Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-369-RC3, 2017

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Eco-physiological characterization of early successional biological soil crusts in heavily human impacted areas – Implications for conservation and succession" *by* Michelle Szyja et al.

Anonymous Referee #3

Received and published: 8 November 2017

Comments to the Authors

The paper of Szyja et al. aims to characterize ecophysiologically early successional biological soil crusts in heavily human impacted areas. For achieving this they choose two locations with a different type of BSC: one dominated by a cyanobacteria and the other by a green alga. Overall, I found the paper representing an interesting contribution to scientific knowledge of BSC ecophysiology because: 1- there are at present not many data available about ecophysiology performance of these type of BSCs and 2- The comparison of the response between bare soil, intact BSC and isolated compo-

Printer-friendly version

Discussion paper

nent is novel and very interesting. Nevertheless, I found some important problems as how the work is presented. The main problems are in the methodology where the experimental design (mainly number of replicates in each experiment) is not clear and in the results, where some of the figures are quite confusing. The question about whether the NP rates should be expressed on a chlorophyll or surface basis is not relevant here and, obviously, will differ if comparisons are made between cyanobacteria and green algae. In my opinion the number of references (85) exceeds the needs of the paper. Beside some minor/typographic errors (i.e. check subscript in CO2 throughout the text), in general, the paper is well-structured, the discussion is good and conclusions clear but it needs to show results in a way that they appear more conclusive.

In conclusion, I find the paper interesting and scientifically sound but taking into account the amount of data and how they are presented I don't think it reach the standards of BG. I have some comments and suggestions that I think will improve the paper.

Major and minor comments

Title

- I suggest removing the second part of the title (implications for conservation and succession) as it does not reflect the content of the paper.

Abstract

- There is no reference in the abstract to one of the main points in the work that is the differences found between response of intact BSCs and of it isolated dominant components.

- Page 1. Line 20. I suggest to remove the sentence beginning "Nevertheless, a major. . ." See comment above.

Introduction

- Page 1. Line 29. Please rewrite the sentence "Investigations. . ." As it is now is

contradictory. Are there abundant or few investigations in cyanobacteria?

- Page 2. Lines 5 to 20. In my opinion the concept of arrested succession should be introduced at the beginning so it is clearer for the reader.

- Page 3. Line 9. Reference Reisser et al. 2007 is not in the list.

- Line 23. I suggest to change “or” for “and a”

- Lines 25-26. Were these “in situ” measurements carried out? I think it would be better say “would allow”

- Line 32. Colesie et al. 2014b not in the list. “Higher” than what?

- Page 4. Line 4. The sentence is confusing and I think is not relevant here. I assume that when authors refers to system they refer to BSC and not to the measurement systems. The treatment or position in the cuvette is another question. Of course there will be variability between samples, but here the comparison is between isolated individuals (green algae or cyanobacteria), soil biocrust and soil. I suggest removing this sentence. Material and methods

- Page 5. Line 2. Check reference Honegger 2008. Is 2003 and also it refers to green algae photobiont but not to cyanobacteria.

- Line 11. n=6. It is not clear to me how the sampling or subsampling was made. From each 6 of C-BSC and 4 of G-BSC you take 3 subsamples?

- Line 12. First, you need to indicate how the saturation light was determined.

- Line 16. Delete “from the”

- Line 19. Should not be a new paragraph.

- Line 21. I understand that the weighing was during the dehydration cycle to have the full response, but not between them. Please explain this.

- Line 23. I suggest new paragraph. “To obtain the net response to light. . .” n=3. Are

the samples BSC or species individuals? It is not clear from the text and in Fig. 2 they appear as individual species measurements.

- Line 25. How the optimal temperature was obtain? Are there any regressions done for this? Data is not show. Please explain.

- Line 29. Should not be a new paragraph.

- Page 6. Line 10. Include “of the two types of BSC” after “levels”. The analysis as it is explained is confusing as there were different number of samples and subsamples for the different experiments. For the drying curves there were 6 C-BSC and 4 G-BSC and from each of these all, dom and soil. But in the light curves there are only 3 C-BSC and 3 G-BSC without distinction of components. So, I understand that BSCall, BSCdom and BSCsoil cannot always be the explanatory variables. Results

- The adjustments of the curves in Fig. 2 doesn’t look very good, especially that of Z. ericetorum, showing an increase in the response and no saturation following the points and not the line. Please check this. Also, how where the light parameters (compensation and saturation) calculated, from individual adjusted curves or from one curve? It should be explain in material and methods. There are no supplementary tables or graphs showing values of these parameters.

- Page 6. Line 4. From Figs 4a and b they don’t contribute to NP response.

- Line 22. Here it is said G-BSCall and C-BSCall but not in Fig. 2. Please clarify. I suggest changing “almost twice as much” for “higher”

- Line 24. Why organisms? Is it not BSC? It is not reasonable that the difference in compensation point was twice as much but then there were no significant. As comment above please explain how this analysis was done.

- Line 26. The same discussion will apply for the saturation points. From Fig. 2 we can understand that there is no saturation at 2000 μmol (just a few lines before it is said that maximum NP rates were reached at 2000 μmol).

Printer-friendly version

Discussion paper

- Page 7. Line 2. Include “dominated BSC” after “commune”. Refer here to Supplementary tables.

- Line 6. Include “dominated BSC” after “ericetorum”. Refer here to Supplementary tables.

- Line 8. Delete “an”

- Water dependent photosynthetic response. In my opinion better than exemplifying graphs, average data of all replicates should be represented. Differences between just two samples are not relevant. Also, curves shown in Figures 4 are very difficult to understand as it is not normal the fluctuation around 80% water content. It must be an artifact that could be masked using averages. Also the water depression is not clear.

- Line 16. Change Table for Tables.

- Lines 26-28. Data shown in the text of ranges of optimal WC seem different from the ones in Fig. 5 (i.e. upper limit never coincident). Please check.

- Page 8. Line 5. I would rather delete this subsection as discussed above.

- Line 20. Table S6

Discussion

- Line 25. BSCs photosynthetic organisms

- Page 9. Line 5. Delete “none” and better G-BSCall and C-BSCall. What does it means physiological flexibility to water gain?

- Line 18. I suggest delete sentence beginning “A depression. . .” as it has already said before.

- Line 23. I don’t see the detection of a CCM from Fig.S2.

Page 11. Lines 2 and 7. Species name in italics.

Printer-friendly version

Discussion paper

-Lines 16-26. In my opinion this question is not relevant as it is obvious.

Conclusions

Page 12. Line 3. The authors conclude that there is a relative temperature independence of NP but the results show significant differences in the response of NP to temperature.

- Line 5. In general, the question about physiological plasticity should be avoid because there are no experiments proving this.

- Line 6. To incorporate the results into global scale carbon cycle models, the work should better provide numerical data sets (i.e. tables).

References

There are too many for the paper. As mentioned above some cited literature in the text is not in the list. Please check references through the list.

Table 1. Following my suggestion about Chlorophyll question then this should not be included.

Figure 2. Legend.The second sentence is not necessary, just n=3.

Figure 3. Legend. What do you mean by . . .of one of the group only? Please indicate what vertical bars represent.

Figure 4. See comment above. Indicate PAR

Figure 5. This graph is very difficult to understand. See comments above. What does the letters mean? Why n=24 here?

Figure 6. As suggested above I would not include this graph.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-369, 2017.