

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

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General comments

1) We are aware of the fact that SRP is not equal to dissolved inorganic P, and we have deleted the confused formulation in the Methods. However, we think that calculating DOP concentrations as TDP-SRP is a standard practice widely used in ecological literature (e.g. the paper of Dore and Priscu (2001) on P deficiency in Antarctic lakes). Although the actual concentration of organic and inorganic P species may be slightly different from what we obtain using the described method, we think that 1) it is useful to use the same method as used in most other studies that we want to compare our results with, and 2) even if there were slight deviations from the reality in terms of organic and inorganic P concentrations, the general conclusion that organic P dominates

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in the system and that the microbes respond to it by phosphatase activity, resulting in the recycling of OP, is still valid.

We have corrected the detection limit - the actual detection limit of the described dissolved P analysis was $\sim 0.2 \mu\text{M}$, mainly due to using bicarbonate buffer as the matrix. The P concentrations in all the samples used in this study were above this limit and so the results are not compromised in any way. The low detection limit of $\sim 0.015 \mu\text{M}$ (or $\sim 0.5 \mu\text{g/l}$), stated originally in the methods, comes from earlier analyses of supraglacial water samples (Stibal et al. 2008b). In this study, P standards of $0.5 \mu\text{g/l}$ were found to be significantly different from blanks.

2) We used different kind of assays in order to answer different kind of questions. We state the conditions of the assay in every figure caption, including the number of assays done (n). Table 2 illustrates the approach we took to estimate the OP utilisation potential, and is compiled from data shown in Figs 3 and 4. Fig 2 shows the assay we used for the calculations of the kinetics parameters K_m and V_{max} , using a substrate concentration range of $0\text{--}500 \mu\text{M}$ ($n=8$). We think that the Eadie-Hofstee transformation is an accepted and routinely used method to do this.

3) We did not assume Michaelis-Menten kinetics implicitly, but our data shown in Fig 2 seemed to follow it quite well. We agree that our samples may contain a mixture of different enzymes; however, most active microbes in this environment are bacteria (including cyanobacteria) unlike in phytoplankton where algae may be important. Fig 2 shows the range of substrate concentrations we used and the low variability between measurements (error bars), and we think it suggests that the substrate concentration of $100 \mu\text{M}$ is indeed saturating.

We agree that we misinterpreted some of our inhibition data, and we have rewritten parts of the Discussion in the revised version of the ms. Para 2 (2707 lines 18-23) has been deleted.

4) This was a confusing formulation – we did not intend to suggest that there was a

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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 this issue in the text.

Specific comments

We have corrected “phosphatase activity rates” to “phosphatase activity” where needed.

We say that “The ratio of (debris:water) 1:500 was found to sequester only 5-7% of MU. . .”. We do not mean here that it was sequestered by the microbes, only that it was lost from solution, most likely due to adsorption. This had to be tested on MU since MUP is not detectable using this method.

An explanation of the quenching correction has been added to the revised version of the ms.

Yes individual cells in cyanobacterial filaments can be distinguished due to chlorophyll autofluorescence inside the cells and enumerated using the filter block U-MWU2.

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