

Interactive comment on “Seasonal dynamics of organic carbon and metals in thermokarst lakes from the discontinuous permafrost zone of western Siberia” by R. M. Manasyrov et al.

Anonymous Referee #1

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This is an interesting paper with a huge amount of data. The authors have done a good effort to interpret the huge amount of data. Given the large quantity of data I wonder if it would have been worth attempting a principal component analysis to further disentangle TE mobilization.

Maybe the authors should consider having another or enhanced conceptual figure that explains TC cycling in their systems. Is it possible to add vegetation and deeper peat element leaching to figure 11 ?

The authors hint that large parts of the particulate Fe bound TE do not make it into the river systems. I think this is an important observation that might deserve a little more

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discussion. Is this a plausible mechanism for TE enrichment in sediments ?

No effort was made to model the distribution of elements using chemical equilibrium modelling such as WHAM VII or VisualMinteq, why ?

The authors measured pH but did not use it to explain some of their observations. Are their results independent of pH ? There is evidence in the literature that pH has a large control on both TE binding and Fe speciation. How much is Ph controlled by DOC and how much by acid anions ?

This is a long list of authors. is it possible to describe the individual contribution of the authors to this manuscript?

From figure 2 to 5 it seems that there are no small lakes available in summer. The question arises which lakes change size?

P. 9 L 12 Were these slopes corrected for the presence of dissolved iron ? You have only three winter samples it seems. Maybe remove the sequence in the figure caption of figure 3. Did you do any statistical analysis of TE and UV280 ? Maybe you could relate that to the element leaching of vegetation as opposed to peat leaching ?

P 13 L. 28 and P 14 L. 2 : This is probably a good back of the envelope calculation but it is difficult to follow. Could you please explain with two more sentences your assumptions. I.e. Kd values for water/ice...maybe consider 1 m² of lake.

P. 16 L. 10 This is a large difference I agree. I would still suggest that the authors make an effort to correct that data for the contribution from dissolved iron.

P. 18 L. 8 Was that Fe(II) method mentioned in the methods section ?

P. 18 L. 25 This should eventually lead to an enrichment of trace elements in the solid phase. Would you happen to have any evidence of that from sediment samples ?

P.19 L. 8 Is that process maybe worth adding to a conceptual figure ? Maybe add to figure 11?

BGD

12, C275–C277, 2015

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P. 20 L. 6 Here and earlier it would be nice if the authors would make an attempt to model the speciation. Why would Cu not be dependent on DOM colloids ?

P.20 L. 13 I assume that you made some solubility calculations...please explain

Figure 3: Please remove winter in the sequence. You have only three sample sites.

Figure 4: Did you attempt to normalize TE by Ca so see which elements could originate from litter leachate ? I assume that element ratios from a number of mosses are known .

Figure 5: How much of the pH variation is explained by the presence of DOC. You seem to have data on ANC (major cations and major anions).

Figure 6. is this figure needed ?

Figure 9: Do you have any explanation for the high Zn values during winter ?

Figure 10: Fe should be ppb instead of ppm.

Figure 11: This figure could be complemented by TE mobility from either leached vegetation, litter or peat.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/12/C275/2015/bgd-12-C275-2015-supplement.pdf>

Interactive comment on Biogeosciences Discuss., 12, 1975, 2015.

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