

Interactive comment on “The origin and role of biological rock crusts in rocky desert weathering” by Nimrod Wieler et al.

Anonymous Referee #2

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In the manuscript "The origin and role of biological rock crust in rocky desert weathering" by Wieler et al., the authors set out to characterize the microbial communities associated with rock crusts on limestone and dolomite host rocks sampled from arid regions. In this aspect they have succeeded. The authors also claim to have discovered how crust-associated microbial communities influence the mediation of weathering processes associated with these clasts. With respect to this second claim, the authors have only shown here that EPS associated with the microbial communities helps the rock surface to retain water, not that the water retention mitigates the weathering process via slowing crystal growth (as is claimed). The finding of EPS retaining water has been shown before in other environments (refs below), but in those studies, the retention of water was proposed to enhance weathering via various mechanisms, not

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retard it. Perhaps if experiments showed that rock weathering decreased under EPS-free portions of the rock surface, I would find this second claim convincing, but these experiments/data are not present. I suggest that the authors rework the manuscript to focus only on the characterization of the community, and not on biogenicity aspects of the crust formation that are not supported by the research findings. In addition, I have the following concerns:

- The authors frequently misspell words that should contain the letter “z” but instead are spelled with an “s” (stabilise vs stabilize; colonise vs. colonize etc.). Perhaps this is a US vs British spelling difference, but the journal editors may want to clarify which style they want used.
- Page 2, Line 14, This sentence could be rewritten for clarity.
- Page 5 Line 12, please provide the number of samples that contain weathering features.
- Page 5 Line 38, I disagree with the authors’ use of the terms “biogenic” to refer to the rock crust. Let’s assume that the ^{13}C depleted values results from the liberation of carbon from photosynthetic materials via respiration (there are other ways to get ^{13}C -depleted carbonate, but let’s just assume the mechanism the authors invoke is correct), that CO_2 should then be creating an acidic environment that does not necessarily favor carbonate formation. More importantly, a carbon contribution from respiration recorded in a carbonate does not make a rock crust any more “biogenic” than any carbonate that forms in any environment in which CO_2 is sourced from respiration, which could be any environment! I strongly recommend that the authors remove biogenic from these paragraphs, as the carbonate carbon isotope data do not demonstrate that living processes were necessary (or even important) for the carbonate crust formation.
- Page 6, Line 28, please provide the full citation information for the Jiang paper.
- Page 6, Line 29, the spatial correlation of a biofilm with a mineral precipitate DOES

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NOT establish that the biofilm was involved in the formation of the mineral formation. As an example, the modern day La Brea tar pits contain abundant bacteria and archaea, that does not mean that those bacteria and archaea are responsible in any way for the presence of the tar of the fossils embedded in the tar, despite their spatial correlation. The same is true for our teeth, or for certain modern stromatolites. I'm certainly not saying that there aren't many cases where microbes are involved in mineral precipitation, there clearly are many, included microbes involved in carbonate formation, but in cases like this, it can be difficult to demonstrate this relationship and we should be careful with our words and our claims.

- Page 7, Line 5, the description of the observed vs. predicted phylotypes (predicted by Chao/ACE) is unclear and should be better described.

- Page 8, "specialism" should be "specialization"

- Page 9, Line 9 says "were was", but should be just "was"

- Page 9, Line 17 – this sentence could be rewritten for clarity

- Page 9, Line 18 – This sentence should probably be ended with "respectively)" to indicate which percentages with each parameter, or better yet rewrite the entire paragraph and give a sentence to each parameter.

- Figure 4b – the dust samples are hard to differentiate using the current color.

- I understand that the rock crusts studied here are not the same as the manganese oxidize-rich rock varnish that has been extensively studied elsewhere, but are the microbial communities similar or different? Would this be worth mentioning as a point of comparison? Some readers will be more familiar with those features.

- The authors propose that the microbial community should be similar to that of the surrounding soil, or incoming dust, if those are the sources, but then demonstrate with their amplicon results, that the communities on the rocks are substantially different from those in the soil and dust. This is an interesting result and worthy of publication for its

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own sake in my view. I think the authors do a nice job with this part of the paper and should be commended.

- The presented results do appear to show that the biofilm contributes to the retention of water at the rock surface. However, this is not a new claim and there are numerous other papers in the older literature that also show this (Potts, M. (1999) Mechanisms of desiccation tolerance in cyanobacteria. *Eur J Phycol* 34: 319–328.; Decho, A.W. (2000) Exopolymer microdomains as a structuring agent for heterogeneity within microbial biofilms. In *Microbial Sediments*. Riding, R.E., and Awramik, S.M. (eds). Heidelberg, Germany: Springer, pp. 9–15. Here, the authors propose that EPS limits salt mobilization and crystallization at the surface. Indeed, other rock weathering studies invoke the water retention capabilities of EPS as a way of maintaining acids and chelating agents in contact with the weathering surface. I appreciate that this could be less relevant under arid conditions, but again, the authors should explicitly say this and test their hypothesis that the water retention retards weathering experimentally.

- Table 1: This table doesn't seem like essential information and I suggest that the authors might instead place it in the Supplemental Information.

- The amplicon results figure in the supplement (Figure S2) is well done, and shows all of the data in a presentable manner. Personally, I would like to see this as a figure in the main body of the manuscript, rather than as a supplemental figure. Perhaps its position (supplement vs main) could be swapped for the current Figure 4 panels C and D?

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-442>, 2018.

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