50 ton ha^{-1} of basalt is likely due to local differences in soil conditions and fluctuations in porewater chemistry. Despite *this variability, significant differences among treatments were identified. However, because this study did not include replicates for the other application amounts, the extent of variability could not be confirmed."*

Table 3: The 5 controls were for each treatment the same correct? it reads here like 15 controls were used with 5 for each treatment

*Response: We indeed used 5 controls, and agree that it can be clarified. Therefore, we updated the table heading: "**Table 3:** Application rates of basalt, concrete fines and steel slags and number of replicates for each application rate. The five replicates where no silicate material was applied are the same mesocosms for the three treatments, i.e., the experiment contained five control treatments."*

Line 160-161: Why was there one week between aboveground and belowground sampling? This could bias the belowground biomass to some extent.

Response: there was a week in between the above- and belowground biomass sampling because of time limitations. The harvesting and separating of the aboveground biomass in different plant parts was labour intensive, and made it not possible to harvest the roots earlier. Although this delay may have had a small effect on the belowground biomass, we do not expect any treatment bias in our results because the delay occurred for all treatments (and treatments were harvested in random sequence).

Line Is 161-165: The authors considered here only the roots that were in 100 cm^3 cores and extrapolate with the assumptions of 50% surface coverage in the centre of the mesocosm and 25% under the plants. This seems to be potentially highly biased considering the root network of maize. The actual total root biomass by root washing was not determined to prove that such simplification can be made here?

Response: We agree that this is a rough estimation, but due to the large pot size, it was not possible to harvest all soil for root sampling. Our approach was based on visual inspection of the plots and experience in previous experiments with maize. Importantly, even though the absolute values may not be fully accurate, we expect that our sampling approach did not affect the differences between the treatments and therefore does not invalidate our conclusions. Similar methods are used in:

Ven, A., Verlinden, M. S., Fransen, E., Olsson, P. A., Verbruggen, E., Wallander, H., & Vicca, S. (2020). Phosphorus addition increased carbon partitioning to autotrophic respiration but not to biomass production in an experiment with Zea mays. Plant Cell and Environment, 43(9), 2054–2065. https://doi.org/10.1111/pce.13785

Verlinden, M. S., Ven, A., Verbruggen, E., Janssens, I. A., Wallander, H., & Vicca, S. (2018). Favorable effect of mycorrhizae on biomass production efficiency exceeds their carbon cost in a fertilization experiment. Ecology, 99(11), 2525–2534. https://doi.org/10.1002/ecy.2502

In our revised manuscript, we added a brief discussion on this point:
Line 161: "Ven et al. (2020) used a similar method, and they were able to close the C balance, demonstrating its accuracy. "

Line 169-174: Was the biomass dried before and how was extracted prior to ICP-OES analysis?

Response: This is already mentioned in line 164: "After drying for 48h at 70 °C, the dry weight (dw) of each plant part was determined."
The digestion is explained on lines 173-176: "For each plant sample, 0.3 g was weighed and digested with H_2SO_4, salicylic acid, H_2O_2 and selenium to determine Ca, Fe, K, Mg, and P and the heavy metals listed above according to Walinga et al. (1989). Si was determined by digestion of 30 mg plant sample with 25 mL O.5 N NaOH."

Line 188-189: From which depth were the samples taken?

Response: We agree that this should be mentioned, and we added the following to the manuscript on line 187: "from right underneath the soil surface (+ 1 cm).".

Line 224: What is the reason for the decrease in soil pH at day 20 for the basalt and the concrete fines?

Response: This is potentially related to heavy rainfall in the days before the sampling period, decreasing soil pH.

Figure 2: X axes should be the same everywhere to better follow the different times of sampling. Another visual improvement could be done by using a different symbol for the controls. It is hard to differentiate between treatments and controls. This applies for all other similar figures.

Response: agreed, we adjusted the X-axis and use a different symbol for the control treatment.

Figure 3: This figure shows that the replicates of the basalt 50 t/ha treatment are in some cases quite variable. Given the fact that the other application rates have no replicates, the variability should receive some attention here.

Response: We agree with the reviewer and added the following to the discussion (lines 383-386): "The observed variability in nutrient concentrations, especially SI, for the control treatment and the 50 ton ha^{-1} of basalt is likely due to local differences in soil conditions and fluctuations in porewater chemistry. Despite this variability, significant differences among treatments were identified. However, because this study did not include replicates for the other application amounts, the extent of variability could not be confirmed."

Figure 5: PC 5 explains only a minor fraction of the variance here.

Response: Indeed, PC5 shows only 6.6% of the explained variance in the soil, but it clearly separates treatments with concrete fines, and steel slag. We therefore added PC5 to the figure, as it indicates which group of soil chemical characteristics differ between these treatments.
We added the following to the results on line 263-270: "While PC2 (positively correlated with Ca, Fe, Mg, Si, and K porewater concentrations, but negatively with soil CEC, porewater Cr

and DIC) did not differ among the treatments, PC3 (negatively correlated with CEC, porewater Ni, Pb, and Mg) and PC4 (negatively correlated with porewater Cr and pH, and positively with porewater V concentrations) showed significant differences among treatments (Fig. 8, Table S7, Fig. S5). PC3 is significantly lower with basalt compared to the control, while PC4 is significantly higher with steel slag compared to concrete fines (Table S7, Fig. S5). The differences among concrete fines and steel slag were more prominent for PC5, which only explained 3.1% of the variance less than PC4 (Fig. 5, Fig. S5, TableS7)."

Line 306-310: Would the authors expect changes in C:N? Given the very high number of figures and presented data in the manuscript, this could be moved to the SI since no important effects are found.

Response: We thank the reviewer for this suggestion, however, the lack of effects of silicate application on the C/N ratio suggests that silicate application did not negatively affect the nitrogen balance in the crops, which is an aspect of crop quality. Even though changes upon silicate application are not per se expected, we prefer to keep this figure in the text because of the importance of C and N, and their ratio, for crop growth.

Line 314: the p = 0.08 is not significant here for the tassel Ca. What does "borderline" mean here?

Response: text adjusted (lines 313-314): "Concrete fines application did not affect plant Ca concentrations, except for significantly increased root Ca concentration and a tendency of decreased tassel Ca concentrations (p=0.08) (Fig. 12)."

Line 335: Why was Pb in the control?

Response: the soil came from a pasture in Zandhoven, Belgium, and our data indicate that this soil already contained low amounts of Pb. The origin of the soil was added to the methods (Line 105-107):

"In May 2021, the bottom 40 cm of each mesocosm was filled with a sandy-loam soil obtained from a pasture in Zandhoven, Belgium (Table 1)"

Line 345: Also here "borderline" is unclear. You could say a non-significant trend for the Ni in stem.

Response: We agree and adjusted the text as follows (line 349-350): "and a non-significant trend of decreased stem Ni concentrations was observed with increasing concrete fine application amount"

Line 361-362: The steel slag and the concrete fine have substantial amounts of calcite (8.6% and 17.9%, respectively Tab. S1). Thus, it is most likely a dissolution of this carbonate. As mentioned before, carbonate dissolution as no effect on CO2 capture. The CO2 capture is the big picture of the presented manuscript. Therefore, the authors should critically discuss that such strong effects of the sleet slag and concrete waste do not indicate solely the weathering of silicates that would result in a CO2 capturing.

Response: We agree and made the following adjustments to line 368-374:
"Concrete fines used in this study contained about 18% calcite, and steel slag about 9%.

Calcite weathers faster than silicate minerals (Berner et al., 1983; Lehmann et al., 2023), potentially explaining the higher increase in DIC with concrete fines and steel slags compared to basalt, as basalt does not contain calcite. Even though the CO2 removal of these silicates is not part of the current study, we want to emphasize that calcite weathering does not contribute to long-term carbon capture, and has a risk of reversal if carbonates reprecipitate downstream after leaching (Berner et al., 1983; Lehmann et al., 2023). In other words, the higher increase in weathering products with concrete fines and steel slag in our experiment does not imply a proportionate increase in CO2 removal.

Line 394-395: This is a too simplified conclusion here. The additional root biomass may also induce priming effects. The effects are not clear yet. Therefore, the authors should reconsider this conclusion here. Some literature:

Sokol, N. W., Sohng, J., Moreland, K., Slessarev, E., Goertzen, H., Schmidt, R., Samaddar, S., Holzer, I., Almaraz, M., Geoghegan, E., Houlton, B., Montañez, I., Pett-Ridge, J., & Scow, K. (2024). Reduced accrual of mineral-associated organic matter after two years of enhanced rock weathering in cropland soils, though no net losses of soil organic carbon. *Biogeochemistry*. https://doi.org/10.1007/s10533-024-01160-0

Schiedung, M., Don, A., Beare, M. H., & Abiven, S. (2023). Soil carbon losses due to priming moderated by adaptation and legacy effects. *Nature Geoscience*. https://doi.org/10.1038/s41561-023-01275-3

Response: Text adjusted on line 410-412:
"Furthermore, increased root biomass with basalt application may indicate higher belowground C inputs by plants, but can also increase microbial activity leading to accelerated soil organic matter decomposition, which can impact soil organic C stocks (Fu & Cheng, 2002; Kögel-Knabner et al., 2022; Kuzyakov, 2002).".

Line 404-405: It is interesting that for the basalt treatment the pH effect is large in the beginning but even during the short time frame of the experiment, the values are very similar for all application rates and controls at day 100. This indicates that the liming effect of basalt is highly limited. It is in many cases argued that basalt dust could act as a lime substitute. However, if the pH effect is only short this will not be the case. Even for the soil pH, the highest application rate has lower pH values at day 100 than the control. The other two materials that contain carbonates have a much stronger and long-lasting effect on the soil and pore water pH.

Response: Thank you for this consideration. However, as pH is a logarithmic scale, even small increases can have a large impact. Only one measurement is below the control treatment, and our overall statistics show that pH increased with the application of all silicate materials. Furthermore, there is only one observation below the control treatment, which is likely related to the variability of the system that is also sensitive to e.g. differences in precipitation. The pH initial pH increase was indeed higher with concrete fines and steel slag, likely because of the fast weathering of calcite. A few sentences have been added in the discussion accordingly:
Line 369-374: "Calcite weathers faster than silicate minerals (Berner et al., 1983; Lehmann et al., 2023), potentially explaining the higher increase in DIC with concrete fines and steel

slags compared to basalt, as basalt does not contain calcite. Even though the CO2 removal of these silicates is not part of the current study, we want to emphasize that calcite weathering does not contribute to long-term carbon capture, and has a risk of reversal if carbonates reprecipitate downstream after leaching (Berner et al., 1983; Lehmann et al., 2023). In other words, the higher increase in weathering products with concrete fines and steel slag in our experiment does not imply a proportionate increase in CO2 removal."

Line 458-464: This paragraph is not clear to me. As the authors state, the N effect might be irrelevant as all treatments received similar and high N with fertilization. The interpretation of NUE is a far stretch to me and not convincing here.

Response: We agree, as we did not measure N use efficiency. We made the following adjustment on lines 474—475:
"Given the biomass increase with basalt and no decrease with concrete fines, the elevated C/N ratio does probably not indicate N limitation, particularly given that the mesocosms received the same amount of N fertilization."

Line 464-474: A strong limitation here is the duration of the experiment. The long-term effects remain unknown. It is likely that the release of toxic elements is slower and thus the bioavailability takes longer that captured here.

Response: This is indeed important to mention, thank you. To address this, the following was added on line 515-519:

"Our short-term experiment did not allow for complete weathering of the silicate materials, suggesting the possibility of increased release of toxic trace elements over time. Furthermore, future decreases in soil pH may re-release these elements into the environment (Kicińska et al., 2022). This way, the toxic trace elements could gradually leach through the soil into the groundwater, potentially posing a risk to water quality and human health (P. Li et al., 2021; Sbai et al., 2024)."

Line 513-515: Also here the duration of the experiment is important to consider. It is not clear if the bioavailability would be higher with the next season when pH effects diminish and toxic elements will become more bioavailable.

We agree and adjusted the text on lines 539-542: "We therefore conclude that, in our experiment, crops mostly benefited from silicate application, with the largest benefits observed for basalt application. We did not find a concerning toxic trace element accumulation in this short-term experiment, but this effect requires further verification through long-term monitoring."

Within the context of enhanced weathering for atmospheric CO2 removal, the authors present an experimental mesocosm study exploring the impact of basalt, concrete fines, and steel slag applications on the growth of maize and potential accumulation of toxic heavy metals. Overall, the authors find evidence of weathering and increased plant growth (with basalt), while they did not observe accumulation of heavy metals in the plants.

I think this is an important study investigating in detail the potential co-benefits and side effects of enhanced weathering, when widely available basalt or industrial materials (concrete, steel slags) are used. I found particularly interesting the dose-response approach, as it may provide more insights into the biochemical mechanisms that make a certain element more or less available. Also, the consideration of concrete fines and steel slags is valuable, as these are being actively considered as potential silicate materials that dissolve relatively fast and favor a circular economy, but there have been concerns for their potential environmental toxicity.

For the reasons mentioned above, I recommend publication after addressing my comments below.

We thank the reviewer for this positive evaluation of our study.

Within the context of enhanced weathering, I think it is important to always consider the CO2 removal aspect, so that co-benefits or side effects can be evaluated relative to the CO2 being removed. This aspect is barely mentioned in the manuscript. In this work, I believe that from the composition of the silicate materials, the authors could obtain an estimate of the potential CO2 removal, calculated based on stoichiometry. In addition, it is not clear whether the authors are collecting and analyzing the leachate, but this could provide an estimate of the actual CO2 being removed and of the weathering rate. If not from the leachate, can the authors estimate the CO2 removal from their measurements? Especially after the first application of silicate materials, the weathering rates are expected to be the highest.

Response: We agree that the CO2 removal potential is a critical aspect of enhanced weathering. However, accurately estimating CO_2 removal is complex and requires in-depth investigation of the soil processes. We did perform such analyses, and these are being compiled in a separate manuscript that provides an in-depth assessment of the weathering rates and CO2 removal in our experiment. In the current manuscript, we opt to focus on the co-benefits and potential risks of silicate applications for agriculture. In response to this comment, we added the following information to the revised manuscript (line 144-145): "Analysis to determine the CO2 removal potential in this study were performed and an in-depth assessment of the weathering rates and CO2 removal are presented in Vienne et al (submitted).".

I don't think this is mentioned explicitly, but the experiment apparently lasted for only 1 season (~100 days). While this may be enough to capture the potential benefits on plant growth, perhaps this is not long enough to capture the accumulation of heavy metals, which is something that occurs over time. In fact, to achieve meaningful CO2 removal, multiple applications may be needed over time. The concern is that heavy metals may cause harm over time, as the soil capacity to retain them is exhausted and they may become more available for plant uptake or even leach with water, potentially affecting water quality. In fact, the problem with heavy metals would not be only for plants, but also for the ecosystem more broadly. I understand the experiment took place in 2021, but I think the authors should discuss these aspects more explicitly.

Response: We agree that the duration of the study is an important consideration regarding heavy metal toxicity and a paragraph was added:
Line 513-519: "While at first sight reduced toxic trace elements in the plant may be beneficial for crop production, increased porewater concentrations, potential accumulation in the plant roots, and the immobilization of these elements in the soil may pose an environmental risk in the longer term. Our short-term experiment did not allow for complete weathering of the silicate materials, and increased release of toxic trace elements may thus occur still over longer time (Dupla et al., 2023). *Furthermore, future decreases in soil pH may re-release these elements into the environment (Kicińska et al., 2022). This way, the toxic trace elements could gradually leach through the soil into the ground water, potentially posing a risk to water quality and human health (P. Li et al., 2021; Sbai et al., 2024)."*

Additional minor comments:

The authors should clarify whether the mesocosm includes a mechanism for water drainage from the bottom.

Response: this is already included in the methods line 134-136: "Each mesocosm was equipped with a 2 cm diameter hole at the bottom for leachate collection, and a root exclusion mat covered the bottom of the mesocosm to prevent soil export through leaching. Glass collectors with a volume of 2.3 L were connected to the mesocosm via polyurethane tubing."

The font size used in figures should be increased.

Response: We increased the font size in the figures of the revised manuscript.

In figure 2, the x axis tick marks should be standardized (for example, all could be 25-50-75). Also, the text on one panel is sometimes overlapping with another panel. Please double check the spacing between panels.

Response: We modified this accordingly in the revised manuscript.

Line 103-104. I believe there is no need to repeat "to a depth of 40cm in the bottom portion".

Response: We thank the reviewer for noticing this and deleted this part.

Line 162. Should the parenthesis be moved to the end of the sentence? Or probably I am not understanding this sentence, as it starts with "one core below the shoot" but then I says "two cores per mesocosm for each layer" in parentheses. Could the authors clarify?

Response: We agree that this is not clearly written, and improved the sentence (lines 159-160: "One soil core (100 cm³) was taken below the shoot of each plant (thus a total of two cores per mesocosm for each soil layer, as each mesocosm contains two plants), and one core for each soil layer at the centre of the mesocosms.