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Interactive comment on "First Pan-Arctic Assessment of Dissolved Organic Carbon in Permafrost-Region Lakes" *by* Lydia Stolpmann et al.

Anonymous Referee #2

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The manuscript presents a newly assembled, Global dataset of DOC concentrations from northern lakes. It is a very nice dataset that is clearly valuable. The mapping of patterns of DOC throughout figures 1 to 3 and tables 1 to 2 are also very useful context for readers. On this basis, I feel the manuscript has great potential to make an important contribution to the field. However, while the dataset is powerful, and geographic patterns are interesting, the paper is lacking in mechanistic insight, with a number of important concepts overlooked. These issues should be resolved before the manuscript could be considered fit for publication. I outline my concerns below, and hope that the authors find this evaluation constructive and useful.

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General comments:

1. I think that the core message of the paper needs to change from one of strongly predicting lake DOC, to stating that predicting DOC patterns at the Global scale is complex, and has resulted in weak relationships with the individual predictors at hand. Throughout, the authors claim that lake DOC strongly depends on environmental properties. I agree, yet I don't think that the results presented here have led to this conclusion. Disregarding p values (which are uninformative due to their dependence on sample size), the correlation strength for every environmental parameter versus DOC is weak, and weaker than those for basic categorical groupings (region/zone) and latitude, which themselves are marginal in strength. The fact that these categorical and spatial variables remain stronger predictors than environmental measures (ice content, ground type, soil C content) tells us that the major drivers of lake DOC are not captured in this dataset (see next point on which predictors I mean). That isn't surprising, and speaks to the complex regulation of lake DOC concentrations. To fix this problem, I think that the messaging of the paper needs to change to emphasize the weakness of the strength of these individual predictors, and the complex control of lake DOC content has to be discussed throughout and emphasized. Throughout, I feel that readers are presented with an over-simplified view of the regulation of DOC concentrations.

2. The paper overlooks the most important mechanisms controlling DOC cycling, i.e., the roles of hydrology and geomorphology in structuring the delivery of DOC to lakes, and water residence time and allochthonous DOC processing. This may be a main reason for such weak relationships presented in their correlation table. Many other studies show that these factors critically shape lake DOC cycling, and without this information brought in to the analyses, the authors are likely missing a big part of the mechanistic story. For instance, the first paragraph of the discussion talks about vegetation density, soil C contact with water, and the effects of permafrost extent on vegetation. It really overlooks these major factors that are very much a part of that story, thereby making the mechanistic explanations in the paper incomplete. To fix this, the authors could im-

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prove the analyses by bringing in new datasets on hydrology, topography (catchment slope), and estimates of water residence time (even for a subset of the lakes) to explore this. At the very least, the importance of these factors needs to be better incorporated into the narrative of the paper, from start to finish.

3. The paper overlooks the role of autochthonous DOC production, and its importance in structuring patterns of DOC within and among regions. It is clear from past work using a bunch of different approaches in different regions, that the sources and composition of DOC ranges widely, and autochthonous sources can be very important in many lakes (e.g., Tank et al. 2011 L&O; Osburn et al. 2017 JGR-B; Osburn et al. 2019 L&O Lett.; Johnston et al. 2020 L&O). A lot of the high-DOC lakes in some regions are indeed rich in autochthonous DOC, so this source input may drive some of the most extreme observations in the current dataset, especially in the Yukon Flats region the authors highlight, which is discussed in that Johnston et al. 2020 paper. The importance is probably lower in some regions than others, and depends on hydrologic connections to the terrestrial landscape and other factors (getting back to comment 2 above). Overall, this is an important factor structuring lake DOC patterns that should at the very least be incorporated throughout the paper, where mechanistic inferences are made. To go a step further, the authors could look at a subset of the lakes for which published organic matter properties are available (optical, isotopic, elemental ratios, or other, depending on what is out there).

Specific comments: P1/L32 – How does this relationship compare, specifically?

P1/L34 – I don't think you demonstrate a strong dependence here, see general comment 1.

P2/L4 – abbreviate carbon as C after first mention. Comment applies throughout.

P2/L11 – regional warming: is this air or soil temperature? Be specific.

P2/L28 – Which lake-based process, specifically?

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P2/L30 – Last sentence needs improving: Which flux? Lakes, streams, what? The paragraph could also be improved by adding a conclusion sentence identifying what the major knowledge gap is here.

P3/L1-9 – This paragraph is somewhat off topic. What does it have to do with lake DOC? Either revise it to link to the topic, or cut it out.

P3/L18 – Lake number or lake area?

P3/L25 – Throughout: Cold/Very Cold/Cool – give us some meteorological values to help understand what these designations mean.

P7/L21 – This section provides little insight. As it is a paper on global patterns, the seasonality seems off topic. What's more, there is no context to evaluate DOC patterns in a meaningful way (which lakes, which regions, etc.). Consider removing this.

P8/L7 – Where is table A2? Why not cite Fig. 2 & 3 with statistics reported to confirm this statement. Comment applies throughout paper about table A2.

P10/L1 – Instead of fig. 2a, why not cite Fig 3 here? Makes the point better.

P10/L5-6 – Why a new paragraph? Same topic/theme.

P11 – Fig. 3 – Where are the statistics? ANOVA and post hoc tests for each panel?

P12/L14-16 – Adding scatterplots and show us the data – the table is good but not enough.

P12/L24 – Does the study provide this insight? I don't think so, since the relationships are quite weak. See general comments for suggestions to improve this section.

P13/L4 – Should the word 'catchment' be plural?

P13/L7 – What about the effect on hydrologic connectivity caused by permafrost extent? Not just terrestrial veg distribution, but also consider the role of permafrost in disconnecting lake surface waters from hydrologic flowpaths that deliver DOC.

P13/L9 – Odd conclusion sentence. Seems unrelated. Consider revising.

P13/L12 – Weak cross-regional correlations don't really tell us about climate change impacts. Consider revising.

P13/L13 – The discussion throughout this last paragraph assumes all DOC comes from terrestrial environments. Not accurate. See general comment #3.

P13/L20 – "Our results" – be specific here and say which results.

P13/L20 to 21 – You do not have the data to infer this mechanism. Revise language here. Also, I do not think that this conclusion is consistent with the comparison of DOC concentrations by deposit type categories in Fig. 3. Yedoma type is not the highest DOC concentration there.

P13/L23-24 – You can't assume this without hydrologic or other information.

P13/L25 – This could be revised to be more mechanistically insightful. Wouldn't we expect lower delivery of allochthonous DOC to these lakes where permafrost limits hydrologic exchange into lakes from land?

P13/L25-26 – Why just photodegradation without mentioning respiration. Both are important.

P14/L1-2 – Cite appropriate references here (Johnston et al. 2020; also Bogard et al. 2019 Nature Geoscience). Also, as discussed in those papers, elevated DOC in many lakes is due to intense autotrophic inputs.

P14/L5 – Elevation and catchment slope would be an easy factor to add. See general comments. This exploration would boost the mechanistic insight in the paper.

P14/L12 – Some studies in the reference list have done this and could be explored.

Some useful references:

Osburn, C.L., Anderson, N.J., Leng, M.J., Barry, C.D. and Whiteford, E.J., 2019. Stable

isotopes reveal independent carbon pools across an Arctic hydroâĂŘclimatic gradient: Implications for the fate of carbon in warmer and drier conditions. Limnology and Oceanography Letters, 4(6), pp.205-213.

Tank, Suzanne E., Lance FW Lesack, Jolie AL Gareis, Christopher L. Osburn, and Ray H. Hesslein. Multiple tracers demonstrate distinct sources of dissolved organic matter to lakes of the Mackenzie Delta, western Canadian Arctic. Limnology and Oceanography 56, no. 4 (2011): 1297-1309.

Bogard, M.J., Kuhn, C.D., Johnston, S.E., Striegl, R.G., Holtgrieve, G.W., Dornblaser, M.M., Spencer, R.G., Wickland, K.P. and Butman, D.E., 2019. Negligible cycling of terrestrial carbon in many lakes of the arid circumpolar landscape. Nature Geoscience, 12(3), pp.180-185.

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