

Comments review

This revised MS has a well improved introduction and discussion section, and contains an elaborate discussion about the possibilities and mechanisms on why CO₂ uptake was larger in basalt amended soils. We have some minor remarks which, when taken into account, will warrant publication of this MS.

- Abstract: Please also update the abstract such as not to frame ERW as mitigation but as a carbon dioxide removal technology. (see also comment 19 in the first round of review);
-
- Abstract: Please also state in the abstract that the marginalized soil was limed and fertilized prior to basalt amendment and was at near neutral pH (to avoid the misinterpretation that this is a study about basalt application on acid soils)
- Removing topsoil: ‘marginalization’. Please specify how many cm was removed?
- Can the authors specify which feldspars/plagioclases are present?
- Line 125: space is missing between 10 and t/ha
- Table 1: Please explain color coding in the caption.
- In Table 2 you use SIC and SOC, in other places in the manuscript PIC & SIC, and there is also mixed use of both abbreviations (line 214). Please make the use of these terms consistent throughout the manuscript
- Section 2.7 Rainwater flux. Please explicitly state that you ignore DIC in rainwater, perhaps with a reference as support.
- L180-200 Adapt formulae such that it is clear that you compute a difference in soil carbon stock ($\Delta(\text{SOC}+\text{SIC})$) as a sum/integration of gas-, leachate-, and sampling fluxes ($\Sigma(\text{F}_{\text{CO}_2} + \text{F}_{\text{CH}_4} - \text{F}_{\text{leachate}} - \text{F}_{\text{sampling}})$) and a difference of the carbon stock in plant ($\Delta(\text{plant}_C)$).

Particularly equation 3 L199 reads as an equation solely in carbon stocks, while some are integrated fluxes. Also in the text there is sometimes confusion between (difference in) stock and fluxes.

- L185 Please motivate why DIC in leachates should be negligible, as it was only measured in pore waters throughout the depth profile. You could refer to the smaller DIC relative to DOC at pore water at 20 cm depth (Table 2) and extrapolate this to the leachate. As leachate DOC had a negligible effect in the total C mass balance and we expect smaller DIC, changes in leachate DIC among treatments should thus be negligible in the total mass balance.
- L214 Add a “.” between SOC and PIC.
- L216 “carbonates ions” -> “carbonate ions”
- L250 Soil Ca and Mg concentrations as proxy for the EW reaction -> change to “Ca and Mg concentrations as proxy for weathering”. See also comment below about the conclusion section.
- L251 “To account for the carbonates which might have initially formed in the soil but later degassed as CO₂ during sample storage prior to analysis, we measured divalent cations (Ca²⁺, Mg²⁺), resulting from silicate mineral dissolution.”

Please delete this sentence, degassing of CO₂ due to a change in CO₂ pressure does not change Ca and Mg concentrations and vice versa.

- L416 (equivalent to 10 tonnes per hectare) please remove this part between brackets or make it clear that this is the application rate of basalt, and not the carbon removal rate.
- - L511 “We had no evidence that the EW reaction took place” – please be more specific. In general this conclusion section can be strengthened by pointing out that you don’t measure inorganic carbon sequestration but you find significant organic C sequestration. This means that the indirect effects of weathering (such as the ones that you have already discussed) are much more important than the direct/geochemical effects.

