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Discussions

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

Anonymous Referee #1

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At first, I would like to inform the editor that I am a geologist skilled in rock salt microstructure and not a biologist or a biogeologist. Therefore, I have no competences to discuss the “bio-implications” of this paper.

This contribution presents results both about microclimate data acquired at the direct vicinity and within the halite pinnacles from the Atacama Desert and microstructural investigations of wet halite grain boundaries by using low-temperature-SEM (LTSEM) and environmental SEM (ESEM) methods. Interpretations of the results point to “a newly identified [nano-porous] halite phase that is intimately associated with the endolithic aggregates”. Therefore the authors suggest that the nano-porous halite phase could enhance the condensation and the retention of liquid water by capillary process needed for endolithic life.

In general, the paper is in good form and well detailed. The implication of the research sounds highly important for publication. In details, the impact of the contribution is only depending on the presence of the nano-porous halite phase at grain boundaries. As far as I am concerned, I find the interpretation of the nano-porous halite phase really controversial.

Please find below my argumentation:

(I) The authors used LTSEM to describe grain boundaries in its “natural” hydrated state and found, after cryo-fracturation followed by freeze-drying, micro- and nano- phases composed of halite. The walls of the halite micro-phase at grain boundary are smooth. They notice that Desbois et al. (2012, 2008) and Schenk et al. (2006) have interpreted similar structures as sublimated brine remnant after freezing followed by sublimation (i.e. the same procedure used in this contribution) but without longer considerations about how the resulting foam/sponge structure (called here micro- and nano- phases of halite by the authors) can develop. By omission of such considerations, the authors suggest that these micro- and nano- porous phases are naturally present, which I think is definitely not provable by LTSEM and highly suspicious considering the following. Because of the rapid cooling by plunge freezing of the staring material (NaCl grains + grain boundary brine) into liquid nitrogen (-196°C), the saturated brine at grain boundary is transformed into ice (H2O) and hydrohalite (NaCl + 2 H2O) (Roedder, 1994; Bodnar, 1993). Subsequently to the sublimation (i.e. freeze drying) of the sample, the brine is clearly identifiable by its typical foam/sponge pattern (Schenk and Urai, 2006; Blackford, 2007; Blackford et al., 2007; Desbois et al., 2008, 2011a, 2012). The foam/sponge pattern is reflecting the evaporation of ice crystals leaving behind voids in hydrohalite foam. Thus, in respect to the above explanations, LTSEM as used by the authors cannot be used to define the structure of halite within the grain boundaries but only provide indication of earlier presence of brine in the grain boundaries. More over, my most recent investigations of wet grain boundaries in salt (paper currently in review) by using LTSEM show that macro-pores identified by the authors are indeed

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the imprint of gas inclusions embedded in brine.

(II) ESEM was used to confirm the presence of porous halite phase at grain boundaries but does not sound convincing because gives more doubts than certitudes. I am not telling that this nano porous phase seen by ESEM does not exist but a robust discussion is needed since it is the key point of the paper. Authors never discussed how such a porous phase network can develop in grain boundaries and the possible artifacts due to the preparation of sample are also never discussed.

Please find below some remarks:

a. It seems that the authors try to make an ambiguous parallel between porous phases at grain boundaries seen by LTSEM and ESEM. LTSEM cannot be considered as a relevant method for detecting such a porous phase since if it really exists, it has the same appearance than sublimated frozen brine (see above). Did you observe macropores with smooth walls under ESEM?

b. Observation points to the presence of nano-porous phases in samples, which have been dried. Therefore, which is the effect of drying of brine in pore in the development of precipitate/recrystallization/efflorescence artifacts? But it is also mentioned that ESEM are performed at the constant temperature of 5°C at partial pressure ranging from 0.13 and 8.75 mbar. Which could be the effect of change of temperature and pressure on the state of residual brine?

c. Why is it necessary to oven-dry the sample? Can they just break the natural sample directly inside the ESEM without pre-drying?

d. Are these nano porous phases frequent under ESEM?

e. The authors showed the deliquescence of the nano-porous phase with increasing RH (figure 6), but what happens if the RH is decreasing? Will the nano-porous phase re-condensate? If not: how could you explain the presence of such a preserved nano-porous phase after several natural RH cycles during the lifetime of a halite pinnacle?

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(IV) I have maybe a naïve question about the state of water needed for endolithic brine: are we speaking about pure water or brine (or salty water)? If the bacteria needs really pure water, I do not understand the relevance of the process suggested by the authors since the deliquescence of halite-made nano-porous phase results in brine. In the case that brine can be sufficient for endolithic life, I am wondering if the story about the water source should be less complicated just by considering the LTSEM experiment which conclude (see (I)) that brine is present everywhere in large amount at grain boundaries (indicated by foam pattern) independently of RH and the presence of a nano-porous phase. Gas inclusions (interpreted as macro-pore by the authors) embedded in brine indicate that the grain boundary reservoir is very tight (e.g. because of grain the boundary healing process, Desbois et al., 2012; Van Noort et al., 2008; Ghossoub and Leroy, 2001).

I recommend then the publication of the contribution in Biogeosciences because of the originality of the approaches and the importance of the topic but with major revisions which have to include much more detailed and critical discussion about the reality of the nano-porous phase in grain boundaries, the key point of the paper.

Additionnal references cited in the review:

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