

## ***Interactive comment on “Microbial colonization of chasmoendolithic habitats in the hyper-arid zone of the Atacama Desert” by J. DiRuggiero et al.***

### **Anonymous Referee #2**

Received and published: 2 March 2013

My general impression is that this is a worthy study. Although the communities are not significantly different from those previously reported from this region (for this reason I suggest taking out phrases “for the first time”, “novel”, “cutting edge”), it does provide valuable, new data in climate and sequencing. However, I would like to see more understanding of the data than presented. For example, are there things that we know now from high throughput sequencing which we did not know before from conventional clone libraries? How this site compare to the previously studied sites in terms of climate characteristics? The paper set out to address the question of what factors determine species compositions, but did not answer it or provide new insights.

Specific comments:

Page 15604 line 23. You characterize cryptoendolithic and chaesmolithic as “indirectly

C8708

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive  
Comment

connected” and “connected to the rock surface”, respectively. I understand it, but not sure if everyone would. According to my understanding, the difference is whether or not the colonized pore spaces are covered by a surface crust (hence the prefix crypto-,) or the difference can be characterized as whether or not pore spaces are directly open to the outside environment.

Page 15605 line 2. I would suggest dropping “hypoendolithic”. I know that this was published, but I am not convinced. Small stone flip all the time as a result of perturbation by animals or humans. I don’t see how you can rule that out.

Page 15609 line 7 “the occurrence of rare precipitations, a lack of fog, and the absence of moisture (dewfall) on rock surfaces, except during rainfall events.” This needs rephrasing. I read this as saying “fogs and dewfalls occur only during rainfall events”.

Page 15609 line 9. potential evapotranspiration or PET is a good gauge for higher plants, as plants have to stay hydrated all the time and deal with evapotranspiration. Desert cyanobacteria are poikilohydric and don’t have to, so PET is not a useful indicator of their stress. I still prefer Imre Friedmann’s concept of total wet duration to be useful and would suggest using that instead of PET.

Page 15609 line 20. I find the presentation of climate data inadequate. The annual average RH does not tell me when the organisms become active and for how long. Why not display the data the traditional way, like McKay et al. *Astrobiology* 2003 3:393-406. It would be much more meaningful.

Page 15610 line 13. These organisms live under gypsum or calcite crusts that happen to form on rhyolite. The physical and chemical properties of the crusts are relevant, but I don’t see the need for the underlying rhyolite to be described in detail. Elsewhere in the paper, descriptions should also be revised so that the reader understands that these communities are associated with the translucent crust, not living within the opaque rhyolite.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Page 15617 line 13 The difference in colonization depth between the two specimens is attributed to water filled cracks serving as optical fibers. This is a puzzling (I know this is based on a published paper, but published papers can be wrong). Why don't attribute the greater depth to calcite crystals transmitting light?

Page 15617 line 22. Sorry, but I don't get how substrate solubility affects the species composition of the community. If an otherwise suitable substrate is not colonized, you can speculate whether or not it is because it dissolves too fast. If the rock is colonized, then it is stable enough. Who gets to live in it therefore has nothing to do with solubility. Your calculation of the age of the calcite coating, 12,000 years, supports my point.

Page 15618 line 4. you interpret the micro scale weathering features in the calcite sample as being due to dewfalls and wet dry cycles. This seems to contradict your climate data where you say dewfalls are rare, except in days following a rainfall. More importantly, why can't be rain that caused these weathering features?

Page 15620 line 23. "Rock mineralogy as a nutrient source". This is contrary to the general understanding that the rock provides a physical home for the organisms, while essential nutrients come from dissolved minerals in rain water or in some cases from the biological weathering of the rock as well.

Page 15621 line 5-12. The statements made in the last paragraph of the discussion are not supported by the data presented.

Figure 3 is not necessary, in my opinion. I did not learn anything that I did already know from Figure 2.

I would turn Figure 4 into a table. The point here is to compare and contrast, something is better suited for a table than a bar graph. I believe interesting details are lost because of the presentation.

I also suggest that the climate data be moved from supplemental to the manuscript, but presented in the traditional way.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

---

Interactive comment on Biogeosciences Discuss., 9, 15603, 2012.

**BGD**

9, C8708–C8711, 2013

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C8711

