Biogeosciences Discussions

Biogeosciences Discuss., referee comment RC2
https://doi.org/10.5194/bg-2021-317-RC2, 2022

Comment on bg-2021-317

Anonymous Referee #2

Referee comment on "Dissolved organic matter characterization in soils and streams in a small coastal low-Arctic catchment" by Niek Jesse Speetjens et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-317-RC2, 2022

General Comments

This paper presents a robust dataset including field and laboratory observations to characterize organic matter dynamics and drivers of these dynamics in a small, coastal Arctic watershed underlain by permafrost. The dataset presented is a novel contribution to the science and it is clear that significant thought went into its collection and analyses to infer as much as possible about organic matter cycling from soils to the streams, including distinct types of tundra plain. While this is a novel, much-needed dataset to further inform the community on these understudied systems, significant changes are needed to make this paper suitable for publication.

Most of the material is relevant to the paper, but there is a lack of flow. A general improvement could be to introduce what is known, then highlight what is not known. For example, it is unclear what I should take away from the second last paragraph in the Introduction. What factors are specifically driving terrestrial-aquatic DOM dynamics in regions underlain by permafrost? A range of factors are provided, some specific and some not, but a cohesive framing of these dynamics is not provided. Under “historic” conditions, how would we characterize DOM flux from permafrost soils to streams? Then, under disturbed conditions, how would we characterize DOM flux from permafrost soils to streams? What do we know, and what are we still unsure of? This framing across all paragraphs should follow a relatively straight line that leads us to the purpose of your study.

The introduction would benefit from more background on IWP tundra and processes unique to these systems that impact OM cycling. The Introduction would benefit from reorganization, focusing on broad scale impacts of climate change on Arctic systems and impacts on OM cycling, then transitioning to IWP processes and OM cycling. Within this context, the reader would be more clearly led to the purpose of the study introduced in the final paragraph. Overall, it is difficult throughout the paper to understand the purpose.

Alongside this need for re-organization and focusing, the purpose of this study needs to be emphasized and mirrored in the Discussion and Conclusion. There is a recurrence of using this dataset – collected over a 10-day timeframe in a single watershed – to better understand broader dynamics occurring in the Arctic. However, there is not enough spatial or temporal coverage to justify this extrapolation.

There is significant time spent on in-stream primary production. If the dataset presented here clearly defines this contribution, that needs to be elaborated far more. In my opinion, there is not enough data here to draw clear conclusions on the role of primary production, as it doesn't appear this was directly observed.

Specific Comments

Is the definition for dissolved in this study 0.7 mm? This needs to be clarified in the Methods.

Line 52: "With raising arctic temperatures"

Remove "with" for proper sentence structure, "raising" should be "rising"

Line 60: Remove "a" before 18%

Line 88: The authors state that the middle and small catchments are exclusively underlain by permafrost, but then provide percentages of the catchments underlain by permafrost (60% and 73%). The word exclusively is incorrect and the sentence needs to be corrected.

Lines 115-116: which thermal layers and soil horizons, specifically? This information should be provided here.

Lines 118-120: There are many sentences throughout the introduction that need to be broken from a single run-on sentence to multiple sentences for clarity. This sentence in particular would benefit from splitting into: 1) The focus on LCP/HCP differences, then 2) LCP and HCP "imprints" on stream water OM composition and flux.

Line 122: Any ways to reduce verbiage will improve messaging and clarity: phrases like "circumpolar small coastal watersheds" take a fair amount of time to interpret. Does it matter that the watersheds are coastal? If so, how? Would small, polar watersheds suffice? You have already highlighted the spatial extent of small watersheds in the Arctic, so "circum" doesn't add much as a descriptor here.

Line 124: Again, while these types of watersheds cover much of the Arctic, you are only considering a single watershed. I suggest more caution in extrapolating results from a single watershed to the "circum-Arctic".

Line 146: I recommend adding: "precipitation means **are** 254 and 161 mm**, respectively**"

Line 155: Remove comma between "Both, winter". Correct future instances with this same convention used.

Line 155: Have winter and summer sea ice extent **gradually** declined? Gradual does not seem the best choice to explain long-term sea ice dynamics in the region.

2.8 DOM optical properties

This section needs to be expanded so readers have a better understanding of how samples were measured and processed. Optical properties are the basis for composition, so it's an important section. Were optical measurements made in duplicate, triplicate?

Line 276: Turned regularly – about how often? Once per day?

2.9.1

So, for each stream, a total of 9 vials were incubated? (3 vials for each time period sampled?) This should be more clearly indicated. Was there a control?

2.9.2

Were these run in triplicate again? Sampled only for DOC, or also optics?

3.2.2 First paragraph (lines 372 -378)

Since the differences in OC between active layer and permafrost are statistically significant for LCP and HCP, I would group these together and separate the description of the flat group, where active layer and permafrost were not statistically different.

Lines 393-394: The tense changes between these sentences; ensure that the same tense (either past or present) is used throughout. This happens in other instances in the manuscript.

Line 419: Swap locations of "from concentration" to read "concentration from"

Line 419-420: This sentence needs to be restructured, it is difficult to read.

Line 421: "begin" should be "beginning"

Lines 434-436: This sentence is contradictory. "Slope ratio (Sr), which negatively correlates with MW of DOM, was negatively correlated with SUVA254 in our porewater samples while Sr shows a weak positive correlation with SUVA254 (i.e. lower MW molecules were less aromatic)." What are the authors referring to for each statement?

Lines 478-479: Permafrost contribution increased 3%, active layer was constant and PP decreased by 4% (check math to ensure all changes are appropriately represented)

Line 521: "overlaying" should be "overlying"

Line 553: Fix "An explanation **is be** the presence"

Line 575: Remove "in" before "in situ"

Line 583-584: “A relatively small proportion of the total SOM pool enters the aquatic system as DOM”

Do you have a reference to back up this statement?

Line 614-619: I would be shocked if the short-term, large variability in CDOM was due to primary production. At the outlet, it seems much more likely that the coastal ocean/lagoon waters are driving variability in CDOM, which would also relate to temperature changes assuming there is a relatively large temperature gradient between stream and marine waters. This also speaks to potential issues with time series observations at the outlet. It seems there is a marine influence. How representative are these measurements of terrestrial inputs versus influenced by processes in the lagoon?

Lines 640-656: The authors need more data to identify primary productivity within streams as such a significant driver of DOC dynamics. From what is presented, this appears a hypothesis at best. If the data is there to support it, it needs to be presented in much clearer terms.

Line 663: I don’t think “hiatus” is the correct word choice here

Line 675-677: I don’t disagree with these statements, but they highlight how little is known with a 10-day sampling timeframe. This is a much-needed dataset for the region, but the conclusions being drawn seem to be overextending the meaning in the data. The data is important on its own, without trying to extend to conclusions that can’t be drawn with any significant confidence.

Line 689-690: “Hence, our data suggest that a large fraction of (labile) DOM may be utilized before reaching the stream network.”

This wasn’t abundantly clear in the Results or data provided – where is this conclusion coming from?

Line 693: “mostly weather and hydrology driven”

I agree with this, but it conflicts with earlier statements about the importance of in-stream primary productivity. It is hard to rectify how weather and hydrology control organic carbon dynamics, but portions of that cycling (i.e., CDOM) are controlled by other factors.

Line 711: “which is probably an overestimation”

I don’t think these numbers should be presented. While the data collected here is important, it covers a single system for 10 days in a single year. Any extrapolation of those results to a larger area is fraught with uncertainty. By providing this number, you are implying some confidence in it, and it is likely to be used in the future (e.g., for modeling) with the required uncertainty detached from the number.

Lines 713-715: Again, the data collected here doesn’t really support these large, general claims. It is more suitable to focus on what you observed. Expanding the relevance needs to be very careful with caveats attached.

Lines 733-735: I don’t think the reference to negative priming is relevant here.

Line 742: “Moreover, there is a simple need to map these watersheds at the basis of the aforementioned challenge.”

This sentence is unclear.

Line 748: “These techniques together with standardization of methods are there for recommended for a harmonized approach on understanding lateral permafrost-OM-dynamics.”

This sentence is unclear.

Line 768: I wouldn’t define a 10-day sampling window as “long-term”

Line 780-782: There is no discussion of remote sensing throughout the paper, only a single mention of it. And really, remote sensing tools aren’t suitable for observing stream OM dynamics – the spatial scale is too fine. I recommend removing this sentence.

Figure 1

The contrast in the upper panel needs to be improved, and it appears that the inlay

indicated in the top panel (black box) does not correspond to the area shown in the lower panel.

Figure 5

This figure would be improved by showing change in dC13 to correspond to changes in DOC shown on the left

Table 1

While the summary is appreciated, I recommend removing this table based on the length of the paper and presence of this information in Methods and references therein.