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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Assessing inter-annual and seasonal patterns of DOC and DOM quality across a complex alpine watershed underlain by discontinuous permafrost in Yukon, Canada" *by* Nadine J. Shatilla and Sean Carey

Anonymous Referee #2

Received and published: 19 April 2019

Overall, I find that the authors provide useful data to help bolster our understanding of carbon cycling in northern regions. However, there were some areas of concern for me: the change in DOC concentrations documented between older studies and the present day was really quite striking; I would have liked to have seen more attention paid to ruling out analytical error. Several of the SUVA measurements were also quite high, suggesting possible iron interference. If Fe value are available for these sites, it would be useful to confirm whether these concentrations may have affected overall values, or change across seasons.

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In addition to these analytical points, I found myself looking for the manuscript to delve into the data a bit further, to add to our process-based understanding of seasonal variation in DOC dynamics in sub-Arctic regions. Examining seasonal variation in C-Q plots, or plots of the various optical metrics might be one good way to do this. Making better use of what sounds to be a rich historical dataset seems like it would also be worthwhile. Finally, the authors discuss the high fall flows that are unique to these study years. While it is interesting that concentration does not change during this time, it would have been nice to have seen an assessment of this effect on overall C export: do these fall flows have a substantial effect on export from the catchment. It is not that surprising that concentration and measures of aromaticity increase during the spring freshet (as the authors point out!), but this becomes one of the main take-homes of the manuscript, as currently structured. It would be nice to see this rich dataset used in a slightly more nuanced way.

Finally, I would recommend some work to create slightly higher quality figures for publication.

Specific comments:

L10: Reference to large Arctic rivers seems mis-placed for this manuscript focused on small stream processes?

L42: Note that Striegl et al. document declines in flow weighted concentration, rather than overall flux. DOC flux was still documented to increase. Also, Frey and McClelland (2009; cited later in this work) and some other authors provide some discussion on why regions might vary in this way.

L61: An obvious point, but it's the combination of high concentration and high flow that causes these high exports to occur; perhaps tweak your sentence here slightly for clarity? In addition, substantially elevated concentrations during the freshet are not necessarily going to occur, even if this is the typical response in western Canada. See, for example Li Yung Lung et al. 2018.

L64-65: Again, this is likely to be region specific, and depend upon the soil profile.

L85: Kokelj reference might be mis-placed here? It is an excellent study, but does not examine DOM.

L91: Consider adding a reference for this statement.

L99: Note two different tenses being used in this sentence.

L109: Some edits needed in this sentence for grammar; switch between passive and active wording about half way through.

L151: Clarify location of the three long-term weather stations.

L187: Note that SUVA is based on absorbance, rather than fluorescence, so the use of 'fluorescence indices' here is technically incorrect. Switch to 'optical indices'?.

L191: Values as high as 6 (typically, anything over $\sim$4.5) almost certainly indicates interference from Fe. See Poulain et al. 2014 ES&T for more information on correction procedures, etc.

L195: Typically, terrestrially-derived DOM would have SUVA values that are higher, rather than lower; perhaps tweak the text inside the bracket? I think you're referring to modified terrestrial DOM here, but this is not necessarily clear from how the parenthetical text is worded.

L233 "resulted"

L234: Rework this sentence for clarity. It's also unclear to me what the nearby weather station is / where it is located, particularly in the context of Figure 2.

L247: Is it possible to display this long-term average, to give the reader a comparison point? (i.e., perhaps move up Figure 6).

L272: As worded, a bit repetitive with previous text (ie, from the site description).

L299: See previous comment re: tweak to your terminology here; note that SUVA is an

absorbance-based metric.

L304: As mentioned previously, these values are somewhat high. Do you have any corresponding Fe data that might help to get a sense of Fe interference? Plot DOM quality indices against Q, rather than time series (or, as a compliment to time series)? Would this help to think about process?

Figure 4: Similar to Figure 5, why not use different shadings of grey for BB and GC? This would allow the reader to differentiate between these catchments (or, see the of overlap between catchments within a landscape type) if they wish.

L372: Certainly, this will be true in systems that switch from baseflow sourced from deeper mineral layers to flow during the freshet that is sourced from organic-rich surface layers (similar to the large river studies being referred to here). But, I'm not sure it will necessarily be universal.

L386: See also Creed et al. (Can J. Fish Aquat. Sci) on this point.

L390: Is this export estimate a unique calculation for this paper, or taken from elsewhere? If the former, then some text in the methods and results should be included. If the latter, then a reference is warranted.

L391: Given the last sentence, I'm unclear on whether DOC concentration or export is being referred to here.

L394: For clarity, it would be useful to refer specifically to "DOC concentration"

L396: While it's somewhat self-evident that wetlands will increase DOC concentration across integrated scales, the residence time of lakes (and, associated biological / photochemical processing) will often cause outlet DOC concentrations to be lower than inlet concentrations. Can you provide data to support the statement that the lake might have increased integrated [DOC] at the outlet?

L399: However, if flow was substantial, this period may have been important for overall

flux, even in the absence of a change in concentration?

L402: There is some literature to support this assertion: ie, that the pool of OM will build up during dry periods, or periods when decomposition is not possible (ie, winter). This material then becomes available for flushing at the onset of rains and/or thaw. It might be useful to reference some of these studies here.

L406: It would be really nice to see a plot of this data, and to see this used for a deeper consideration of process-level effects. Does a C-Q plot show distinct seasonal patterns, for example? Swapping Q for runoff in a plot would allow comparison of multiple panels from different watershed components, which could be very instructive.

L415: Similar to the comment above, it would be nice to see this data plotted, so that the reader could understand the magnitude of the effect.

L417: Agreed – this is a very big difference! Were early samples properly acidified to remove inorganic carbon during analyses? Also, please tweak the sentence to clarify whether in all cases you're referring to concentrations measured at GC.

Section 4.2: As worded, this section is a bit repetitive with the results section. Consider tweaking to move away from a restatement of the results, and more towards a discussion of what these optical indices can tell you about process. Also consider cutting much of the first paragraph, where you discuss the inability to validate the PARAFAC model. If the model is not to be included, then perhaps it is best to omit its discussion from the paper?

L475: If this is well documented, this seems like an excellent opportunity to take this data and generate a more process-level understanding of DOM generation. Can knowledge of flowpaths be more directly tied to the DOC and DOM patterns being observed to more directly discuss OM generation at the sub-catchment and catchment outlet?

L481: Note that this is in agreement with expectations from the literature. See, for example Creed et al. CJFAS (as above).

L498: To me, the change that might be expected in this catchment with permafrost thaw is not clear from the text that precedes. Most work from permafrost regions is now suggesting that biodegradeability, at least, should increase with thaw, although it's not clear that this change will be visible at even the sub-catchment outlet scale (see, for example, work by Spencer and others). Declines in concentration are presumably more related to soil profiles than the presence or absence of thermokarst.

L503: Effects of late summer / fall precipitation. I agree that this is an understudied, and worthwhile avenue of investigation. Can you pull out this section with more clarity? For example, it would be nice to see a more quantitative examination of effects on export – surely if discharge is increasing substantially, export is also affected? Is it possible to calculate overall effect on export?

L520: Again, I do wonder about these DOC values. Any chance you have some old, preserved (or frozen) samples that could be re-analyzed? A difference of $\sim$10 mg/L is substantial, and an obvious culprit is a lack of full removal of inorganics (ie, bicarbonates) from the sample during processing.

L537: Try to avoid single sentence paragraphs.

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