

Interactive comment on “Constraints on Enhanced Weathering and related carbon sequestration – a cropland mesocosm approach” by Thorben Amann et al.

Jessen (Referee)

sj@ign.ku.dk

Received and published: 4 December 2018

Amann et al. analyse results from a well performed mesocosm experiment. The experiment is apparently mainly aimed at deducing the rate of terrestrial carbon sequestration from soil amendments by dunite (olivine). The manuscript clearly addresses scientific questions of relevance for Biogeosciences journal.

Below I list general comments. However, please see also the attached PDF-file, which contains comments of both major and minor importance. My comments in this file and below may be perceived as very direct; therefore please allow me to express my deepest respect for the authors, their work, and their important choice of subject. I

[Printer-friendly version](#)

[Discussion paper](#)



truly believe the manuscript can become an important and significant contribution to the literature!

Yours sincerely
Soren Jessen

General comments

1. A set of CO₂-sequestration rates makes use of a correction for preferential flow. I find this problematic. The underlying reasoning for factoring up the inferred CO₂-sequestration is that lots of weathering take place in the statically held back solution, which is allegedly continuously bypassed by macropore flow.

However, even in the case of a dominantly preferential flow system, the net weathering rate would still be a function of the effluent water flux multiplied by its weathering product concentration and a stoichiometrical coefficient. The deduction of a preferential flow factor therefore is, as far as I am convinced, purposeless for estimating the CO₂-sequestration.

Furthermore, elsewhere in the manuscript the authors state that “extended periods of drying out” resulted in “slowed down or ceased chemical weathering processes”, which is quite the opposite to the assumption made above of quantitatively important reactions in zones of static water. So there is a contradiction here which also needs to be solved.

2. The manuscript concludes that CO₂-sequestration “was shown” (without the above correction). I disagree: DIC needs to percolate to the groundwater table and beyond, and/or form (stable) carbonate minerals and/or org. C stocks. The latter two are not measured, and effluent DIC concentrations are (according to the manuscript) measured too infrequently to be applicable. The use of Mg as a proxy for DIC need to be carefully documented by data (not just by theoretical reaction stoichiometry) before it

can be used as direct evidence. I think the authors should include a discussion (in the Introduction) of the requirements for enhanced weathering to actually be achieved.

In my opinion, the 'fixes' for the manuscript to 1) and 2) above might include to acknowledge that the elements under consideration (e.g., Mg) are non-conservative, and that the retarding processes are not investigated mechanistically in this study (at least, the data were not shown). Therefore the preferential flow-calculation (which uses Mg) must be skipped, and the tight conclusion regarding the sequestration must be softened. While the sequestration cannot be said to be "shown", in my opinion, I do think the authors could safely say that their results 'indicate a potential' CO₂-sequestration of X t C/ha/yr.

3. A thorough analysis of the water balance for the mesocosms needs to be presented. The water balance need to include an evaluation of the transport time for water through the mesocosms.

4. The overall purpose of the study appears to be slightly blurred. The focus on estimating CO₂-sequestration rates infers that this was the main aim. (However, one must then ask why DIC in the effluent was not more carefully measured, ie., what were the mesocosms designed for?)

Another purpose appears to be to demonstrate the use of dunite as a "model mineral" for enhanced weathering experiments. (But then, why the strong focus on the estimation of CO₂-sequestration rates and trace elements, which implies a focus on field applicability?)

The authors should state the objective(s) more clearly.

5. Presentation and structure: The manuscript needs to undergo a major revision in terms of the structure, conciseness of the text, and its figures. For example, the manuscript contains many repetitions and many imprecise statements. Also, many results are presented in the Discussion and some discussion take place in the Results

[Printer-friendly version](#)[Discussion paper](#)

section. Some results were not presented (or did I miss them during my reading?), but were still referred to/used in the Discussion. The artwork need to be polished; generally, the figures in the supplementary information seem to be better worked through than the figures in the manuscript, although the depths (e.g., cm below soil surface) needs to be added in the supplementary material, rather than using a 'depth number'. Five replicates for each treatment combination were conducted, but this need to be visualized by statistics in the figures.

Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2018-398/bg-2018-398-RC1-supplement.pdf>

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-398>, 2018.

Printer-friendly version

Discussion paper

