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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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Major issues

Ad 1) We have checked all the calibration data and we must admit that the actual detection limit of the described dissolved P analysis was ~0.2 μM, mainly due to using bicarbonate buffer as the matrix, and we have corrected it in the text. 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 ~0.015 μM (or ~0.5 μg/l), stated in the methods, comes from earlier analyses of supraglacial water samples (Stibal et al. 2008b, Biogeochemistry 90:1). In this study, P standards of 0.5 μg/l were found to be significantly different from water blanks, and all the very low values (0.75 μg/l on average) in that study were obtained on the same day as this calibration. The value

of 0.62 μg/l (i.e. 0.02 μM) is from Dore and Priscu 2001, Limnol Oceanogr 46:1331. Therefore, we believe that all the P concentrations and their ratios shown or cited in this paper are reasonably trustworthy.

Ad 2) We explain why we used the sediment weight normalisation in the Discussion (p 2708 lines 8-15) and convert our values to the most widely used unit (μmol l-1 h-1; e.g. Dore and Priscu 2001, Mikucki et al. 2004, Foreman et al. 2007) (p 2708 lines 23-25). However, we agree that it is a good idea to normalise our results to biomass, and we have added a biomass normalisation estimate to the Discussion of the revised version of the manuscript, based on the amount of C (being ~50% of POM) in the debris (Stibal et al 2008a, Environ Microbiol 10:2172-2178). The values (~95-670 nmol P mg(POM)-1 h-1) indicate severe P limitation (Jansson et al. 1988, Hydrobiologia 170:157).

Ad 3) This is probably a confusing formulation – we did not 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 this issue in the text.

Ad 4) This is a very good point. It can be explained in several ways. First, the free enzyme may be relatively short-lived (e.g. Pettersson 1980, Arch Hydrobiol 89:54); second, the enzyme activity sites may have been detached from the cells during handling; and third, most likely and most important: the P-ase activity may be inhibited by the added phosphate rather than the production repressed. The latter is actually supported by the insignificant effect of DIP at saturating concentration of MUP (except in 24-hr incubations in the dark) – at low concentrations of MUP high DIP blocks the enzyme and inhibits the activity, whereas at high MUP concentration (>DIP addition) MUP blocks out DIP and is processed by the enzyme. This may then mean than the P-ase activity is more inhibited than repressed and that the production may actually be constitutive rather than repressible, although some repression probably occurred during the 24-hr incubations. We have changed the Discussion accordingly in the revised

version of the ms; the evidence is still that P is deficient (see ad 2) but it may be that the microbes maintain a relatively high production of P-ase. This was found in Antarctic lakes and attributed to an adaptation to rapid and stochastic substrate input (Dore and Priscu 2001).

Ad 5) We acknowledge the limitation of this estimate, and we clearly state this in the text. We have measured SRP and DOP in situ as well as various fractions of debris-bound P (Stibal et al. 2008b), and we think that most of the debris-bound is physically able to interact with living microbes due to its being either loosely bound or associated with biomass; however, it is clear that we could not simulate the in situ conditions in the lab precisely. Still, we think that such an estimate is of interest for future nutrient cycling studies of glacial environments. We think that the image we suggest – that while DOP is relatively rapidly recycled, the debris-bound P pool is large and probably sufficient for the microbial community for the whole season ($\sim$2 months in the summer) – is correct despite the unavoidable departure from the in situ conditions.

Minor issues 1) We have deleted “limiting”.

2) The cryoconite debris used in this study was described in detail in previous papers (Stibal et al. 2008a, b, and its P contents are also stated in the Study site section (p 2700 l 22-26).

3) Significant = measurable and significantly different from controls

4) “controlled” changed to “inhibited”

5) “rates” deleted

6) There is evidence that unlike organic C and N, OP in the debris does not decrease over time (Stibal et al. 2008b). Microbial biomass is washed away from the glacier to proglacial and subglacial systems along with the debris and does not accumulate on the surface (Stibal et al. 2008a).

7) We say that debris along with atmospheric deposition is the main source. It is

true that IC and IN probably come mostly from the atmosphere (as CO2 fixation and deposition of NO3-). We don’t have enough information about nitrogen fixation in this environment at the moment, and the work on this is in progress.

8) Nutrient poor defined in the revised version (<4 μM DIN, <0.1 μM SRP)

9) We assume that if the concentration of inorganic nutrients is very low (which it is), they are likely to be rapidly sequestered by the microbes.

10) This is SRP, and here it might be higher than in oligotrophic lakes due to the high load of biomass-packed debris. The mean SRP at this glacier was $\sim$20 nM (Stibal et al. 2008b).

11) This is defined in our previous paper (Stibal et al. 2008b), and is all particulate P, ie chemically bound, adsorbed and microbial biomass-contained. We have added this to the revised manuscript.

12) Defined in Stibal et al. 2008b according to published work (ie Hodson et al. 2004, Hydrol Process 18:2409), briefly chloride and hydroxide extractable P. An explanation has been added to the revised version.

13) “Dissolved organic P” replaced by “phosphate monoesters”

14) Thank you for pointing this paper out to us, we now refer to it.

15) “Arctic ecosystems” changed to “Arctic terrestrial ecosystems”

16) see 12)

17) “dissolved inorganic P species” deleted

18) Quenching correction description added

19) We don’t know here, but we assume that both adsorption and physical shading play a role.

20) Typesetting issue

21) All errors are standard deviations, added to text

22) The sd here is correct – it's the sd of the calculated ratios of TDP-SRP and TDP in individual samples.

23) Probably very few as they are mostly filamentous and entangled within the debris.

24) Some may be (ie Nostoc); work on nitrogen fixation is currently in progress.

25) TDC:TDP ratios deleted

26) Paragraph deleted (see also major point 4)

27) Yes but the water:debris ratio in the lakes is probably much lower that in cryoconite holes.

28) This possibility has been added to the text.

29) Not from cryoconite but there are data from other glacial sediments (Hodson et al. 2004 and references therein).

30) Bacteria may have a higher affinity to phosphate than algae, but most phototrophs here are cyanobacteria.

31) It is possible that alkaline P-ases would be more likely to be inactivated; however, it is also possible that some or most of the P-ases here are acid. This was the case in some acid lakes (Jansson et al. 1988). Therefore, we use the term "phosphatase" in the general form and avoid the alkaline/acid tags.

32) There are birds on Svalbard, but they mostly nest on the shore or on the cliffs and very rarely fly over the glaciers. We assume that the input of bird P to the glacier surface is negligible.

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