Response to Anonymous Reviewer 1 - egusphere-2023-1792. Author responses added in blue.

Reviewer 1:

> *"The study by Keskitalo et al. aims to decipher carbon sources and their drivers in an Arctic river and its tributaries. The authors focus on particulate organic carbon (POC) and are particularly interested in differences between two seasons, spring freshet and summer, as well as differences among different sizes of streams/rivers and the mainstem of the Kolyma River in Siberia. The authors found differences in POC sources between seasons and in small tributaries compared to larger streams/rivers. With this study, the authors want to contribute to a better understanding of the carbon dynamics of the often overlooked smaller tributaries in the Arctic and their sensitivity to climatic stressors. The study provides important results for a better understanding of streams/rivers of different sizes, their carbon dynamics, and their sensitivity to climate warming. Nevertheless, I have questions about the data and the statistical analyses of the data that I would like the authors to address. Please find my comments in the order they appear in the manuscript below."*

Thank you for your review and positive response to our manuscript. We think the reviewer comments have helped to improve this manuscript and we address them one by one below.

1. ***Title:*** *I like that the authors formulated subheadings in the discussion that summarize the main findings. However, the title of the manuscript is very descriptive stating that the study is about "Seasonal carbon dynamics of the Kolyma River tributaries, Siberia". I would like to ask the authors to think about rephrasing the title and summarize the major result(s) and better highlight the focus of the study, the POC. Furthermore, the authors do not only report seasonal dynamics but also look at the role of the smaller sized streams compared to the mainstem of the river. A well-summarized title might also encourage more readers to look at the article.*

We agree that the title could be more detailed and suggest here a new more descriptive title for the manuscript (addition in *italics*): "Seasonal *particulate organic* carbon dynamics of the Kolyma River tributaries, Siberia".

2. ***Last sentence of the abstract*** *(lines 20-21): "As Arctic warming and hydrologic changes may increase OC transfer from smaller waterways through river networks this may intensify inland water carbon outgassing." I do not see the link between carbon transfer and outgassing. I miss the conversion of carbon, i.e. decomposition in the system that leads to higher CO_2 production through respiration, which can then cause a higher outgassing rate than at other times. I also do not believe that an earlier onset of primary production in tributaries compared to the mainstem during freshet necessarily leads to higher evasion rates. This process in itself actually leads to a reduction in CO_2 levels. I therefore suggest rewording the last sentence to fit the reported results or adding one or two more results from this study to make the connection here.*

We have re-written this sentence and included CO_2 fixation by primary producers, a process we see now that we have not highlighted enough in the manuscript. The last sentence of the abstract reads now (the additions in *italics*):

"In lower order systems, we find rapid initiation of primary production in response to warm water temperatures during spring freshet, shown by decreasing δ^{13}C-POC, in contrast to larger rivers. *This results in CO_2 uptake by primary producers and microbial degradation of mainly autochthonous OC, however, if terrestrially-derived inorganic carbon is assimilated by primary producers, also CO_2 emissions may occur.* As Arctic warming and hydrologic changes may increase OC transfer from smaller waterways to *larger* river networks, *understanding carbon dynamics in smaller waterways is crucial*."

3. ***Statistical approach**: I wonder if the Welch's t-test for testing differences between the two seasons is correct here for this study design. The assumption of this test is that the two groups are independent. At the same time, the authors want to test whether spatial characteristics in the watershed influence carbon dynamics at a sampling point. This implies, in my opinion, that the authors assume that the location of the sampling point in the landscape influences water quality and carbon pools. Hence, the two samples collected in two seasons at the same site might be more similar than the others and should be paired for the statistical test. Can the authors please explain why they use the independent Welch's t-test or change their statistics if there is no justification for choosing the statistical test. I have one additional comment about the statistics. The authors use simple linear regressions to investigate how environmental factors influence carbon dynamics (Fig. 2). They state that they want to "examine how watershed characteristics control carbon concentrations." (lines 18/19). This could be done by running multiple linear regressions or linear mixed models with sampling site as a random factor (to account for their dependency) to see which factors are "most relevant" for controlling carbon dynamics. In this way, they could incorporate several independent variables.*

Thank you for these insights. Firstly, we did consider both paired and non-paired tests while establishing whether river waters sampled in spring and summer were independent and came to the conclusion to use the non-paired test. Reconsidering the test now, a paired t-test would be more appropriate as you point out that the landscape connects these sites. We have changed the Welch's test to a paired t-test (or non-parametric Wilcoxon ranked sum test if paired t-test assumptions were not met). The overall results did not change, except for δ^{13}C-POC and DIC of the Kolyma mainstem (from significant to non-significant). For the sites K3 and K4 of the Kolyma River mainstem, we used an average of the two replicate samples on these sites during freshet to pair them with the same sites sampled during summer. We have updated the method, results, and discussion section accordingly along with Table A6.

Secondly, we chose to use simple linear regression (Fig. 2) instead of multiple linear regression to investigate relationships between the variables that we found interesting: these were whether water temperature can explain changes in δ^{13}C-POC and how carbon isotopes (δ^{13}C-POC and Δ^{14}C-POC) may explain POC-%. We chose here to do simple linear regressions as we were interested in response of both $\delta^{13}C$ to temperature and POC-% to carbon isotopes, thus having two different response variables that we were interested of.

4. The authors measured POC and particulate nitrogen (PN) and also show ratios of POC to PN in table 1. The C:N ratio can also be an indicator of algal or terrestrial material, with ratios around 8 being of algal origin and with increasing ratios being more terrestrial. Please see Figure 1 in Meyer 1994 (Meyers, Philip A. "Preservation of elemental and isotopic source identification of sedimentary organic matter." Chemical geology 114.3-4 (1994): 289-302.). Perhaps this could be included in this manuscript and highlighted in the discussions. For example in line 238.

Thank you for this comment. We agree that C/N is a good indicator of source as well and have added the following sentence to the discussion:

"While the POC pool is dominated by autochthonous OC, it is likely that allochthonous OC is also present, as suggested by POC/TPN ratios (e.g., Meyers, 1994) and our source apportionment results (see Section 4.3 and Fig. 5)."

5. At the beginning of the discussion before the first subheading (after line 229): It would be nice to insert here a summary of the main findings in relation to the main objectives formulated in the abstract (lines 17-19) and at the end of the introduction (lines 36-38).

We have included a short summary at the beginning of the discussion that reads as follows:

"*Our results show contrasting water chemistry and carbon dynamics between spring freshet and summer in the Kolyma River tributaries and mainstem. The river POC is mostly autochthonous both in the tributaries and the Kolyma mainstem during both seasons. Small and midsized rivers differ in their POC composition from large rivers with higher POC-% (freshet and summer), lower $\delta^{13}C$-POC (freshet) and higher $\Delta^{14}C$ (summer).*"

6. Lines 347-352: "While POC concentrations did not significantly differ between large and small/midsized rivers during freshet, composition of POC showed clear differences: the δ13C-POC was lower and POC-% higher in small and midsized streams/rivers than in large ones, indicating an early onset of primary production in these lower order streams. This may fuel CO_2 evasion via degradation of autochthonous POC that is likely partly comprised of permafrost OC and/or prime degradation of allochthonous OC, however, further studies are needed to discern implications on CO_2 emissions in a system level." I like the conclusions the authors draw here. They highlight very nicely the most important results and implications here that I think are worth publishing. However, when primary production is higher, the authors usually also conclude that there is higher CO_2 evasion. I am not sure I follow this interpretation. Also in the discussion, the authors interpret their data in a similar way. Although one cannot rule out the possibility that more CO_2 is emitted when primary production is high, the direct consequence is that more inorganic carbon is taken up. Demars and colleagues nicely discuss the balance between primary production and respiration in streams as temperatures rise. They conclude that warming will not lead to an increase in CO_2 emissions in streams and rivers. See Demars et al. 2016 for a discussion on this topic (Demars, Benoît OL, et al. "Impact of warming on CO_2 emissions from streams

countered by aquatic photosynthesis." Nature Geoscience 9.10 (2016): 758-761.). This comment is related to the one about the last sentence of the abstract.

Thank you for this comment. We agree that while our focus was looking at potential CO_2 emissions from permafrost carbon degradation, we haven't highlighted inorganic carbon fixation by primary producers - an important process not to overlook. We have read the suggested paper (Demars et al., 2016) and incorporated their results to our discussion as follows:

"*While warmer water temperatures have been shown to increase microbial degradation at a similar rate as primary production, additional supply of terrestrial OC may increase degradation rates resulting in higher CO_2 emissions (Demars et al., 2016).*"

Additionally, we have changed the end of the abstract to highlight CO_2 uptake processes (see our response to question 2). Furthermore, we have highlighed CO_2 fixation also in the disuccion and conclusions.