

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 #2

Received and published: 16 October 2017

Dear Editor, dear authors: Please find here my review concerning the paper “Eco-physiological characterization of early successional biological soil crusts in heavily human impacted areas – Implications for conservation and succession” by the authors Michelle Szyja, Burkhard Büdel, and Claudia Colesie. I hope that all of you find comments made useful and that they can improve the final manuscript at some extent.

The manuscript is an interest piece of research with a main input to the general state of the art, which is, under my point of view, the ecophysiological characterization of early successional stages of biocrusts. These types of biocrusts are not often analyzed in

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the literature, more focused in later stages of development. The analysis is pertinent in the sense that desertification seems to be a real threat worldwide, and that climatic conditions predicted for the future in several parts of the planet are going to change regarding water availability and distribution. Thus, early successional stages of biocrusts are not only important due to the fact of being pioneer colonizers of barren habitats, but also because climatic predictions point to them as possible dominant organisms in areas under strong hydric stress, where later successional stages would not succeed. Up to the authors to use this concept in the introduction in order to remark even more the importance of their study

Besides this, the manuscript offers, as authors underline, relevant C fluxes data set of not often analyzed biocrusts, which are valuable data for modelling attempts regarding the relevance of these organisms in C cycling and the impact that future environmental changes will have over this. Some of the current models available lacks of proper reliability and only by doing direct measurements of different biocrust types at different parts of the world will be possible to overcome these problems

Finally, authors provide quantification about inorganic fluxes of carbon released in the samples studied, information that is always useful to know before designing an experiment about gas exchange with samples with soil attached. The more we know about this, the better and accurate data sets we will produce.

Due to these points, I would recommend publication of the paper if the authors are able of changing and/or explaining some points that I do not see clear or that, at least, I did not understand properly.

In detail:

INTRODUCTION

Page 4 lines 4-5: Please clarify this sentence. I think that authors want to say here that depending of the treatment made to the sample (sample with soil, without it, or bare

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soil) a different response will be found in the gas exchange experiments. But I do not understand the sentence: “We expect that the position and arrangement of the sample inside the measurement system, here a cuvette, will influence the photosynthetic values”. Are the authors analyzing, at some point, how the position of the sample inside the cuvette is influencing gas exchange measurements?

I think that the sentence is confusing and is not a good choice to close a, on the other hand, well developed introduction

MATERIAL AND METHODS

P5, L13: Could authors provide some info about why was this set of temperatures chosen for the experiment?

P6, L9: I do not see clear how a one way anova can be, at the same time, a multifactorial anova. To my understanding, the authors are using a one way anova with type of crust being the factor (meaning that only on efactor is being analyzed), and each of the dependent variables analyzed at each moment (NP, DR, WC.) being the variable. Is this correct? Probably just a matter of terminology but I see it a bit confussing as written now

P6, L15: A space is needed in “bystatistically”. Besides, which methodology was used to compare these limits?

RESULTS

P7 L23-25. After having a look to Fig. 4 I agree with what is written here, but I think that is falling in contradiction with what is written in the abstract about the issue: “and low or no depression in carbon uptake at water suprasaturation” (abstract L18). I think that the text in the abstract regarding this issue should be changed to fit more accurately what is written in results

P7 L27-28: I think that what authors want to underline here is that C-BSC and G-BSC water content values are close between them both situations, “all” samples and “dom”

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samples. But as it is written now it seems that, for example, for C-BSC “all” and “dom” values are similar between them, which does not seem to be correct. Just a small correction would solve the possible confusion.

FIGURES

Fig. 2. I think that both sub-graphs should be scaled equally at the Y axis in order to compare gas exchange rates between C and G crust types easily

Fig. 4. Please indicate in the figure legend the amount of light used for the experiment

Fig. 6. This figure is hard to follow for me. I think that the variable “effect size” is a ratio between C and G crust types calculated for “dom” and “all” samples and based on area of each sample and chlorophyll content, but I do not understand why such ratio is called “effect size”. Could authors please provide more explanations about this graph? I do not understand either that bump of the effect size at 25 °C for chlorophyll based net photosynthesis. I have read in different parts of the text that authors consider that net photosynthesis has not a statically significant drift with temperature on an area basis, at least for the green algae crust. Does this graph mean that temperature has a significant effect over photosynthesis on a chlorophyll basis but not on area basis? Besides, the figure is supposed to show differences in the effect size for both *N. commune* crusts and *Z. ericetorum*, but I do not see clearly which is which in the graph.

DISCUSSION

P9 L13-22: Authors discuss in this paragraph about the differences in depression of net photosynthesis at high water content between C “dom” crusts and G “dom” crusts, explaining ecologically why makes sense the fact of not finding this depression in *Nostoc* (C) and finding it in *Zygonium* (G). After having a look at figure 4, it seems to me that there are more measured points at high water content (over 80% of maximum water content) in *Zygonium* than in *Nostoc* (I mean, for CBSC dom it seems that there is a gap between 80% and 100% of water content). Any explanation for this?

Could this affect the ecological interpretation of the depression of net photosynthesis at high water contents or authors are using other indicators to analyze this issue?

P9 L23-27: I have gone to the supplement figure S2 in order to try to follow the detection of the CCM mechanism and its relationship with depression of photosynthesis at high water contents. This is something quite interesting physiologically under my point of view that deserves more research efforts in the literature. I have seen that authors propose (correct me if I am wrong) that the fast changes in differential CO₂ response in the gas analyzer after light changes supports the existence of the CCM in Nostoc, and that this was not found in Zygonium. Do you mean that the response of Zygonium after light changes was different or somehow slower than in Nostoc? Is there any support in the literature for this pattern? (I mean presence or absence of CCMs in cyanobacteria Vs green algae)

P10 L1-2: I have been following with interest the lack of optimum temperature for net photosynthesis in the green algal crusts because it was something initially unexpected to me. What I see in relation to this in Fig. 3 regarding C and G “dom” subgraphs, is that Nostoc follows a pattern of raised net photosynthesis with temperature through all the temperature range and that Zygonium shows a raise up to 17°C and a decrease at 22°C (but 22 showed highest photosynthesis compared with 12°C). I know that authors are supporting their idea of lack of temperature optima in the stats, which I think that is right and interesting, but after looking the graphs it seems to me that it could be perfectly said that Nostoc dom has a temperature optima at 25°C and Zygonium at 20°C.

If Zygonium is less adapted to long activity periods than Nostoc, I would expect a concentration of metabolic activity during softer environmental conditions, and this should shift temperature optima to lower values rather than erase the concept of optimum temperature for net photosynthesis. On the other hand, author’s statement of lack of temperature optima in the green algae is supported with the graph 3b for G-BSC all, where the link between net photosynthesis and temperature is clearly erratic

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and defined by a lack of pattern. I just would like to know author's opinion about this, because their approach to T optima concept based in stats is absolutely right to me.

And this is a different issue, but it is surprising to me the lack of statistical differences in Nostoc between C-BSCall and C-BSCsoil net photosynthesis. It means that the photosynthetic cyanobacteria layer of the soil is not creating any relevant C input compared with bare soil. Interpretations for this behaviour?

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

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