

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2021-272

Anonymous Referee #2

Referee 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-RC2>, 2021

General comments

The study presents porewater data for DOC/TN concentrations, SUVA values and ^{14}C dates of DOC in July and September from drainages in Alaska. Authors relate their observations with leaching of DOC during the seasonal thaw that is labile. The introduction reviews general findings from literature and mentions concepts to characterize DOM composition (aromaticity, molecular weight, aliphatic compounds-microbial processing, vegetation-derived DOM). These concepts of DOM composition need to be more clearly and directly related with the proxies that authors report (C/N ratios and SUVA values) so that the reader can follow why authors conclude about the lability of their samples.

Specific comments

Abstract

Line 23 – Specify which “biogeochemical indicators” you mean to make the statement less vague.

Line 24-25 – Based on the data and its interpretation, it is unclear how authors conclude that the old DOC is highly labile.

Introduction

Line 27 – Consider adding a more recent reference:
<https://bg.copernicus.org/articles/11/6573/2014/>

Line 41 – Consider replacing "exceeds" for "may exceed" to not overgeneralize as not all data in the cited references support DOC export being greater than NEE. For example, the study of Billet 2004 shows DOC export is greater but the data in the study of Christensen 2007 is less clear and for the study of Roulet 2007, DOC export exceeds some years but NEE is on average greater.

Lines 44-51 – The main message in this paragraph is that old DOC is labile thus relating permafrost thaw with potentially large C loss downstream. It may be also worth adding other studies that indicate that little old C via DOC seems to be mobilized or mineralized in thawing ecosystems - see references below:

<https://onlinelibrary.wiley.com/doi/epdf/10.1111/gcb.15756>

<https://www.nature.com/articles/s41467-020-15511-6>

Line 52 – Replace "melting" for "thawing", also in line 56

Line 59 – Do you mean "surface soils" here?

Line 62 – It seems the statement of DOC export increasing with streamflow is based on the spring thaw. The relation of DOC export being water rather than carbon limited is thus based on the fact that large DOC pools accumulate throughout several months in winter and then are flushed during snowmelt. As currently phrased it seems that this relation would also occur during other hydrological events such as after precipitation. Consider rephrasing as "This seems to be the current case for the Arctic as DOC export by streams and rivers largely occurs during the increase in streamflow during snowmelt, implying that DOC transport and production is water, not carbon, limited (REFS)" or alike.

Line 69 – Add "age" after "provide insights into the"

Line 74-79 – Consider adding before this last sentence that age typically increases with depth and that the flowpath associated to seasonal thaw may thus be reflected in the 14C-DOC downstream.

Line 89 – Rephrase sentence for clarity. As stated, it reads as the biodegradability decline from January to December rather than seasonally.

Line 100 – Rephrase. The part of "do not provide information about the locations within their watersheds..." is unclear

Line 99-115: Authors state their expected findings in this paragraph. The introduction has reviewed broad aspects of DOC cycling but it is hard to relate with these expected findings at the end of the paragraph. Specifically, 1) why do authors expect DOC to become more enriched in aromatics over the course of the summer? If this is related with the degree of microbial processing, it would be helpful to spell out more clearly this relation in the introduction. 2) Also, the first time that the concept of thaw lake appears is here and it seems reasonable to introduce it before to better understand the stated expectation.

Methods

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

Line 121 – This is unclear - what are internal and external drainages?

Line 145 – The statement of calculation of SUVA follows after a statement about absorption coefficients. For clarity, please state how you calculated SUVA and whether you used "absorption coefficients" (from your Equation 1) or "spectral absorbance".

Line 157 – Add "Dissolved oxygen" for DO

Line 163 – Add "significance level"

Results and discussion

Figure 2 caption – Specify units of C/N ratio: either mass (g/g) or mols (mol/mol)

Line 186-187 – What does it mean that DOC increased from July to September "in samples from the thaw table depth"? Do you mean the "deeper samples"? Rephrase for clarity.

Line 191 – Based on the data presented so far (14C-DOC, DOC concentration, DOC:DON, DON concentration), it is unclear why "undecomposed" fits in the statement. Also, it is unclear why "vegetation-derived C" fits in the statement. It is not simply "organic carbon"? Please rephrase or provide information that allows understanding how you link the measurements with those adjectives/sources.

Figure S1 and S2 – Please correct the units on the x axis of Figures S1 and S2

Line 215-217 – How do authors conclude the last part of the sentence "that has not previously undergone microbial processing and may be biolabile." The paper does not provide any experimental evidence of DOC lability. If authors want to relate their SUVA values with the degree of lability, they should describe and put in context the relation of SUVA vs lability observed in other studies. Clarifying how authors relate SUVA values with lability would help to clarify this.

Line 223 - Replace "were" for "was"

Line 226 – Explaining the potential reasons of these results and their relations is missing. In its current state, the results and correlations are presented without any further explanation. Why do CH₄ concentrations are greater in July than in Sept? Why do 14C-DOC is higher ("younger DOC") in July than in Sept? Suggestion: Is it possible that both CH₄ and 14C-DOC are related to vegetation seasonal patterns with more active vegetation in July releasing younger substrates (higher 14C-DOC) that are preferentially used resulting in CH₄ production and higher soil concentrations? This influence of vegetation could be stronger in shallow depths which could explain why these relations are only observed in samples of the top 10 cm and not deeper down (30-40cm).

Figure 4 – Correct units of x axis in panel 4a. It probably should be mM or mmol/L?

Line 247-254 – It is unclear how this paragraph adds to the story. I guess the point of this paragraph is to discuss about the dominance of hydrogenotrophic vs acetoclastic at the site but it seems all the information is based on previous studies and poorly related with the findings presented in the paper. Consider reworking this paragraph and merge it with the previous one where the alpha fractionation data is presented.

Line 271 – Decreasing DOC "concentrations" or "¹⁴C-DOC"? Please clarify

Line 273-275 – Apart from the age, can authors explain how DTBLs differ in other ecological characteristics among young, medium, old and ancient? Is there a vegetation succession during the maturing of these ecosystems? Is it commonly expected to have shallower thaw depths in younger DTBL? Such additions may help to better interpret the ecological relevance of the differences in DOC concentrations and SUVA presented in Figure 6.

Line 275 – Does Figure 6a present only "deep pore water samples"? If so, please include in the caption of Figure 6 as it currently only mentions "DOC concentration across depths", which is unclear.

Line 280 – Authors refer to "the degree of organic matter decomposition". This goes back to a previous comment - authors should more clearly state whether high or low SUVA values are associated with higher or lower lability based on previous findings. This would avoid the confusion of thinking that high SUVA results in lower lability but rather here it seems that authors relate the SUVA values with the quantity of vegetation derived C, which seems to decline with DTBL aging. Please clarify how you relate SUVA values with lability and DTBL aging in your study.

Line 296 – Replace "older DOC and younger DOC" for "old and young DOC"?