

Interactive comment on “Novel water source for endolithic life in the hyperarid core of the Atacama Desert” by J. Wierzchos et al.

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We thank Prof. Dr. Emmanuelle J. Javaux for handling our manuscript entitled: Novel water source for endolithic life in the hyperarid core of the Atacama Desert, as well as anonymous Referee#2 for her/his valuable and helpful comments and suggestions.

Below we specifically address the questions posed by this Referee. The changes made in manuscript revision are also detailed and described, as we certainly believe that they have enhanced the quality of the paper, thus making it suitable for consideration for publication in BG. All and every Referee's original comments are provided, followed by a detailed response.

“I cannot pronounce myself about the possibility that the nanoporous phases observed

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in LT-SEM and ESEM are (or not) the consequence of experimental artifacts.” The issue of potential experimental artefacts was already commented and discussed in “Referee #1 response” and much more attention have been paid to this issue in the new version of the manuscript.

...”even if the probes are tightly adapted to the holes drilled, they may not be recording exactly the same values as would occur in nano- or micro-porous halite areas”... This issue is fully explained in the Supplementary Information section. The reading volume of the RH/T sensor inside the halite pinnacles was calculated as 0.36 cm³, which corresponds to the volume of frequently-occurring large pores or cavities in natural halite samples

“I find the observations that water condense and remains liquid forming brines for long periods very interesting. However, one can only regret the absence of replicate measurements in a second nearby halite pinnacle as control. The conclusions would have been much stronger. Indeed, if these observations are confirmed in a more statistical way, they would imply that these endolithic microorganisms are wonderfully adapted to recurrent drying up and rehydration, and to develop their metabolism whenever water is available (and at very low water activity due to the high salt concentration).” Referee #2 makes a good point regarding the importance of statistical analyses. We note, however, that our results confirm and expand the results contributed by Davila et al. (2008), who reported the occurrence of deliquescence events inside a different halite rock in the same Yungay area. Our new results show that the onset of deliquescence occurs earlier than expected, and that wet stages are much longer than previously reported. This is due to the use of smaller RH and EC sensors. Hence, we believe that our results do have statistical significance. Had not we observed any deliquescence event in the course of one year in our new study, one could claim lack of robustness in our conclusions, yet the fact that our results not only confirm but improve previous observations might be taken as a robust indicator of water dynamics in the halite, and the adaptation of the endolithic organisms to wetting-drying events and high salt con-

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centration. Of course, the absolute duration of wet conditions might change between rocks and from year to year (i.e., due to el Niño-related events), but these variations are expected to be statistically insignificant over long time-scales. In our opinion, it is also worth noting that our measurements collected 158,031 samples of data, what we believe statistically significant.

“Another question that remains open is what happens below the colonization zone. Is water condensing there as well? If so, why do heterotrophic microorganisms colonize that region as well? Is it only because they associate tightly to the photosynthetic cyanobacteria? How does organic matter distribute within the pinnacles? If the zone below the colonized area does not induce water condensation, why is it so?” Our field observations indicate that the distribution of cyanobacteria inside the halite is controlled, at least in part, by light penetration. Typically-endolithic phototrophs inhabit a portion of the rock that receives sufficient light but below stressful levels (i.e., too much light inhibits photosynthesis). The penetration of endolithic colonies into the halite has been commonly observed to be controlled by the presence of impurities (i.e., layers of clay and sand grains) that shield the PAR. Hence, deliquescence is expected to occur below these layers, yet cyanobacteria colonization is not observed. In very clean halite pinnacles, endolithic colonization throughout the rock has been observed (data not shown), thus suggesting that deliquescence is not limited to the outer layers of the rock. The occurrence of deliquescence below the colonization zone is indirectly inferred from the presence of large pores and small dissolution voids. However, we dispose of no sensor data to prove that point. We have only monitored the distribution of heterotrophic microorganisms associated to the cyanobacteria, but we expect heterotrophs to also appear in deeper parts of the halite, even in the absence of phototrophs. This issue will be the focus of future research aimed at determining microorganism diversity in the halite rock. Regarding the issue of organic matter distribution within the pinnacles, we thank the reviewer for his insight. Nevertheless, while this issue might be very interesting, we feel that such an analysis would fall outside of the scope of the present study.

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Minor comments:

“Page 3073, lines 8-9. "Soils here are the most abiotic on Earth". Abiotic means without life, so soils here as everywhere else are either biotic or abiotic, but they cannot be little or very abiotic.” The word abiotic has been replaced by sentences that convey the idea that soils in the Atacama Desert are one of the most extreme environments for life on Earth.

“Page 3073, line 28: Halotheca genus (not genera).” The term 'genera' has been replaced by 'genus'

“Page 3076, line 18: imaging instead of imagining.” The term 'imaging' has also been corrected.

Interactive comment on Biogeosciences Discuss., 9, 3071, 2012.

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