Reviewer 2: In CDR, the idea is that alkaline rock captures CO2, while generated secondary minerals and increased cation exchange capacity (CEC) could enhance plant productivity and soil organic carbon (SOC) retention in soils. By studying different soil fractions, authors show that CO2 capture would be minimal, thus MS is tuning down hopes that basic rocks can mitigate CO2 buildup to atmosphere. Manuscript is interesting to me, who is novice about the subject. MS gives an idea how difficult it is to estimate how much carbon is ultimately stored at least for longer periods and how to estimate it.

We thank reviewer 2 for the interest in our findings.

Q50) MS gives a good introduction to subject, even could be in many points more accurately written (lot of typos) and also quite lengthy (25 pages on supplementary material). My comments may be inaccurate and not totally on topic. Furthermore, I cannot comment much on methods. I am wondering about some conventions on this branch of soil science, and also supriced how far "real" carbon balance measurement community is from CDR world. This said as an excuse to begin with.

We sincerely thank the reviewer for their thoughtful comments and for openly stating the limits of their expertise in some areas. We appreciate the time and care taken to engage with the manuscript.

Regarding the length of the supplementary material, we agree that it is extensive. However, due to the large number of analyses and visualizations required to transparently document our results, there was no suitable alternative to placing them in the supplement. We felt this was the most appropriate way to ensure full reproducibility and clarity without overwhelming the main manuscript.

Q51) If I got it right, the benefit for long term CDR "0.4-0.5 kg CO2 ton-1 basalt or 0.020-0.025 tCO2 ha-1 for a basalt application rate of 50 t ha-1CDR" by enhanced weathering. So total climatic impact is rather the opposite to aims of basalt rock dust application. When energy consumption on transport, spreading and crushing of basalt is added to equation. However, this is not said enough clearly, at least to a reader which is not an expert on this subject.

We thank the reviewer for this important observation. We agree that the net climate impact of enhanced weathering must account not only for CO_2 removal through basalt application but also for emissions associated with the sourcing, crushing, transport, and spreading of the material. In the revised manuscript, we now make this point explicit in the discussion. We also note that the updated long-term CDR potential, after increasing η to 1 (cf Q19) and including non-significant alkalinity slopes, was 26.33 ± 6.13 kg CO_2 ton^{-1} basalt (Table 4).

For comparison, Lefebvre et al. (2019) estimated that, in Brazil, for 65 km transport, 75 kg CO2 is emitted/ton CO2 removed by enhanced weathering due to life cycle emissions. They assume 0.225 ton CDR/ton basalt: 75 kg CO2 emission / t CDR * 0.225 ton CDR/ ton basalt = 16.87 kg CO2 emitted/ton basalt applied. This is substantial and should be included in every C crediting methodology.

We incorporated this in the discussion, on line 540: Moreover, life-cycle emissions associated with mining, grinding and transporting rock are typically of the same order of magnitude as our relatively low potential inorganic CO2 removal (Lefebvre et al., 2019).

Q52) Reference to earlier studies near conclusions showing benefits of CDR, would help to understand this statement: “Our results suggest that EW using basalt amendments may not yield the immediate inorganic carbon dioxide removal (CDR) benefits previously anticipated”.

We agree with the reviewer and have clarified that the “previously anticipated” CDR benefits refer to projections from earlier studies and assessments, such as those discussed in Minx et al. (2018), which highlighted enhanced weathering as a promising negative emissions technology. We have therefore added this reference to provide clearer context. In addition, we add chapter 12 of the IPCC report from 2022 (Babiker et al. 2022).

line 626: Our findings indicate that basalt-based enhanced weathering may not immediately lead to the inorganic CO_2 removal previously anticipated in projections and IPCC reports (Babiker et al., 2022; Minx et al., 2018).

Q53) As I have been measuring mostly CO_2 as flux out from soil and DIC in lake water columns, I wonder how big emphasis is put on DIC concentration in MS to soil water DIC. And also to DIC flooding to sea. (When nothing got trough experimental mesocosmoses). Sea as the fate of carbon and the base level where to compare everything related to greenhouse gases? Why not Global Warming potentials used by IPCC? Maybe explained more in introduction, why conventional flux measurements are not enough, may help people like me to understand better why this methodology is used, As a novice, I think that just measuring CO2 emission with closed chamber in dark, measuring carbon gain to plant biomass (done here?) would add quite much to carbon capture (or carbon dioxide capture) measurements, when also DOC concentration is measured (done).

We thank the reviewer for this thoughtful comment. We agree that We agree that a comprehensive greenhouse gas (GHG) budget would ideally include all carbon fluxes and pools, including CO_2 fluxes from root respiration, SOM respiration, DOC, DIC, and biomass changes.. However, the focus of our study follows current EW monitoring and certification frameworks, which focus primarily on inorganic C removal via DIC export to the ocean, as this is considered the most durable form of C sequestration. We have clarified this rationale in the Introduction to help non-specialist readers understand why we focus on DIC and base cation dynamics rather than a full carbon balance.

Line 46: In this study, we focus on DIC export from soils to the ocean, as this pathway is considered the most durable form of carbon sequestration (Renforth & Henderson, 2017), rather than aiming to quantify a full greenhouse gas budget.

We also refer to complementary studies providing additional insights into plant biomass changes in this experiment (Rijnders et al., 2025). For an in-depth analysis of changes in CO2 fluxes, SOM and DOC we refer to the work of Boito et al. (2025 and Steinwidder et al. (2025).

In the revised manuscript we included this information on line 138: This experiment was part of a larger mesocosm experiment that aimed to investigate heavy metal fate and plant biomass in silicate amended maize plants (Rijnders et al., 2025).

And line 564: Although unfavourable for inorganic CDR, if base cation bearing secondary clay minerals would form, they can increase SOC (Georgiou et al., 2022; Heckman et al., 2022; Steinwidder, Boito, Frings, Niron, Rijnders, De Schutter, et al., 2025).

Q54) And still how to separate DIC coming from root exudates from that coming from weathering (without using carbon stable isotopes to label plant root exudates).

Thank you for this question. Indeed roots can also exude HCO_3^-, it is unclear if the amount of exudation of HCO_3^- by roots would be affected by basalt amendment. Nonetheless, we quantifiedy the weathering rate based on cation tracing, not on DIC. We added a sentence in the discussion to clarify that we cannot distinguish between DIC coming from root exudates and DIC derived from rock dissolution.

Line 470: Increased DIC in basalt soils relative to controls may result from enhanced plant root respiration or DIC exudation or from mineral weathering; our dataset does not allow these effects to be separated.

Q55) Authors note that “**The undesirable side-effect of base cation scavenging** (by plant/soil pools) **is release of CO2**”. I totally agree to this as a technical problem for measurements, but not as one who has done some respiration measurements and know that microbes do a lot, pH has an effect, out salting due to fertilization, concentration difference between atmosphere CO2 and DIC, wind and temperature also affect CO2 fluxes.

We think this was a misunderstanding. While we fully agree with the reviewer the soil CO2 efflux also depends on temperature, moisture, pH, here we were referring to degassing of inorganic C.

We rephrased this sentence to avoid confusion:~~;~~

Line 112: The undesirable side-effect of base cation scavenging (by plant/soil pools) is release of protons through charge balance, which convert negatively charged DIC (HCO_3^- and carbonate anions (CO_3^{2-})) to H_2CO_3 which is in equilibrium with gaseous CO_2 ($CO_3^{2-} + H^+ \rightarrow HCO_3^-$ and $HCO_3^- + H+ \rightarrow H_2CO_3 \Leftrightarrow H_2O + CO_2\,(g)$).

Q56) I think text needs to be clarified for broader audience, terms like CDR, TA, DIC, MRV approach etc. explained more clearly.

We agree with the reviewer and have more clearly defined CDR, DIC and TA. MRV was removed from the text:

CDR: As also addressed in Q4, we use (potential / realized) inorganic CO2 removal and avoid use of ‘inorganic CDR’. We do keep the abbreviation CDR as this is a commonly used acronym. We state the importance for CDR in the first line of the intro:

Line 42: To meet the "well below 2°C warming" target established by the United Nations' Paris Agreement, Carbon Dioxide Removal (CDR) must complement conventional climate change mitigation efforts (Minx et al., 2018).

We added a clear definition for TA (total alkalinity), see Q10.

DIC was better introduced: Line 49: DIC (the sum of aqueous [CO_2], [HCO_3^-] and [CO_3^{2-}]) can either be measured directly or estimated indirectly from total alkalinity (TA) or electrical conductivity, which are less expensive to monitor and can be empirically linked with DIC through calibration curves (Amann & Hartmann, 2022) (see also **Fig. S10**).

MRV: we replaced this by “monitoring C sequestration” to make it clearer to a broader audience, we replaced MRV by monitoring where needed, for example in these sentences:

MRV deletion: Line 131: Here, we make a mass balance after 101 days of experiment, investigate the fate of base cations through exploration of sequential extractions as ~~an MRV~~ a monitoring strategy for weathering and implications for C sequestration.

Line 602: However, this ~~MRV~~ monitoring approach involves complexities such as feedstock correction, leaching solution strength and soil heterogeneity.

Q57) Not start sentences with chemical abbreviations, like “Al” instead of Aluminum etc.

We screened the entire document and defined atoms/molecules when first used.

Q58) There were also lot of typos, mistakes and even repetition in reference list and also missing journal names. Easily corrected things, but these are annoying to reader. Even my comment are critical, still this MS has potential to be published in SOIL journal.

We thank the reviewer for noting these issues. We have carefully rechecked the entire reference list and corrected all typographical errors, missing journal names, and duplicated entries. Specific corrections include:

-Preprint that did not have a journal Kanzaki et al. (2024) was published after submission, replaced by Kanzaki et al. (2025)

-The preprints of Reershemius et al. (2023) and Reynaert et al. (2023) were cited in stead of the published MSs, these are now replaced by the correct citations of the published manuscript.

-Isometric, 2024 (not a scientific paper, replaced by Clarkson et al., 2024) see also Q42.

-Palandri et al. (2004) is the only reference still cited without journal name as this is a report from the USGS and not a scientific article

Q59) Some general notes, only few of the typos are shown in list. the name of MS ... inorganic CDR..., which is not written open thus hard to get interested at rapid look to those not familiar with CDR acronym.

We adapted this and refer to the modified title in Q5. In the revised manuscript, we consistently use inorganic CO_2 removal.

Q60) “SOM bound to cations in the exchangeable pool is expected to be more prone to microbial decomposition than SOM bound in the oxidizable pool”, a reference here after, or was it earlier?

We agree that this statement should be supported by a reference. We have added a citation to Poeplau et al. (2018) and clarified the reasoning by specifying that SOM in the exchangeable pool is extracted with weak salt solutions and typically represents the more labile carbon fraction with faster turnover.

line 206: SOM bound to cations, extracted with weak salt solutions in the exchangeable pool typically has a low turnover time (Poeplau et al., 2018) and is therefore thought to be more susceptible to microbial decomposition than oxidizable SOM.

Q61) 208 “in all aboveground biomass parts: stems, leaves, flowers and corn ears”. is it corn or maize, and ears?

Thanks for noticing that there was still a mention of ‘corn’ we decided to use maize throughout rather than corn. I fixed two cases where corn was still mentioned.

An "ear of maize" is the harvested part of a maize plant that contains the kernels, a cluster of seeds arranged on a cob and protected by a husk. We previously used corn fruit, but were advised that the correct terminology is a maize ear.

Supplement materials

Q62) Conductivity and DIC correlation jump in at Suplemental material, not discussed earlier. Is it a autocorrelation with amount of fertilizers etc.. maybe more discussion on this on main article, not discussion in supplement?

We agree that the relationship between DIC and electrical conductivity should be introduced in the main text rather than appearing only in the Supplement. We have now linked Figure S10 to the corresponding discussion in the main manuscript and clarified that these relationships are used to derive DIC from more easily measurable parameters, as described by Amann & Hartmann (2022).

Line 49: DIC (the sum of aqueous [CO_2], [HCO_3^-] and [CO_3^{2-}]) can either be measured directly or estimated indirectly from total alkalinity (TA) or electrical conductivity, which are less expensive to monitor and can be empirically linked with DIC through calibration curves (Amann & Hartmann, 2022) (see also **Fig. S10**).

Q63) In supplementary figure, Ph, DIC, TA and DOC conc. increase at 100 and 150 tn ha-1 application. pH may be the driving force, so scale better so that 7.0 is visible (now 6.8 – 7.2). (I think it is the border between acid and base - in water)

We adapted Fig. S6, so that the y axis of the pH panel (Fig. S6A) has axis ticks at every 0.2 units of pH rather than at every 0.4, so that it shows exactly where pH 7 is.

Q64) In sup. Fig S8 exp. days and application rate or captions are mixed, there was not 200 days in experimnt.

This has been corrected.

Q65) Unclear in table S4: Effects and significance: is this time or application amount in regression?

The effect of application amount in column 2 and time x application amount interactions in column 3, we indicated this in the first row of the table. N.S. = not significant.

Top soil pore water (0-10 cm)		
Parameter (unit)	Basalt effect and p-value	Time x basalt interaction effect and p-value
pH (-)	**+6.9 e-3 (p<0.01)**	**N.S.**
DIC (mg/L)	**+1.6 e-2 (p=0.27)**	**+4.9 e-4 (p=0.04)**