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

Nimrod Wieler et al.

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Dear Editor,

Please find attached a revised version of the manuscript, titled "The origin and role of biological rock crusts in rocky desert weathering". I thank you and the anonymous reviewer for the constructive comments and appreciate the time you have invested in improving this manuscript. The changes to the manuscript include text changes that address the points raised by the reviewer. Thank you for the efficient review process. I will be glad to answer any further questions.

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Discussion paper

Sincerely,

Roey Angel on behalf of all Co-Authors

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Discussion paper

Comments by reviewer:

R: 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 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 ïňĄndings.

A: Thank you for comment, we appreciate this feedback and have addressed all the specific comments below. We specifically addressed the issue of the influence of biological crusts on rock weathering processes in detail below.

R: 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.

A: Indeed, these words are spelled with “s” under the British spelling system. We

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Discussion paper

followed the British spelling system throughout the manuscript as is customary for European journals.

R: Page 2, Line 14, This sentence could be rewritten for clarity.

A: The sentence was rewritten in page 2 lines 12-14: “Recently, Bruthans and colleagues (2018) conclusively demonstrated that in temperate climate moisture flux followed by salt crystallisation at the boundary layer govern the case hardening model.”

R: Page 5 Line 12, please provide the number of samples that contain weathering features.

A: Ten rock samples, from each lithology, were studied and characterized. The number of samples is mentioned in page 5 lines 21-23: “To study the possible differences between these sites, we performed geological characterisation of 10 limestone and dolomite rocks collected from the arid and hyperarid sites, respectively, testing for mineral content, porosity, permeability and elasticity”.

R: 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 13C depleted values results from the liberation of carbon from photosynthetic materials via respiration (there are other ways to get 13C depleted carbonate, but let’s just assume the mechanism the authors invoke is correct), that CO2 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 CO2 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.

A: We did not measure carbonate rather we measured the isotopic signature of the

all carbon form together using pyroslysis. The negative values found in the crust layer indicate a mixture of marine carbonate sedimentation (with d13C values close to 0 per mill) with freshly photosynthesised (not respired!) carbon (which typically has a d13C values of -20 – -30 per mill for photosynthetic microorganisms). Since no other terrestrial process, but photosynthesis, is known to generate such low 13C values such values are considered a very reliable signature of biological carbon fixation. The term "biogenic" was therefore used in this context to indicate the contribution of a biological process to the carbon pool and to differentiate it from the parent rock material, which had a clear abiotic signature of marine carbonates. We further do not suggest that the crust was formed only by direct precipitation of microbial activity but it is a mixture of both organic and inorganic materials that binds together. This is in fact what defines biological soil and rock crusts.

R: Page 6, Line 28, please provide the full citation information for the Jiang paper

A: The citation was corrected in the text in page 6 line 28: Jiang et al., 2004, and in the reference list-"Jiang, W., Saxena, A., Song, B., Ward, B. B., Beveridge, T. J. and Myneni, S. C. B.: Elucidation of Functional Groups on Gram-Positive and Gram-Negative Bacterial Surfaces Using Infrared Spectroscopy, Langmuir, 20, 11433-11442, 2004".

R: Page 6, Line 29, the spatial correlation of a bioïňĄlm with a mineral precipitate DOES NOT establish that the bioïňĄlm 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 difiňĄcult to demonstrate this relationship and we should be careful with our

words and our claims.

A: We suggest that the laminated rock crust results from microbial activity. These laminated fabrics resembled to similar fabrics in marine stromatolites. Such fabrics in marine environments were linked to diverse sediment-microbes interactions and included diverse microbial communities (Bosak, Liang, Sim, & Petroff, 2009; Dupraz et al., 2009).The mediation of microbial activity in the crust formation, in our study, is mainly suggested as a binding agent to form thin coatings on the rock surface. We support this statement with the presence of the EPS and the isotope measurements that were found in the rock crust and were absent in the host rock. We further do not suggest that the crust was formed only by direct precipitation of microbial activity but it is a mixture of both organic and inorganic materials that binds together.

R: Page7, Line 5, the description of the observed vs. predicted phylotypes (predicted by Chao/ACE) is unclear and should be better described.

A: The text was corrected in page 7 line 5: "The communities of the BRC showed an average of 182 observed, 354 predicted bacterial phylotypes, and Shannon's H was 3.8 (Fig. 4A; Table S2), for arid limestone and 129 observed, 315 predicted phylotypes and Shannon's H was 3.3 for hyperarid dolomite, with no significant difference between the rock types."

R: Page 8, "specialism" should be "specialization"

A: The text was corrected in page 8 line 32: "The BRC communities also differed from their surrounding soil and dust, indicative of the specialization of the colonising taxa to rock environment."

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

A: The text was corrected in page 7 line 9: "The diversity of the dust samples was as poor as the BRC's (169 and 107 observed and predicted OTUs and Shannon's H = 3.0 and 1.5, on average) and did not differ between sites (Fig. 4A; Table S2)."

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

Page9, Line18–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.

A: The text was corrected in page 7 line 17-18: "Beta-diversity analysis, using variance partitioning, showed statistically significant differences between samples on the OTU-level based on climate, sample type (i.e. rock, soil or dust), and to a small extent also via their interaction. These variables were found to significantly contribute to the differences in bacterial communities accounting for 22%, 40% and 3.8% of the total variance, respectively (Fig. 4B, Table S3)."

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

A: We increased the colour intensity of the dust samples for better clarity.

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.

A: A comparison between the microbial communities of the manganese oxidize rock varnish and the ones in the rock crusts mentioned briefly in our work in page 2 lines 26-27, page 7 lines-13,16,26 and page 8 line 3. The comparison refers to the work conducted by Lang Yona et al., 2018.

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 own sake in my view. I think the authors do a nice

job with this part of the paper and should be commended.

A: Thank you, we appreciate this feedback.

The presented results do appear to show that the bioïňĄlm 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 bioïňĄlms. 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 crystalization 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.

A: The current knowledge dealing with retention water by EPS, as mentioned, has limited data on the role they might play in correlation with arid rock morphologies. Cavernous weathering features result from three initiation conditions: porous media, salt solution and hydration-desiccation cycles (Scherer, 1999, 2000). We followed the current knowledge on EPS, found in the rock crust, to test how its water retention abilities, may correlate to arid rock morphology. To do so, we ran a desiccation experiment, discussed in the manuscript. This experiment showed that rock crusts, containing EPS, retard water at the rock surface. Resulting from this experiment we note that the rock crust containing EPS limits the water needed for generating salt solution at the rock interface, and limits rock weathering over small spatial scales .

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

A: The table was moved to the supplementary

The amplicon results ïňĄgure 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 ïňĄgure in the main body of the manuscript, rather than as a supplemental ïňĄgure. Perhaps its position (supplement vs main) could be swapped for the current Figure 4 panels C and D?

A: We now include Figure S2 as part of the main text. However we kept panels C and D in Figure 4 because they illustrate the real proportions of various taxa rather than a difference in a binary comparison