Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-365-RC1, 2017

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Algal diversity of temperate biological soil crusts depends on land use intensity and affects phosphorus biogeochemical cycling" *by* Karin Glaser et al.

Anonymous Referee #1

Received and published: 4 December 2017

General Comments:

The presented paper focuses on algal diversity in biological soil crusts (BSCs) forming in temperate forests. So far little is known about the BSCs in temperate forests and what organisms create them. This makes the topic of this paper very interesting. However, the paper unfortunately does not seem to merge very deep to this topic and gives rather shallow impression with multiple inaccuracies.

As a main problem I see the way how the data for algal diversity were obtained. Even though the authors are aware of the fact that the enrichment cultivation method is not suitable for all groups of algae and cyanobacteria and that it can recover only cultivable

Printer-friendly version

Discussion paper

taxa, they still decided to use it as the only source for their data. It seems that at least part of the samples (if not all) were also observed directly in the microscope without cultivation. Why the morphological identification was not done also from these direct observations? The combination of culture dependent and independent methods would provide more accurate and detail information about the algae present in the crusts even without using the molecular methods. And the authors would be able to record not only the presence or absence of given taxa, but also their abundance. Most of the conclusions are thus limited only to the cultivable algae and not to the real forest's crust diversity.

I would appreciate if the Introduction provided more information on the BSCs in forests. Most part of the Introduction introduces BSCs as we know them from the arid regions, including their ecological roles and what threatens them there. But the desert areas and open arid sites in temperate regions are very different from temperate forests, so I think it would be useful if the authors talked a little bit more about whether these facts are true for forests as well. How are the BSCs in forests defined and established? Is "green cover" really equal to BSC? (If "just" green cover was present on a statue or a wall, it would be probably called biofilm, rather than a crust.) Why should they be interesting? Maybe providing more information about the specific sampling sites would help to clarify it as well (did the authors have to remove the litter first to look for the green cover or was the sampling done in open sites in forest, . . .). I know the sampling itself was done as part of different study, but I think this information is worth repeating (maybe as part of Table 1), because it would be important also when looking at the algal diversity and which exact factors influence it.

I am a little confused about the terms silvicultural management intensity, managed forest, and so on. Could the authors please specify more what it means in practice with regard to the BSCs? Do I understand it right that in more managed forests the soil is more often disturbed by heavy machines, traffic, etc? Maybe the authors could provide more detail on how the protected forest differed from the managed forest specifically

Printer-friendly version

Discussion paper

with regard to the crusts (overall soil cover, amount of dead biomass on the ground, density of the tree stand,. . .). Also it is not clear which samples were collected from the protected and which from the managed forest. The only indication the readers get is that the SMI is lower for natural forests and higher for pines. But the authors do not specify anywhere above/below what number the SMI needs to be so the forest can be considered protected/managed. Thus, in Table 1 it is not possible to find out which and how many samples were taken from which protected vs. managed type of forest (and I was not able to find it anywhere else in the text as well).

Even though the title promises information about the phosphorus biogeochemical cycling, the readers do not learn much new information and the data connected with P do not seem to be significant. The previous paper of the authors (Baumann et al., 2017) often referenced in the text seem to provide much detail information.

Specific Comments and Technical Corrections:

I would not mix algae and cyanobacteria under the name algae. Instead of ". . .52 different algae species. . ." I would consider "51 algae and a cyanobacterium" to be more precise as the prokaryotic and eukaryotic organisms are not included together.

The abbreviations "cf." are in italics in many places in the text, please check.

Methods: Study site: How many of the pinus and fagus samples originated from protected vs. managed forests?

page 2, line 3: e.g., Belnap. . . → e.g. Belnap (no comma)

p. 2, l. 31: mucilage SHEDS - mucilage SHEATHS maybe?

p. 3, l. 20: What does DFG stand for?

p. 3, l. 27: . . .the upper two millimeters of the crust WERE. . .

p. 4, l. 18: Community composition based only on algae recovered by cultivation on agar plates does not reflect the real situation.

p. 5, l. 3: algaL richness

p. 5, l. 19: 26 out or 23 samples. . . confusing statement

p. 6, l. 2: , which IS based on. . .

p. 6, ls. 29-31: To overcome these limitations, researchers proposed to combine (!) molecular and morphological methods, SINCE molecular techniques ALONE sometimes ALSO fail to detect some algae.

p. 7, ls. 7-8: Absence or presence. . . The whole sentence in unclear, please check.

p. 7, l. 17: which HAS filamentous nature and WERE determined. . . unify

p. 8, l. 1: Figure 2 does not show anything about Klebsormidium morphospecies.

p. 10, l. 8: bulk soil (Baumann et al., 2017) - space missing

Table 2 : Pearson CORRELATION...

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-365, 2017.