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Biogeosciences Discussions

Interactive comment on "Phosphatase activity and organic phosphorus turnover on a high Arctic glacier" *by* M. Stibal et al.

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General points

We have added some explanations and clarifications of the concentration and availability of P to the revised version of the ms. Our previous paper (Stibal et al. 2008b, Biogeochemistry 90:1) was fully focussed on the presence, state and availability of P in the supraglacial environment of Werenskioldbreen and defined all the terms connected to it. This paper uses these chemical data (ie dissolved P concentrations, P contents in different fractions within the cryoconite debris, C:N:P ratios) and refers to the previous one wherever needed, looking at the biological response to the chemical environment. We think that repeating all the methods, definitions and results from the previous paper would not benefit the clarity of this paper.

Dissolved P was measured in this study because we expected a different aqueous P environment due to the stable lab conditions. We assume here that SRP is readily available for microbes, unlike DOP.

The length of an ablation season on Werenskioldbreen is ~1-2 months, dependent on the weather. No long-term data on this are available, thus no means or sd's can be provided.

Specific points

Most suggestions have been incorporated into the revised version of the manuscript.

We think that the description of the study site including the previously obtained chemistry data is more concise and reader-friendly than splitting it into Introduction and Methods sections. We have transferred it into a separate "Study site" section outside of the Methods, leaving only the sampling description in it.

As for the light intensity, incident radiation on the glacier surface can be much higher than 50 μmol m-2 s-1 (up to ~1500 μmol m-2 s-1, mean ~250) but this could not be achieved in the lab while maintaining a low T. The used intensity may occur on a foggy day on the glacier (Stibal et al. 2007, FEMS Microbiol Ecol 59:265). It was measured in the used PP tubes. An explanation has been added to the Methods.

SYBR Gold was 100x diluted.

The bioavailability of P is explained in detail in the previous paper (Stibal et al. 2008b), based on previous research. Briefly, chloride and hydroxide extractable P is considered bioavailable. This has been added to the text.

The conversion factor of 2 we use is based on the experimental ratio, which is explicitly stated in the sentence.

As for the dilution issue, the in situ debris:water ratio just could not be used due to the interference in the spectrofluorometric measurements – this is explicitly stated in the

Methods. It is therefore difficult to compare our results to the "real" ones as they cannot be measured. It is unclear how the dilution might affect P-ase activity, the effect would most likely be indirect via light-connected processes such as photosynthesis which are likely influenced by changes in debris:water ratio by means of shading. Adsorption to debris particles affects the detectability of the activity but probably not the activity itself – this is supported by the fact that most active P-ase sites were found to be attached to debris particles in the ELF part.

We compare means or ranges here which is not enough to do robust statistics.

Dissolution of debris is obviously affected by dilution, but all the experiments were done using the same dilution and so we don't think it is an issue here.

About the temperature effect, what we say here is that the T effect on P-ase activity in our samples is similar to other places and so the enzyme is not likely to be adapted to very low T. Some organisms living permanently in very cold places have cold-adapted enzymes; this is just not the case. We agree that the effect of substrate concentration will be higher than that of temperature. So, the potential of P-ase activity in downstream systems will be greater given a higher T and similar substrate concentrations, which is the case for most proglacial environments.

As for the light vs dark activities, this is probably a confusing formulation – we didn't intend to suggest that there was a stimulation of P-ase activity by darkness as much as that the lack of light stimulation (expected for light-stimulated phototrophic microbes which would need P) may mean that the activity is mostly associated with heterotrophs. This was just a suggestion and it encouraged us to use ELF. We have clarified it in the text.

The last section (OP turnover potential) has been rewritten as suggested.

Interactive comment on Biogeosciences Discuss., 6, 2697, 2009.