

Interactive comment on “Biological weathering and its consequences at different spatial levels – from nanoscale to global scale” by Roger D. Finlay et al.

Anonymous Referee #3

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General comments:

Finley and coauthors provide here an interesting and timely review on biological weathering across scales. It is well written and meets current questions and gaps of knowledge in this field. The general organization of the manuscript might on the other hand be significantly improved. I do not doubt however that some restructuring will enable this discussion paper to reach a wide audience and the large impact it deserves.

We are grateful for these positive comments and agree that some re-structuring will improve the paper

I would first like to acknowledge the fact that covering such a wide topic is challenging, and I would like to congratulate the authors for their effort to try to bring together various aspects of the study of biological weathering in one single review paper. In that respect, I found the general organization according to spatial scales very attractive in the first place. The resulting sections, however, lack of focus, while the last sections do not seem to follow this original plan (e.g. section 6 on insights from stable isotope methods). As a result, the reader might get easily lost or distracted by some of the digressions.

From the introduction (section 1) we review processes and experimental analyses at successively larger spatial and temporal scales (sections 2,3,4 & 5). Thereafter we discuss new possible methodological approaches using stable isotopes (section 6) and modelling (section 7) before presenting some concluding remarks (section 8) and finally (section 9) outlining some key questions, knowledge gaps and suggested future approaches. We think this structure is logical and will now explain it at the beginning of the revised manuscript so there is less likelihood of readers getting lost. Section 9 will also be expanded.

I think that the richness of ideas and concepts gathered here is a real originality of this review, but the author may want to be careful that the reader keeps track of the point that they are trying to make in a given paragraph. Section 5, which gathers a main section introducing concepts as diverse as “mineral evolution”, the geological carbon cycle or plants as holobionts and another subsection on carbon allocation and sequestration including carbon cycle and geoengineering concepts is for instance a little hard to digest.

We agree that that the review includes ideas from diverse disciplines (and also that this contributes to the originality of the review) but we also accept that these ideas can be introduced in a way that makes them more “digestible” and we have tried to do that in the revised manuscript.

To improve this point, I could first suggest gathering the different processes and links existing between them in a dedicated introductory section to make sure all readers are on the same page before tackling more detailed aspects of each scale. For instance, the relevance of allusions to long-term sequestration of carbon (e.g. lines 7-9 p. 8 and lines 24-26 p. 6) for the general topic of the paper might be unclear to some readers until they reach section 5.1. Another example is the

geological cycle of carbon, the presentation of which is scattered across section 5 and somewhat redundant (e.g. p. 11 and 14). An introductory section could also enable to present the order of magnitude of the different processes and elemental fluxes to be considered here (e.g. typical elemental flux derived from primary mineral weathering vs. typical plant uptake and potential export related to forestry practices vs. typical atmospheric input for a given type of system) which is something missing here. Second, I would recommend organizing sections into subsections to keep the reader oriented. I would also avoid sections including a sort of single small subsection, e.g. 5->5.1->6 or 7->7.1->8.

We agree with these helpful suggestions and will re-write the introduction to describe the structure of the article and introduce the key concepts to be discussed. We will change the section divisions to make them more consistent between chapters and try avoid single small subsections.

Another general point is that I find that the manuscript is lacking a few but quite important references. I try to provide a couple of them in the specific comments section below, which I hope the authors will find helpful. Aside from those points, I am enthusiastic about this interesting manuscript and I would recommend its publication provided that a couple of modifications and restructuring are done.

We are pleased this reviewer recommends publication, welcome the suggestions concerning re-structuring and will try to follow the helpful advice to make the article clearer.

Specific comments:

-Section 2: Alt and Mata (2000), Benzerara et al. (2007), Furnes et al. (2001) and Torsvik et al. (1998) are additional references on the biotic origin of tubular structures that the authors might find useful to include. l.12 p.4: the effect of turgor pressure on biomineral weathering is also discussed by Li et al. (2016)

Thanks for these helpful suggestions

-Section 3: Maybe the first paragraph might be strengthened by adding a couple of references when presenting common biofilm features to guide the reader, especially if some studies are relating these biofilm properties (e.g. retention of water) to mineral weathering (e.g. fluid-mineral contact time). In the second paragraph, Barker et al. (1998) is probably another classical reference on biofilms and microenvironments that might be added. In the last paragraph dealing with the interplay between bacteria and mineral weathering should be strengthened in my opinion. Some recent references including Mitchell et al. (2013), Montross et al. (2013), Wild et al. (2018) and Wild et al. (2019) are missing here and should be included at this point I think. l.19 p.6: "Burial" is referred to as "incubation" in Uroz et al. (2012). I would recommend sticking to this latter term. l.23 p.6: I am not completely sure of the relevance of the position of the last sentence (l. 23-26). I would move it upward or delete it.

Thanks for these helpful suggestions

-Section 4: l.29 p.6: the statements of the production of acidifying substances (H⁺, organic acids) and ligands that complex with metals in the minerals may need to be supported by quotations. l.30 p.6: "that retard weathering rates" reduce or decrease weathering rates would be more accurate. l.7 p.7: "uptake of positively charged nutrients such as NH₄⁺ and K⁺, result in exudation of protons" may benefit from the support of a quotation. **OK**

-Section 5: This section is a little bit dense, I would suggest dividing it into subsections. C3

Agreed – we will use sub-section titles to improve readability.

-Section 6: This section is thematic, not intrinsically associated to a given scale. Also, I am questioning the scientific relevance of specifically distinguishing studies from the QWARTS project from other studies.

Hopefully the re-structuring of the introduction will improve the readability and make it easier to understand our approach. Theoretically stable isotope measurements could be discussed within each scale section but we thought it was easier to group these studies together – especially as we introduced new results from a hitherto unpublished study. The fact that these results are so far un-published is one reason for distinguishing them and the idea behind the special issue was to highlight the recent research done within this interdisciplinary project but we can remove the reference to the project if the Editor thinks this is more appropriate.

-Section 7: Direct in situ measurements using gravimetric approaches by Augusto et al. (2000) or Turpault et al. (2009) or interferometry methods by Wild et al. (2019) are not reported by Akselsson et al. (2019) but might be worth mentioning since they directly meet some of the challenges implicitly pointed out in this manuscript regarding the validation of weathering models and the transposition/upscaling of laboratory mesocosms to field systems. In the second paragraph, I find the description of the influence of the different processes on the dissolution rate a little bit unclear, and I feel that the clarity of this section might be improved. Otherwise, readers who are not familiar with that type of models will be easily lost. I would suggest reorganizing this section and starting by presenting the different parameters controlling the dissolution rate (temperature, pH, chemical affinity, . . .) and then, in a second step, describing the influence of plant metabolism on these factors and thus on the dissolution rate. I would also strongly recommend using an equation (e.g. developed from equation 3 in Erlandsson et al. (2016), equation 3 in Godderis et al. (2006) or equation 1 in Palandri and Kharaka (2004)) to visually support this discussion. I would also avoid mentioning the concepts of “weathering brakes” or “transition state theory” if they are not explained. This might be more confusing than useful for readers, depending on their background.

We thank the reviewer for these detailed, helpful suggestions and will do our best to incorporate them in our revised manuscript. We aim to shorten section 7 substantially and to re-structure and simplify it so that it is more directly relevant to the weathering processes described in the rest of the manuscript.

Thank you for these additional references

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