Biogeosciences Discuss., 7, C4518–C4521, 2010
www.biogeosciences-discuss.net/7/C4518/2010/

Biogeosciences Discussions

Interactive comment on "Effect of permafrost thawing on the organic carbon and trace element colloidal speciation and microbial activity in thermokarst lakes of Western Siberia" *by* O. S. Pokrovsky et al.

Anonymous Referee #3

Received and published: 27 December 2010

General comments:

There is a great need in studying thermokarst ecosystems that have a great potential to participate to global carbon cycling. This study therefore presents very important results, and a powerful way to study compounds at the fringe between POM and DOM and their relation to TE. These processes are fundamental for in-lake mechanisms (like GHG emissions and carbon burial) and export to coastal marine waters. The work is of good scientific quality, but the syntax needs to be improved and some redundancy eliminated in order to facilitate understanding of this precious data set.

Specific comments:

1. The interpretations/conclusions may be a little over-rated as the results obtained can only suggest biological processes (bacterial degradation and production of phytoplankton exudates) acting along the chronosequence. Planktonic characteristics (minimally Chla and total bacteria) would have helped to support conclusions relative to autochthonous metabolites acting at the oldest stages. CDOM optical characteristics could bring insights on DOM composition (see for example Helms et al. 2008); it would have been interesting to include these in such a great effort of size fractionation. Including 'microbial activity' in the title does not reflect what the study mostly covers (OM-TE interactions along the chronosequence). Culturable heterotrophic bacteria may only represent a small fraction of total active bacteria in such systems. Culturing microbes under lab conditions may only reflect a small proportion of the species present (the ones that respond well to culture conditions), not necessarily the ones that had an important function in situ, with a possible bias towards rare species (see Pedros-Alio 2006). What culturable heterotrophic bacteria really means needs to be discussed. Also, this method for culturing bacteria under 3 trophic conditions needs to be supported.

2. Are there any evidences in the litterature that microbes use OM and leave out the associated minerals?

3. Along the chronosequence, photolysis is another important factor that affects DOM molecular size (and perhaps coagulation/flocculation?), especially is such exposed ecosystems. Can this be discussed?

4. Although authors refer to earlier papers for methods, the delay between sampling and measurements needs to be given for this specific study and consequences need to be acknowledged. For example, sample fractions >0.2 μm contain bacteria which continue to transform OM in the bottles; delays in measurements can generate biases. Also, these fractions include microbial cells (prokaryotes and small eukaryotes of less

BGD

7, C4518–C4521, 2010

than 5μm) which contribute to OM, not only organic molecules of different sizes. Could these aspects be discussed/acknowledged? (cf Fig. 5).

5. Key words such as GHG fluxes, heterotrophic lake status, allochthonous C (peat) bacterial utilization, and the hypothesis '...thermokarst lakes (are) efficient mediators of carbon flux from soil to the atmosphere via bacterial decomposition of dissolved OM', are presented in the introduction, yet the implications of OM-TE interactions along the chronosequence is not really discussed nor the hypothesis tested.

6. p. 8045, line 25: The sentence 'As such, microbial transformation of aquatic organic and organo-mineral colloids should be the main mechanism of soil allochtonous OM processing by aquatic biota in the course of thermokarst lake development', needs to be better supported.

7. Presence of phytoplankton and zooplankton in old khasyreys is mentionned in the study site description: does it mean you have data on that? It would corroborate the observed organic ligands and the putative phytoplankton exometabolites.

8. p. 8049. There seems to be a contradiction in the results: all OM fractions decrease along the chronosequence, with more pronounced decreases for the smallest fractions, but at the same time relative proportion of small size DOC increases from 15 to 20 to 23%? Also, what is small-size allochthonous OC in the form of phytoplankton exometabolites; allochthonous carbon can not come from phytoplankton. This section needs to be clarified and better supported (see above).

9. Why refering to <1 kDa OC as 'potentially bioavailable' since heterotrophic bacteria are presented as consumers or both colloidal and truly dissolved OC??

10. FO and O bacteria did not grow, or they only did not correlate to OM?

11. After giving the results of figure 7, there is a discussion on the controlling factors of CO2 supersaturation that seems out of context.

12. If dissolved trace metals (Fe, Al) are a unique feature of themorkarst lakes, can

BGD

7, C4518–C4521, 2010

you give some comparisons with other lake types? What makes them unique needs to be explicitely given. In the text, I only read that insoluble TE (like Ti and Zr) are very concentrated compared to continental waters and Siberian rivers.

13. Would be interesting to include some discussions on microgels and nanogels which are large colloids (see P. Verdugo's papers).

Technical corrections:

-p. 8058, line 19: bacterioplankton (typing).

-Allochthonous (add an h).

Plus several other things; the manuscript needs be revised by and English-speaking person.

Interactive comment on Biogeosciences Discuss., 7, 8041, 2010.