

## Reply on RC2

Karis J. McFarlane et al.

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Author comment on "Age and chemistry of dissolved organic carbon reveal enhanced leaching of ancient labile carbon at the permafrost thaw zone" by Karis J. McFarlane et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-272-AC2>, 2021

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*Authors' reply - Thank you for your review and suggestions for improvements to the manuscript. We agree with your comments and will make the minor edits suggested in the revision, including the additional references you've provided. You also make several important points that we will address in the revision as we agree they will improve the manuscript.*

*1. Clarification of how we relate our measurements to lability of DOC. Thank you for identifying this issue. We will include the revision a more developed explanation of how characteristics (SUVA and DOC:DON ratio) of the dissolved organic matter chemistry can relate to decomposability and the extent of microbial processing of organic matter. This should address several comments (in the abstract, introduction, and results/discussion) related to how we interpret our data to suggest a highly labile DOC source and differences with age across the drained thaw lake basins. For the sake of clarity in this response, we interpret higher DOC:DON and SUVAs to reflect fresh vegetation inputs, which should have: 1) higher C contents relative to N (these ratios converge with microbial processing); 2) higher SUVA 440, indicating more high molecular weight compounds (which break down into smaller molecular weight compounds with microbial processing); and 3) higher SUVA 254 and 350, indicating greater aromaticity and lignin content – compounds that are present in plant C inputs and break down with microbial processing.*

*2. Clarification of the trends in CH<sub>4</sub> concentration for the surface and shallow samples. R1 also pointed out an issue with this sentence (L225-226). In the revision, we will remove the correlation results – this is not a meaningful correlation between 14C and CH<sub>4</sub> concentration, but a shift in surface and porewater chemistry from June to September. We had attempted to clarify that it is the seasonal pattern that was meaningful, but agree that it was confusing to include the correlation results. We will only provide the ANOVA results, which show the decline in CH<sub>4</sub> concentration in surface and shallow porewaters from July to September. CH<sub>4</sub> concentrations were presented and discussed in the Throckmorton et al., 2015 paper, and we will refer to that paper here as well for more discussion on these patterns. This point was intended to be a relatively minor one to place the new data in the context of the other geochemical data available for these samples and to lead into the following paragraph focused on the deep porewater. We think making this change to this sentence will help clarify our point concisely. You pose interesting questions, however! As discussed by Throckmorton et al., there does seem to be a shift not only in the methanogenesis pathway (more acetoclastic in July and more hydrogenotrophic in September) but also in the depth where most of the CH<sub>4</sub> is being*

*produced (in shallow porewater in July to deep porewater in September). Throckmorton et al. also suggested this might be driven by a shift in C source related to vegetation inputs. We have looked to see if we observed correlations between CH<sub>4</sub> concentrations and the DOM chemistry variables we measured. If the increase in CH<sub>4</sub> was driven by plant inputs, we would expect to see a correlation with the indicators of fresh plant organic matter sources (higher CH<sub>4</sub> with higher DOC:DON or SUVAs) – but we did not observe any correlation between CH<sub>4</sub> concentration and OM chemistry. An alternative to the plant C inputs hypothesis, is that the CH<sub>4</sub> values may be higher in July than in September because of a buildup of acetate over the winter when cold temperatures should inhibit acetoclastic methanogenesis, but we did not measure acetate at our sites and this is a speculation. We do not think our data are strong enough to support one of these explanations over the other. This is a point that we could develop in the paragraph following the discussion of the CH<sub>4</sub> fractionation factor, though we had not intended to do so as we have not added anything new to what was discussed previously in the Throckmorton et al paper.*

*3. More information about DTLB ecology to aid in interpretation of results. This is a great suggestion and will be worked into the introduction and this discussion paragraph (starting at line 273). In the introduction, this will include a bit more description of how DOC chemistry and age might change with different watershed drainage characteristics as well. For example, in DTLBs in our study region, organic layer thickness and degree of organic matter decomposition increase with increasing DTLB age. Young and medium aged basins have relatively productive plant communities with grasses in young basins yielding to sedges in medium basins. Advancing in age to old basins, distinct ponds form and mosses are found in addition to Carex. In ancient basins, the distinctive polygonal ground has formed with pronounced microtopographic rims and lows and overall vegetation growth is lower than in the younger basins. In addition, we will add to the discussion that our finding of lower SUVA<sub>350</sub> and SUVA<sub>440</sub> in DOC from old and ancient basins, is consistent with succession, soil, and landform development patterns as we would expect to see a decline in these indicators of vegetation-derived, unprocessed organic matter as the basins age and the vegetation communities become less productive. We agree these changes will improve the manuscript.*

*We are confused by one of the specific comments for the methods section: "Line 119 - Please describe what you mean by "drainages" or where were samples collected from. In soils/sediments from channels and streams or in soils adjacent to water channels? Just" We will use "watershed drainages" rather than "drainages" here in the revised manuscript. The rest of the paragraph explains from where and how the samples were collected and we are unsure what additional information you are hoping for in this first sentence. We are happy to consider additional clarifications if you can better describe what you find missing in the study area and sampling description in this paragraph.*