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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.

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The reviewer is right saying that referring to the fraction < 1 kDa as “potentially bioavailable” is not warranted, given the large amount of information on lower bioavailability of small-size organic matter fractions and we added necessary remark in the last section of the manuscript together with pertinent references. In the revised version of the manuscript, we describe the 1 kDa dialysate as Low Molecular Weight (LMW) fraction. We corrected the Fig. 4 caption as recommended. The reviewer suggests that the coagulation and flocculation of organic matter cannot explain the size distribution of

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OC presented in Fig. 5 and proposes that the increase in 0.45–5 μm fraction of OC may be due to degradation of large planktonic cells which produce cell fragments in this size range. He is also right that exopolymeric substances from phytoplankton and bacteria may also contribute to this increase. We modified the text accordingly and added pertinent references. The reviewer is also right saying that, in addition to exometabolites production, the smaller size of OC in the larger and mature lakes can be a result of lower input of soil and peat-derived organic matter to these lakes, due mostly to a large water-body in relation to the length of the shoreline. As such, the average residence time of the allochthonous organic macromolecules in these lakes is longer, and they have been exposed to degradation by bacterioplankton for a longer time, and are therefore smaller in size than those in the smaller, young lakes. We modified the text accordingly and added necessary citations. We agree with the reviewer that in Fig. 7 there is a certain contribution of small OM from phytoplankton producing more fresh OM compared to refractory soil-derived fraction which is respired by bacterioplankton. We corrected the text accordingly. Page 11: We corrected the term neutral molecules and we shifted the elements such as Sb and As to the group of anions. Note that Ge is likely to be in the form of $\text{Ge}(\text{OH})_4^\ominus$. Page 12: The reviewer suggests that, because fulvic acids are just a few nm in size, significant part of soil-derived organic matter is found in the 1 kDa dialysate and Cu and Cd are not influenced by the coagulation of colloidal Fe and Al oxy(hydr)oxides and remain in LMW fraction. We agree and added this possibility in revised text (section 3.2). Page 13. The reviewer proposes that the proportion of small size Fe decreases with pH because Fe-hydroxide is the first step in the formation of Fe-oxy(hydr)oxide particles. This is certainly true, however, in studied system the majority of ionic Fe is bound to complexes with LMW organic ligands and it is the competition between the $\text{Fe}(\text{OH})_n(3-n)^+(\text{aq})$ and Fe-LMW species that will define the pH-dependent pattern of in-situ dialysis. We did not modify the text in response to this comment. Page 15: The reviewer is certainly right that the decreased concentration of organic acids may lead to a higher pH. However, the decrease of pH along the chronosequence of lake development is not that well pronounced as DOC concen-

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tration and we can not use this criterion as quantitative degree of system maturation. Page 15: The reviewer also noted that the concentration of 0.45-5 μm organic matter could increase along the ecosystem stabilization trend. This increase stems from appearance of biopolymers and other organic matter from phytoplankton and could further contribute to the coagulation of colloidal Fe and Al (Wilkinson et al., 1997; Perret et al., 2000). This is very insightful comment and we added this possibility to revised text in section 3.3.

We corrected all technical details noted by the reviewer

We thank Björn Stolpe for his very constructive comments.

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