

***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.***

**Dr. ingri (Referee)**

johan.ingri@ltu.se

Received and published: 10 December 2010

**General comments** This is a very interesting paper. Although the investigation is performed in Western Siberia, the results are of excellent scientific significance in many aspects. The scientific quality is good to excellent. Some corrections in the English text (not specified in this comment) are necessary but overall the presentation quality is good.

**Specific comments** In the conclusions it is stated that culturable heterotrophic bacteria

C4280

decrease along the lake development (page 8058 line 6) but on line 13 it is stated “ Upon progressive consumption of OM by bacteria in the course of lake development there is a systematic decrease of both DOC and dissolved metal concentrations”. ????

How can it be a progressive consumption when the bacteria numbers decrease?

It is stated that phytoplankton produce small inert ligands that complex Cu and Cd. It is not clearly shown how this was measured?

Are these small organic ligands produced by photo reduction? In this type of water (fully oxidised, low pH, high DOC, high Fe) photo reduction of organic bound Fe(III) should be important (during the Arctic summer). Are there any indications of photo reduction in the data? The authors should add a short section discussing the role that photo oxidation might have on the measured components.

Why are the total concentrations of Fe and Al higher than for elements like Ca, Mg etc.? Which type of weathering (or other processes) can form this type of enrichment of Al and Fe? A short discussion about this would have been interesting.

Are the dissolved and particulate ratios Fe/Al, Al/Ti, Zr/Ti reflecting local bedrock or not? It would have been informative if the authors briefly had discussed element ratios in the material. Usually, Al and Ti are used for normalisation of particulate-colloidal data, an element/Ti or element/Al ratio close to the average crust ratio usually indicate detrital particles, but is this normalisation procedure valid in Boreal-Arctic waters?

The authors state that the free water column in all lakes were fully oxidised. Was any reduced interface detected in the bottom sediment/solid organic soil?

Manganese shows high dissolved concentrations in the water, in spite of high dissolved oxygen concentrations. Manganese does not form strong complexes with humic substances, why are then high Mn concentrations found in most lakes (low pH)? Is the decrease of Mn in Figure 9 correlated with temperature or pH? This decrease appears not to be related to increased bacterial activity and subsequent increased oxidation of

C4281

Mn (culturable heterotrophic bacteria decrease along the lake development)?

Technical corrections The map in figure 1, is not very clear. The detailed map is not necessary

Page 8051 line 2-3 in the form of neutral molecules. . . .of these elements only  $\text{Si(OH)}_4$  should be zero charged?

alkaline earth not alkali-earth

page 8052 line 25-27 is this a valid conclusion, the correlation coefficient is larger for Fe compared with Al?

Page 8050 line 21 usually not usual

Page 8054 line 19-21 It is important. . .DOC concentration. It is not clear what the authors mean, rewrite.

Page 8057 line 17 (TE/Fe) dissolved should be truly dissolved. It is not clear why the  $K_d$  value represents incorporation of TE inside the colloids. You can still have adsorption on the surface of the colloid?

---

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