Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-41-RC3, 2019

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

Received and published: 29 April 2019

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.

I would first like to acknowledge the fact that covering such a wide topic is chellenging, 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.

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.

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.

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.

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)

-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.

-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.

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

-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.

-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.

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