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Biogeosciences Discussions

Interactive comment on "The composition of endolithic communities in gypcrete is determined by the specific microhabitat architecture" *by* María Cristina Casero et al.

Anonymous Referee #2

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Comments on: "The composition of endolithic communities in gypcrete is determined by the specific microhabitat architecture" by Casero, Meslier, DiRuggiero, Quesada, Ascaso, Kowaluk, & Wierzchos

General comments: In this paper the authors investigated the microbial associations present in three distinct microhabitats (cryptoendolithic, chasmoendolithic, and hypoendolithic) in samples of gypcrete from the polyextreme Atacama Desert using molecular techniques. They further try to tie differences in the associations to the architecture of the microhabitats. Although the fact that these three microhabitats harbor different associations Is not surprising, the work described is solid example of the use

of a multidisciplinary approach to the problem. The molecular sampling of different microhabitats from the same stone is also novel. Therefore, I am in favor of publishing the paper with only a few minor revisions. My only major concern is that they haven't really succeeded in separating the influence of the architecture of the microhabitat from the myriad of other environmental variables (light, moisture, temperature, chemistry, etc.) that may be influencing the development of the association.

Specific comments:

1. I am a little concerned about the reliance on molecular methods to characterize the associations. I understand such methods are necessary given the nature of the problem, but they sometimes overlook obvious features. In this particular case, the authors describe two differently pigmented layers in the cryptoendolithic habitat. The cause of this difference is not addressed. However, in Wierzchos et al. (2015), gypsum samples collected from a spot only a few miles away showed a similar pattern in the cryptoendolithic habitat. In this case the upper, orange-pigmented layer was dominated by eukaryotic algae. These do not appear in the present analysis. Are they absent? Or are they not picked up by the molecular methods used? There is also no discussion of whether the orange and green layers mentioned in the present paper represent different morphologies of the same association or different associations in the same microhabitat.

2. In the discussion on page 11 it is suggested that the water relations in the three microhabitats differ as a result of the architecture. A little elaboration here might help with the argument that architecture determines the association.

3. Have the authors considered the proposal by Friedmann and Sun concerning the relative proportions of mycobionts and phycobionts in lichens in response to temperature (Microbial Ecology 49:523-535) in the in relation to the authors hypothesis concerning the relative proportions of phototrophs and heterotrophs in extreme environments?

Technical comments:

1. Does the infrared camera used to measure surface temperature need to be calibrated to gypcrete in order to get an accurate temperature? Most systems need to take the emissivity of the surface material into account first.

2. I am not clear concerning the numbers of samples and replicates. It looks like three rocks were used. Two of these contained cryptoendoliths, three contained hypoendoliths, and all contained chasmoendoliths, and more than one chasmoendolithic association was sampled for each rock. Does this give enough statistical power for the analysis?

3. The CT scans by themselves are difficult to interpret (Figure 2).

4. I did not see any discussion of UAM811, which seems to hold a somewhat anomalous position in maximum likelihood tree (Figure 5).

Minor issues: 1. Azuá-Bustos et al. 2015 is missing from the references. Replaced by Azuá-Bustos & González-Silva 2014?

2. Wierzchos et al 2012a and Wierzchos 2012b need to be differentiated in the references.

3. Cockell is misspelled in line 61

4. I prefer to put genus and phyla ahead of the names: “genus Chroococcidiopsis” instead of “Chroococcidiopsis genus”.

5.Change “the limit established by Nienow (2009)” to “the established limit (Nienow 2009)” (line 264)âĂŤNienow cited the limits but they were established previously.

6. Camara et al 2015 should be Camara et al 2014 (line 270)

7. Changes “consolidates” to “supports” (line 280)

8. instead of “unidentification” might be better to say “inability to identify.” (line342)

9. Pointing references are run together. (line 485)

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