

Detailed review replies of

First Pan-Arctic Assessment of Dissolved Organic Carbon in Permafrost-Region Lakes

Lydia Stolpmann^{1,2}, Caroline Coch^{1,2,3}, Anne Morgenstern¹, Julia Boike^{1,4}, Michael Fritz¹, Ulrike Herzs Schuh^{1,2,5}, Kathleen Stoof-Leichsenring¹, Yury Dvornikov⁶, Birgit Heim¹, Josefine Lenz^{1,7}, Amy Larsen⁸, Katey Walter Anthony⁷, Benjamin Jones⁹, Karen Frey¹⁰, and Guido Grosse^{1,2}

¹Alfred Wegener Institute, Helmholtz Centre for Polar and Marine Research, Potsdam, Germany

²Institute of Geosciences, University of Potsdam, Potsdam, Germany

³World Wildlife Fund, The Living Planet Centre, Rufford House, Brewery Road, Working, Surrey, GU214LL

⁴Geography Department, Humboldt-Universität zu Berlin, Germany

⁵Institute of Biochemistry and Biology, University of Potsdam, Potsdam, Germany

⁶Agrarian-Technological Institute, Peoples' Friendship University of Russia, Moscow, 117198, Russia

⁷Water and Environmental Research Center, University of Alaska, Fairbanks, AK, USA 99775

⁸Yukon-Charley Rivers National Preserve and Gates of the Arctic National Park and Preserve, National Park Service, Fairbanks, AK, USA

⁹Institute of Northern Engineering, University of Alaska, Fairbanks, AK, USA

¹⁰Graduate School of Geography, Clark University, Worcester, MA, USA

Correspondence to: Lydia Stolpmann (Lydia.Stolpmann@awi.de)

Interactive comment on

“First Pan-Arctic Assessment of Dissolved Organic Carbon in Permafrost-Region Lakes” by Lydia Stolpmann et al.

Anonymous Referee #2

Received and published: 7 January 2021

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.

[Thank you very much for the appreciation of our dataset and the constructive comments.](#)

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.

We would like to emphasize that our objectives were to synthesize existing lake DOC data to provide a pan-Arctic dataset, and to present what we conclude from this dataset. Our synthesis shows how important local studies of single lake catchment are to explain the complex mechanisms of regulating lake DOC. This dataset should be basis for further investigations. We agree that the correlations for our environmental parameters and the lake DOC concentration are weak but we wanted to highlight the strongest relationships in our dataset, even if these are rather weak. However, we also agree that it is necessary to highlight that these relationships are weak and that we need to describe and discuss the complexity of regulating DOC concentration in lakes more in detail in the discussion. Therefore we added an additional subsection to the discussion, we attached here:

“5.3 The complexity of lake DOC regulation

The analysis with our dataset with the available pan-Arctic data have shown the strongest significant relationships between ecoregion and lake DOC concentration as well as between geographical latitude and DOC concentration and permafrost extent and DOC concentration, even if these relationships are generally weak. Recent studies present more parameters influencing lake DOC concentration. Xenopoulos et al. (2003) analysed catchment characteristics of lakes and found that lake perimeter and the proportion of watershed occupied wetlands are strongest predictors for DOC in lakes of the temperate forest. On a global scale, the proportion of watershed occupied wetlands and lake elevation are strongest predictors for lake DOC. Tranvik et al. (2009) described that lake area, which is connected to lake volume and water retention time, might be negatively correlated in regional studies but that it is not an important DOC predictor in a global view. The fact that the majority of predictors for lake DOC differ in regions demonstrate the complexity of the regulation of DOC concentration in lakes. However, these parameter are not included in our study, which could cause the weak relationships of our analysis. Also not included in our analysis due to limited data availability is the hydrological connectivity. Therefore, less allochthonous (i.e. produced in the lake catchment) DOC is transported to hydrologically isolated lakes (Bogard et al., 2019). Instead,

those lakes are characterized by autotrophic C inputs by fixing atmospheric CO₂ (Bogard et al., 2019). In these lakes, such as in the Yukon Flats, also evapoconcentration plays an important role (Johnston et al., 2020). In addition to allochthonous DOC, autochthonous (i.e. produced in the lake) DOC is influencing the DOC concentration, especially in lakes with low connectivity. Autochthonous DOC includes phytoplankton productivity as well as heterotrophic bacterioplankton respiration processes (Chupakov et al., 2017).

The influence of hydrological, climatic and topographical parameter on the DOC concentration of a lake is very complex. Whereas our pan-Arctic dataset provides first insights of the relationship between environmental parameter and lake DOC concentration, regional studies are necessary to understand these complex mechanisms and to determine DOC predictors, which are differing regionally. In addition to the described terrestrial DOC predictors, newest”

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 improve 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.

We agree that we need to clarify that important aspects for the regulation of the DOC concentration, such as hydrology and topography, were not included in our pan-Arctic analysis and that this has an influence on the results. Recent studies on lake size distribution were inconclusive (Muster et al., 2019) and statistical analysis showed that the lake size distribution could not be explained by hydrological variables such as TDD s, evapotranspiration, precipitation, permafrost extent, glaciation history, or broad-scale topographic parameters (i.e., elevation and slope). At the moment, we cannot subject this aspect to an additional analysis, as this would go beyond the scope of the current project possibilities. But we included the description of the role of these important factors in our paper, especially in the discussion subsection 5.3 (see above), and incorporated them into the conclusions as an outlook for further exciting work.

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).

We agree that the role of autochthonous DOC production should also be examined and discussed, which we have added in our paper at subsection 5.3 (see above).

Specific comments:

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

In this sentence we refer to our results regarding analyzing the relationship between lake DOC concentration and soil organic carbon content. In the results subsection 4.7 we described that our analysis result in a weak significant relationship between lake DOC and SOCC of the upper 100 cm ($p = 0.1$; $p < 0.05$). Analysing the relationship of lake DOC and SOCC in the upper 300 cm we found a weaker significant relationship ($p = 0.09$; $p < 0.05$). In comparison, Sobek et al. (2007) assumed soil carbon content as a strong predictor for lake DOC.

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

We deleted 'strongly' in the sentence.

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

We changed it accordingly.

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

We specified in the manuscript that there was found a permafrost warming driven by higher air temperatures:

“More recently, the permafrost warmed globally by an average of $0.29^{\circ} \pm 0.12^{\circ}$ C over the 2007-2016 period due to higher air temperatures, with some of the strongest warming trend (about 0.9° C per decade) measured in individual boreholes in Siberia (Biskaborn et al., 2019).”

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

Here we mean the mineralization by photo oxidation or microbial activity, resulting in CO₂ and CH₄ emission. We clarify and changed the sentence to:

“The mineralization of DOC in a lake is a major component of the global carbon cycle and contributes to the greenhouse effect (Finlay et al., 2006). Vonk et al. (2015) suggested that this

form of carbon flux represents roughly one third to one-half of the net carbon exchange from land to the atmosphere in the Arctic.”

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.

In this paragraph we want to highlight the importance of analyzing DOC in lakes because lake-based process, such as mineralization by photo oxidation or microbial activity, are a major component of the global carbon cycle and contributes to the greenhouse effect (Finlay et al., 2006). To clarify the understanding of the sentence we revised it:

“Vonk et al. (2015) suggested that this form of carbon flux, comprising the carbon flux from surface waters to the atmosphere and from land to ocean, represents roughly one third to one-half of the net carbon exchange from land to the atmosphere in the Arctic.”

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.

Thank you for this remark. We agree that this sentence about riverine DOC does not fit here and decided to cut the sentence out.

P3/L18 – Lake number or lake area?

Here we mean 10 % of the lake number of 7,500. To clarify that we also here referring to the study of Sobek et al. (2007), we added the reference to the end of the sentence.

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

Thank you for this comment, which makes clear that the constant repetition of ‘cold/very cold/cool’ does not highlight climatic differences between the study areas. We revised the section by cutting the climate classifications in each study area paragraph and by including the climate classification at the beginning of the section as follows:

“In our synthesis, we included 2,167 samples from 1,833 lakes of 13 study areas (22 sites) across the Arctic, sampled from year 1979 to 2017 (Table 1). The study areas of our dataset are dominated by tundra climate and very cold subarctic climate. The Nunavut study area is also characterized by cool continental climate.”

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.

We agree that the section provides only little insights. This was also criticized by the first reviewer. For this reason we have expanded this section. Enclose you will find the additions we made. We added Table A2 to the Appendix and the following to subchapter 4.1:

“In our dataset we found six lakes that were sampled three times over the same period. These six lakes are located in Qeqqata on Greenland and were sampled in April, June and August. For five of these six lakes, the highest DOC concentration of the respective sampling series was found for April samples. Then, the DOC concentration decreased until sampling in June and increased until sampling in (Tab. 2). For these lakes, we observed a 30 % to 45 % higher DOC concentration in April and up to 25 % higher DOC concentration in August in comparison to the June sampling and therefore shows a seasonal variability. This variability we checked for a larger dataset. Therefore, we used data from the study areas Southcentral and Interior Alaska since samples in these areas were available for each month from May to September. However, these samples include all samples of these study areas and not a sample series of a single lake. We focused on the median DOC concentration for each month for each of the two study areas. For Southcentral Alaska we found a pattern similar to that in the lakes in Qeqqata. We found a 17% higher DOC concentration in lakes that were sampled in May and September compared to lakes that were sampled in July. However, the same pattern cannot be seen for Interior Alaska (Table A2).

In order to make a more precise comparison with the Qeqqata lakes, we compared samples of the whole dataset from the months June and August. For these months, in addition to the Qeqqata samples, samples from the study areas Yamalo-Nenets A.R., North and Northwest Alaska, Southcentral and Interior Alaska were available. In three of the four study areas we also found higher DOC concentrations in August compared to the June samples like for the Qeqqata lakes.”

Table 1: DOC concentration of six lakes (Qeqqata, Greenland) sampled three times in one year.

Lake name	DOC concentration [mg L ⁻¹]		
	April	June	August
SS906	8	5.2	5.8
SS1381	39.2	24	31.3
SS2	35.1	23.8	27.7
SS8	52.5	28.7	38.3
SS904	8.1	5.2	5.8
SS1590	31.1	36.6	25

Table A2: Overview of median DOC concentration for sampling months. Includes those samples that could be assigned to a sampling month unambiguously.

Study area		Median DOC concentration [mg L ⁻¹]						
		April	May	June	July	August	September	October
Greenland	Qeqqata	33.1		10.2		25		
Siberia	Yamalo-Nenets Autonomous Region			18.1	29.6	13.9		
	Khanty-Mansi Autonomous Region				14.1	10.9		
	Chukotka Autonomous Region				9.6			
	Krasnoyarsk Krai				8.3	9.8		
	Sakha Republic (Yakutia)				8.1	10.2	7.8	15.3
Canada	Yukon				12.9			
	Northwest Territories				8.5		13	
	Nunavut				2.2	3.8	2.6	
Alaska	North and Northwest			4.5	9.1	8.2	8.1	
	Southcentral		15	13.6	12.5	16.3	15	
	Interior		12.7	18.4	12.6	20.3	21.3	

We also added this point to the discussion subchapter 5.4 Challenges of a pan-Arctic assessment, describing possible causes of the seasonal variability found in a small scale single lake analysis and similar patterns analyzing multiple lakes per month. Additionally, we name the lack of data to make a more solid statement about seasonal variability and that continuous sampling during the ice-free period is needed.

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.

Table A2 contains the exact DOC concentration ranges, median DOC concentration values and mean values for each individual study site in the various study areas. However, since this table is very large, we decided to move it to the appendix. In this sentence, we refer to table A2 because it provides exact values as differences between the sites. However, we agree that the different DOC concentrations in the study sites are also very well demonstrated in Figures 2 and 3 and that we should also refer to them here. That's why we added:

“We found differences between the four regions of Alaska, Canada, Greenland and Siberia, as well as between study areas and study sites within these regions (Fig. 2, Fig. 3, Table A2).”

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

We agree that Figure 3 highlights the variability of median DOC concentration in the study regions on a better way. We changed it accordingly.

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

Thank you very much for this comment. We changed it accordingly.

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

For our statistical analysis we first used the Shapiro–Wilk normality for testing whether our data is normality distributed. But our data does not follow a normal distribution. So, we did not use the ANOVA but the Spearman rank correlation coefficient (ρ) to measure the relationship between two variables and the Wilcoxon-Mann-Whitney test to determine the difference in means between two populations. We described this on page 7 in the subsection 3.3 Statistical Analysis.

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

We added the following scatterplot to this paragraph to better visualize the data of lake DOC concentration and soil organic carbon content.

Additionally, we would like to notice that the entire dataset is in progress for publication in PANGAEA.

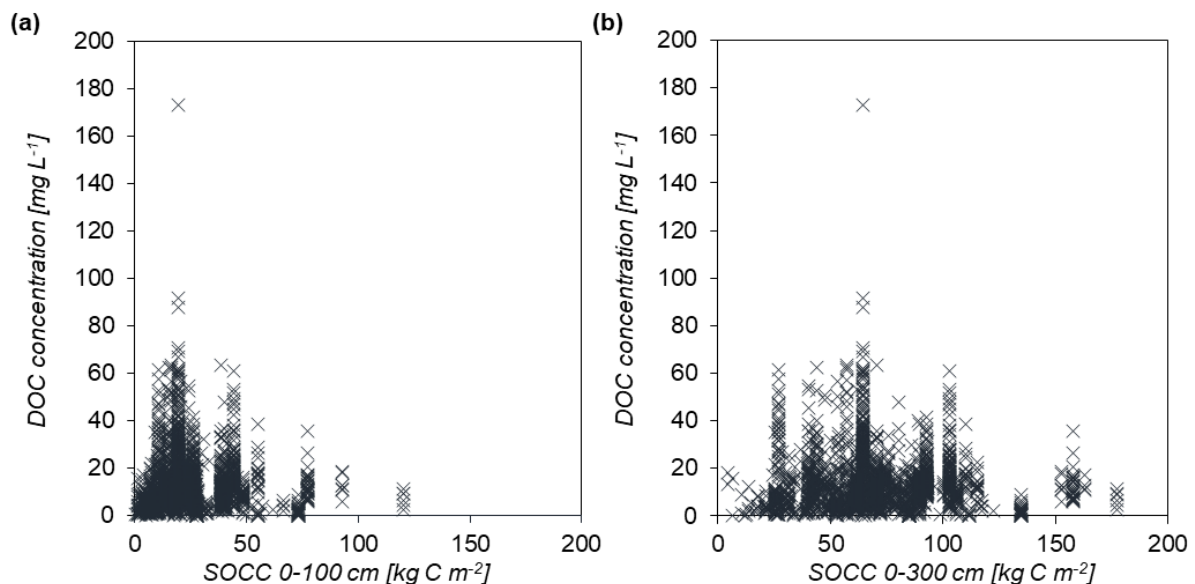


Figure 4: Scatterplots for lake DOC concentration and lake surrounding soil organic carbon content (SOCC) in a depth of 0 to 100 cm (a) and in a depth of 0 to 300 cm (b). To better visualize the relationship of both parameter we limited the y-axis to 200 mg L⁻¹. So, three lakes with the DOC concentrations of 433 mg L⁻¹, 507 mg L⁻¹ and 1,130 mg L⁻¹ are not included in this plot.

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.

We agree that we had to improve the insights of potential sources of DOC in pan-Arctic lakes of the permafrost region. With the help of the reviewers comment we were able to strengthen the manuscript and to better provide this insights, especially by extending the discussion section. Please find the improvements in the following comments and in the response to the first general comment.

P13/L4 – Should the word ‘catchment’ be plural?

Thank you very much for this comment. We changed it accordingly.

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.

We agree that hydrological and carbon cycles are closely linked, which has been stated in previous publications (Vonk et al., 2015; Beel et al., 2020). We did not analyse the hydrological connectivity (both vertical and lateral) in this paper, but added the importance of the hydrology in the discussion. Additionally, we agree to mention the hydrological connectivity here and extended the sentence.

“In contrast, higher permafrost extent in higher geographical latitudes results in lower vegetation density, lakes are less connected and thereby hydrologically isolated leading to lower DOC concentrations”.

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

We agree that this sentence about riverine DOC sources is unrelated in this paragraph about the relationship between lake DOC and ecoregion. Therefore, we decided to cut the sentence out.

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

We revised this part to clarify our point:

"Climate change affects ecosystem structure which in turn may impact lake DOC concentrations and associated biogeochemical fluxes (Sobek et al., 2005). For example, enhanced DOC concentrations in a lake provide an increased basis for the mineralization of DOC through photo oxidation and by microbial activities, which may result in higher CO₂ emissions from these lakes. In our first pan-Arctic assessment of DOC in lakes of the permafrost region we found that DOC concentrations in lakes become significantly higher when transitioning from tundra zone to the tundra-boreal transition zone to the boreal zone. In addition, DOC concentrations are overall lower in permafrost zones that are less continuous. Both trends suggest that climate change, projected to result in expansion of boreal zone into the tundra zone and a decrease in permafrost continuity will likely result in higher DOC concentrations in lakes of these regions. Moreover, the shift of the boreal forest might lead to changes in the hydrological connectivity to more connected lakes and further might lead to an

increase of allochthonous DOC in lakes. This may then also result in higher CO₂ fluxes from pan-Arctic lakes."

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

We agree that not all DOC comes from terrestrial environments. In addition to the new subsection 5.3 where we discussed the complexity of lake DOC regulation, we revised the paragraph you find above at the previous comment.

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

We specified and changed it accordingly.

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.

It is right that we first considered the DOC concentrations in all occurring deposit types of our dataset. Here we found the highest DOC concentrations in lakes surrounded by mountain alluvium, glacio-lacustrine and glacial deposits and not in lakes surrounded by yedoma deposits (Fig. 3). Then, we focused on yedoma lakes and non-yedoma lakes, including all lakes that were not assigned to yedoma deposits. For yedoma lakes we found a median DOC concentration of 11.8 mg L⁻¹, comprising 16 % of the datasets lakes. This is significantly higher than the median DOC concentration we found for non-yedoma lakes with 10.3 mg L⁻¹, comprising 79 % of the datasets lakes (section 4.5 of the manuscript). To clarify our point we revised this part:

“The comparison of the DOC concentration in lakes in the yedoma region and the DOC concentration in lakes in non-yedoma regions, comprising all other lakes of our dataset, results in significantly higher DOC concentrations in yedoma lakes compared to non-yedoma lakes. This might be attributed to the mobilization of old labile yedoma carbon by thermos-erosion along rapidly expanding lake shores and thermokarst processes, which (Strauss et al., 2017). Nevertheless, the partial dataset of non-yedoma lakes comprises 79% of lakes in our dataset, including lakes surrounded by different deposit types and lakes of highest DOC concentration in our dataset.”

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.

We agree with the last three comments that in this paragraph more explanation and discussion is needed and we decided to revise the sentences on P13/L23 to 26 as follows:

“We assume that yedoma lake generation is influencing yedoma lake DOC. The formation of yedoma lakes, due to deep thermokarst subsidence, results in deep and often closed basins

(Morgenstern et al., 2011). As result of the missing lake connectivity, DOC is locked in the lake, originating partially from eroding organic-rich yedoma deposits (Strauss et al., 2017) and melting yedoma ice wedges (Fritz et al., 2015). Further, the lacking lake connectivity might prevent flushing of yedoma thermokarst lake waters with river water and snowmelt water. Hence, we assume that yedoma thermokarst lakes are more likely to have elevated DOC concentrations than other more connected lakes as well as mixed larger and shallower lakes, where photodegradation and respiration plays an important role, are associated with lower lake DOC concentration.”

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.

We agree that the suggested references need to be mentioned here. We revised as follows:

“For our database, this connection may explain the relatively high DOC concentrations of lakes in the Yukon basin. Here, the lakes are less hydrologically connected and the region is very arid. Therefore, evaporation can concentrate DOC (Johnston et al., 2020)”.

Additionally, we added the aspect of the autotrophic carbon input described by Bogard et al. (2019) in the discussion section 5.3.

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.

Thank you very much for suggesting this here but at the moment we cannot subject this aspect to an additional analysis, as this would go beyond the scope of the current project possibilities. We want to mention that our dataset provides a basis for further analysis. We plan to extend the dataset to conduct more analysis and for example use parameter such as elevation or catchment slope.

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

We agree that there have been studies carried out in lake areas which are remote in the Arctic. Here we want to highlight that in particular multitemporal sampling of those especially remote lakes is logistically challenging but necessary to investigate the seasonal variability of lake DOC in different regions.

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 hydroclimatic 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.

[Thank you very much for adding this useful publications.](#)