

Dear authors,

Both reviewers have sent me their assessment of your revised work. It is clear they are both happy with your changes. Reviewer one has one final suggestion on an element that should be added to the discussion, on how this work can or cannot be generalized to other systems. I think the suggestion is justified, and I would encourage you to add that to the paper. Once this is done, I will happily accept the paper for publication in Biogeosciences.

All the best,

Marijn

Dear editor,

We thank you for positive feedback and are pleased to hear that both reviewers are happy with the revisions.

We agree with the additional suggestion from reviewer one and we have incorporated this into the discussion on lines 524-526: “Our findings provide valuable insights into the impact of EW on *Zea mays* using basalt, concrete fines and steel slag. However, the effectivity, benefits and risks of EW can vary with soil characteristics and the specific silicate materials used (Abdalqadir et al., 2024; Lewis et al., 2021).”

Additionally, we replaced ‘CO₂ sequestration’ with ‘CO₂ removal potential’ in the abstract (Line 11), as requested by reviewer one.

With kind regards,

Jet, Arthur and Sara

The authors revised the manuscript and addressed my comments in a satisfactory manner. This is a valuable contribution to the field of ERW and using rock dust but also other products. The uncertainty regarding carbon dioxide removal are much clear and the frame of the study with a plant focus improved. Also the experimental design is clear now.

I only would like to add that the authors should be clear in the discussion that not only the time but also only one soil limits the generalization of the results. Thus, it would be good when the authors clarify that longer experiments that include a larger variety of soils but also amendments are needed. Many of the effects will be site and feedstock specific. The authors cannot extent on this but the limitation of generalization should be clear. Nevertheless, it is an important study and it is clear that combining dose variation with all potential variables would not be in the framework of a feasible experiment.

In the abstract is still the terminology of CO₂ sequestration used. The authors agreed to use CO₂ removal and thus it should be revised for consistency.

Response: We thank the reviewer for the positive assessment of the revised manuscript. We agree that it is important to mention that our findings are specific for our soil and that the impact of EW on crops can vary across different soil types and feedstock added. Therefore, we added the following to the discussion:

Lines 524-526: *“Our findings provide valuable insights into the impact of EW on Zea mays using basalt, concrete fines and steel slag. However, the effectivity, benefits and risks of EW can vary with soil characteristics and the specific silicate materials used (Abdalqadir et al., 2024; Lewis et al., 2021).”*

We also replaced “CO₂ sequestration” by “CO₂ removal potential” in the abstract (line 11).