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Interactive comment on "Porewater $\delta^{13}C_{DOC}$ Indicates Variable Extent Of Degradation In Different Talik Layers Of Coastal Alaskan Thermokarst Lakes" *by* Ove H. Meisel et al.

Anonymous Referee #1

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This manuscript reads extremely well. The design of the study is described well, the methods are solid, the presentation of the data is very clear. The main finding is that the top sediment layer of thermokarst lakes has more active C cycling than the bottom layer - which stems from a different geological period and has mainly been formed as a marine deposit, and probably prior to the onset of permafrost formation. The results that are discussed most extensively are the carbon isotope data, especially delta-13-C. The discussion section is quite speculative - opening for mechanisms that drive the isotope signal both up and down. It is finally concluded that the relatively large variation of the delta-13-C signal in the top layer is likely to be related to the increased DOM degradation rates.

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I would suggest an equally plausible explanation, i.e. that the top layer is more heterogeneous than the bottom layer, because the C inputs here stem from 'old' (permafrost eroded soil) and 'young' (tundra vegetation) sources (how about sphagnum in the ponds? not mentioned - but the ponds are shallow and could have a thriving sphagnum community). The weakness in the data collection is that the isotopic characterisation has not been matched with an assessment of DOM degradability. That could have been done with a simple dark incubation, and would have given a measure of which DOM (talik or lake sediment DOC) was most accessible to microbes. It is likely, of course, that the microbial community in the top is a lot more active (and has access to more nutrients) than in the talik. So some additional nutrient measurements (CN ratios of DOM can also give an indication of DOM nutrient depletion and how far it might be degraded) would have been helpful. So, it seems a bit as though the isotopic data (except for the 14C) actually doesn't help a lot with understanding C cycling in these lakes. I can agree with the conclusion - but not because of the evidence suggested by the 13C results. You also write yourself that the variation in DOC might be source-driven (l 505), but you seem to leave that line of reasoning later in the discussion

So while I admire the manuscript for its clarity and its great explanations of thermokarst formation, I think that the interpretation of the data has some lose ends. 1. I miss a hypothesis (l 98 would be a good place). 2. please add some information about the presence of vegetation / sphagnum in the lakes (minor issue, but good to know). 3. a more extended explanation of the normalizations should be added to the methods. 4. Figure 5a suggests to me that an equal amount of SOM delivers more DOC in the top than the bottom (l 485), which a different interpretation than what you have. Please clarify. 6. Fig 5b (should you turn around the axes? DOC on the x-axis? more intuitive to me), here it looks like the DOC concentration drives the isotope signal and that the isotope signal in both layers is essentially the same, it's just driven by differences in DOC level. This is also shown by figure 6b (LOI and DOC were somewhat interchangeable, weren't they?) for 5a, what is the relationship if you plot it against normalized DOM? 7.

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l 505. DOC is not highly bioavailable, especially when it's allochtonous material. It's of course a relative measure, but have a look at the Catalan paper on half-lives of DOM along the aquatic continuum. In boreal systems, oligotrophic, cold, terrigenic DOM can be quite resistant to degradation. 8. Kudos for discussing pH dependence solubility and sorption/desorption of DOM.

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