Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-365-AC1, 2018

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.

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Received and published: 10 January 2018

We would like to thank the Referee for the constructive review of our manuscript. It is obvious that it was read carefully by the Referee. All arguments are solid followed by helpful advices to revise the MS.

1) The Referee sees a bigger problem with the way how the algal diversity was obtained. Our results based solely on the identification of cultivable algae. We also observed the algae directly in the crust. But in this case it is impossible to identify the algae correctly for the following reasons: although the crust were rewetted and incubated for a short time, algae are not in a good state. A lot of assimilates or irregular

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shape make it very hard to see and identify the morphological characteristics. Normally in direct observations, only few cells of one species can be well observed; for correct morphological identification many cells of the same species in different states are necessary. For example, for identification of Chlorococcum-species it is necessary to observe also young cells. In a mixture like in the soil crusts, it is hard to tell if one algae is a young status of Chlorococcum, or if it belongs to some Chlamydomonas-like morphotype. This is possible in a well prepared enrichment culture, where colonies of algae are separated on the agar. Also most of the detailed morphological description in "Syllabus der Boden-, Luft- und Flechtenalgen" are based on algal cultures. It is known that environmental factors influence the morphology. Therefore, correct identification is only possible with the same or very similar approaches like in the handbooks; in this case, to use common alga media. The Referee is right, with direct observation we could have also said something about the abundance and thus about biodiversity. As we can only rely on presence/absence data, we will to change the wording throughout the text and rather use "richness" instead of "diversity" to avoid misleading implications.

2) The Referee would appreciate more information on BSC in forests. We understand the doubts of the Referee, because most literature deal with BSCs from arid regions. Thus, we will follow the suggestion to enhance the introduction part. We will also enlarge the sampling description and include some pictures from our sampling campaign, which might help to get an impression of BSCs from temperate forest.

3) The Referee would like to see the section on management intensity more specified. We understand that with more details on the silvicultural management intensity and a careful wording we can avoid confusion about it. We will also add the information about protected and used forests in Table 1 to make this point more clear.

4) The Referee sees a disagreement between the title and the conclusion of the paper. We understand the arguments of the Referee, which is in accordance with the second Referee. Of course, we don't want to make false promises. Thus, we would like to change the title "Algal richness of temperate biological soil crusts depends on

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management intensity and correlates with inorganic phosphours".

5) We are very grateful for the specific comments and technical corrections, which will be all followed as suggested.

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