Biogeosciences Discuss., 8, C4085–C4089, 2011
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Biogeosciences Discussions

Interactive comment on "Organic matter mineralization and trace element post-depositional redistribution in Western Siberia thermokarst lake sediments" *by* S. Audry et al.

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Received and published: 2 November 2011

General comments The authors present a clear, multi-faceted study of sediments of a thermokarst lake located in Western Sibiria. This study is somewhat unique due to the comparison of lakes representing different stages of ecosystem development. Overall, the report appears scientifically sound, but the text still needs the author0s attention with respect to style and some clarifications (see Specific comments below). In general, you should avoid overstating the significance of your study. First, you base your statements on one time measurements. Secondly, you did not measure processes. You can only draw conclusions from concentration differences in the profiles. This is

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no direct proof. So, please weaken your strong statements (Page 20: mineralization of OM, shift in pathways, related to heterotrophic activity, etc.) in section 6 (Environmental significance and conclusions).

We agree that we did not measure processes and that we based our discussion and conclusions on indirect proofs. However, it is widely admitted that our approach, based on porewater and particulate concentration-depth profiles, is a powerful tool, whilst indirect, to access processes occurring in sediments during organic matter mineralization. We also agree that in some cases we might have overstated our statements. Therefore, and accordingly to the referee suggestions, we weakened our strong statements when needed in the revised manuscript, particularly in Section 6. Clarifications of the text have been done according to the referee's comments and suggestions

Specific Comments 1) Page 10, Porewater: You can shorten the part of the results describing porewater geochemistry, since the data presented in the figures are very easy to understand and self-explaining. Just highlight the main differences between the profiles.

We have shortened (by about 20%) the portion of the text describing porewater geochemistry as suggested by the referee. We think that further shortening could impair the comprehension of the points we aim at making in the Discussion section.

2) Page 14, line 416: You did not determine the reductive dissolution of Mnoxyhydroxides. You only measured increased concentrations of Mn(II) which suggest reductive dissolution. So, please weaken your statement.

We weakened our statements about reductive dissolution of Mn- and Fe-oxyhydroxides in the whole Section 5.2 according to the referee's comment.

3) Page 4, line 442: It should read "anaerobic nitrification". We differentiate between oxic and anoxic conditions, but there are aerobic or anaerobic processes and aerobic or anaerobic microbial populations.

BGD

8, C4085–C4089, 2011

We agree. The text has been changed accordingly.

4) Page 15, line 450: Please change the order of these processes with respect to the order of the more favorable thermodynamic process.

The order of the oxidation reactions of organic matter have been changed with respect to the amount of resulting energy of each reaction: denitrification, reductive dissolution of Mn- and Fe-oxyhydroxides and sulfate reduction.

5) Page 15, line 477: You are mentioning here transient redox conditions caused by groundwater inflow. These redox fluctuations could have caused regeneration of the Fe- and Mn-oxyhydroxide pool. However, this regeneration was excluded in the paragraph above. This is contradictory to me. Please clarify.

We agree that this was confusing phrasing. The end of Section 5.2 was rewritten for more clarity and to avoid any contradiction with Section 5.3. It reads now (page 15): "Therefore, in the course of the life cycle of thermokarst lakes, reductive dissolution of Fe- and Mn-oxyhydroxides likely becomes the prevalent OM mineralization pathway. Accordingly, reactive Fe- and Mn-oxyhydroxides become progressively depleted, as evidenced by the decrease of Feasc and Mnasc from the 3rd stage to the 5th stage (Fig. 4). This also suggests that the possible partial regeneration of Fe- and Mn-oxyhydroxides due to oxygenated surface or ground water circulation at depth (see section 5.3 below) is not enough to maintain both such pools during the course of the ecosystem maturation."

6) Page 17, line 534: Unfortunately, you did not measure AVS concentrations which would include FeS. Please comment.

Results from AVS extractions are known to be very difficult to interpret because: (i) they include both components argued to be part of and not part of the pool of acid-reactive sedimentary sulfide components and (ii) AVS may be almost totally dissolved sulfide species and not reflect the presence of metastable iron sulfide minerals (Rickard &

BGD

8, C4085–C4089, 2011

Morse, Marine Chemistry 97, 2005, 141–197). Therefore, we chose not to determine AVS contents in our sediments. However, we believe that the concentration-depth profiles of Fe we present is our manuscript are explicit enough (showing a clear decrease, i.e. consumption, of porewater Fe at depth in the sediment) to support our contention of authigenic precipitation of Fe-sulfides and trace element association with these.

7) Page 18, line 552: The release of As under anoxic conditions could be caused by release or by reduction of As(V) to As(III) which is more soluble under reduced conditions. Please modify your statement.

Our statement has been modified according to the referee's suggestion (page 18, lines 570-571).

8) Page 19, line 606: I strongly disagree that high-resolution concentration-depth profiles are provided. Your resolution is not higher than from other studies using peeper, dialysis chambers, or sediment cores. Most readers would think in a micrometer- or nanoscale if you use this term. Please avoid this misnomer.

We agree. The term "high-resolution" was withdrawn from the text.

9) Please check your list of references for typing errors.

The list of references have been checked and corrected when needed.

10) Table 1: What does the gap mean? Not determined?

For more clarity, the 'hyphen' has been replaced by "n.d." and the table caption has been updated accordingly.

11) Table 2: Do you refer to dry wt of soil? Which depths were sampled?

In this table, we do not refer to soil but to the sediments of three thermokarst lakes, as stated in the table caption. The values given in this table are for dry weight (this detail has been added to the table caption) of sediments and represent the mean particulate concentrations calculated from all the samples of each cores.

BGD

8, C4085–C4089, 2011

12) Table 5: Please reformat this table. It should be similar to the others.

The ‘arial’ font has been changed for the ‘Times New Roman’ font.

13) Figure 8. Where are the other data in a and c? You stop after 15 cm depth.

Due to very small volumes of porewater retrieved after centrifugation for Shirokoe and Khasyrei lakes for sediment samples below 15 cm depth, it was not possible to determine trace element concentrations in porewater samples below 15 cm depth.

Interactive comment on Biogeosciences Discuss., 8, 8845, 2011.