



EGUsphere, referee comment RC2
<https://doi.org/10.5194/egusphere-2022-516-RC2, 2022>
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Comment on egusphere-2022-516 *With author response added in blue italics*

Anonymous Referee #2

Referee comment on "Contrasts in dissolved, particulate and sedimentary organic carbon from the Kolyma River to the East Siberian Shelf" by Dirk J. Jong et al., EGUsphere, <https://doi.org/10.5194/egusphere-2022-516-RC2, 2022>

The Jong et al. manuscript contained an enriched dataset of organic matter in different forms, including dissolved, suspended and sedimentary, from samples collected along the Kolyma River to the East Siberian Shelf. A comprehensive list of parameters was measured on these samples, including carbon stable and radio-isotopes, lignin phenols, lipid biomarkers, mineral specific surface area etc. They also used a mixing model to quantify the contribution of organic matter from three endmembers to these samples. The main conclusion was that DOC, POC and SOC along the transect have distinct compositional and degradation patterns, with significant contributions from permafrost- derived OC, particularly for SOC and DOC. It was also concluded that degradation occurred along the river to ocean transit based on biomarkers and OC loadings on minerals, among other minor conclusions.

Clearly this data is much more comprehensive than what has been published about the Kolyma River, or other Arctic rivers in general, as they included all three phases of organic carbon, and bulk and specific parameters. These data will be of value to the community, thus need to be published. The conclusions are solid, although I have to say that they are kind of expected and it is hard to find anything particular novel from what we already know.

The novelty of our study lies in the combination of the extensive fluvial dataset (three OC components along a river transect) with existing marine shelf/water column data. This shows that the largest shifts in OM composition actually occur in between the fluvial and the marine realm. We have highlighted this finding and its implications better in the manuscript.

It is great that DOC, POC and SOC were all measured in a same study, but the authors need to acknowledge the fact that SOC may be in totally different time scales in terms of mobilization and transport than DOC and POC. DOC and POC are co-transported with water flow, but SOC is likely not unless in a storm fasion. In other words, their resience times are way different.

We agree that we should put more emphasis on this, as now it is only mentioned in the final paragraph of the discussion. We will add information on this matter in appropriate places in the revised manuscript.

It is also not clear the depth of riverbed sediment was collected. This is important to know, as one could imagine surface 1cm could be very different from 10cm, in terms of not only the transport but also the level of dissolved oxygen which would affect degradation. The authors need to factor this in to the text.

We sampled the first couple of cm (approximately 1 – 5 cm) of surface sediment from the riverbed. The Van Veen grab sampler is not the most consistent device, especially using it in fast flowing river water, but any sample we obtained that looked 'intact' and of sufficient volume (i.e. no sample running out/leaky sampler) was stored and analyzed.

We changed the first sentence of 2.2.2 to: "Riverbed sediments were sampled using a Van Veen grab-sampler, sampling surface sediment up to 1 – 5 cm, and stored in sterile Whirl-Pak® bags."

Despite the comprehensives of this dataset, I still feel that there are a couple of key parameters missing, which would strengthen their arguments. For example, production was attributed to be the major contributor to the POC, but why not directly quantify the Chla concentration? This would direct address riverine production. ^{14}C -DOC was not measured, either. They offered a couple of references, but I think this is a key parameter to have, particularly because its changes along the transect would offer further insights into the OC dynamics. The situation may not be as simple as cited, "earlier studies show that Kolyma River and tributary DOC is relatively young...". Similarly, I am not sure why lignin phenols were not measured on POC?? This would directly address the contribution of terrestrial plants...

Thanks, we agree that these parameters would have been valuable to measure. As our initial focus was not so much riverine production (but instead tracing terrestrial matter) we did not collect Chlorophyll a. In hindsight this would have been very valuable.

For DOC- ^{14}C and lignin POC there were some methodological constrains;

For DOC- ^{14}C , our solid phase extraction setup (to concentrate DOC) was not guaranteed ^{14}C -contamination proof, and we had limited space to bring whole water samples for DOC ^{14}C back. Besides, there really are quite a few studies that have measured ^{14}C -DOC in the Kolyma mainstem, and our choice was to focus more on the composition.

For POC lignin, unfortunately the GFF filters we used to collect the material could not be used in the lignin extraction protocol. A different type of filter would be needed to collect samples for quantification of lignin phenols.

One of the motivations for conducting this work was the elusive nature of cycling and degradation of POD during the lateral transport through the whole watershed, as set up in the Introduction by the authors. However, when all the data are integrated, say from Figures 3-7, the degradation signals were most pronounced from the river mouth to East Siberian Sea, regardless of the end member contribution (Fig. 4), normalized biomarker concentration (Fig. 6), or biomarker degradation (Fig. 7). In a sense, I think that these data collectively mean that the estuary section is more important than the river stream itself in terms of organic matter processing. Yet, this was not discussed but should be (even though you may not agree with me).

We fully agree with the reviewer on this point, and see that this comment ties in with the first comment (the novelty of this study). By integrating data on all carbon species and by presenting the data as a continuous transect, we see that the transition zone between river and ocean is the place where most changes happen. This will be included in the next revision of the paper, as in the response to the first comment.

Line 60: delete the “.” before “degradation”

Thanks for noticing this!

Line 68: should be “Hilton et al. (21015)”

Changed to “Hilton et al. (2015)”.

Lines 121-130: it is a bit awkward to have a table and figure in the introduction. I would suggest that this be moved to the next section.

This figure and table are under section “2.1 Study area and sample locations” in the chapter “Methods”, which is the appropriate location for it according to us.

Line 153: how deep did the sampler penetrate? This may be important information (see my comment above).

The Van Veen sampler sampled the top 1-5 cm of surface sediment, we have added this to the text (see full response above).

Line 174: change to “according to Deirmendjian et al. (2020).”

Changed to “following the method of ...”

Line 252: it’s not clear what you meant by “...our own algal sample”. How do you know it was algal bloom? And there would be other types of organic matter in a riverine sample!

This was a visual observation while sampling. We cannot rule out a small contribution of terrestrial OC here, but given the very high OC concentration of the particulate matter sampled here (46%) points towards it consisting of almost pure, likely algal, organic matter.

We changed this sentence to "... and the sample of the Panteleikha River from this study ($\delta^{13}\text{C} = -33.5\text{\textperthousand}$, $\Delta^{14}\text{C} = -26\text{\textperthousand}$), where an algal bloom was observed during the study period." To clarify a bit better where this statement comes from.

Line 499: it could be simply due to the conversion of aldehyde to acid during oxidation, not necessarily selective degradation.

Thank you for this comment. We looked into this again, and think that it may lie in between a “conversion of aldehydes to acids” and “aldehydes degrade faster than acids” as we state the manuscript.

To quote Opsahl & Benner (1995): “Elevated Ad/AI ratios are indicative of microbial oxidation of propyl side chains of lignin which increases the carboxyl content of the remaining lignin ... ” This means that due to this microbial oxidation process more degraded lignin yields less aldehydes relative to acids, increasing the acid/aldehyde ratio, as lignin is a complex organic polymer and the CuO oxidation process splits the lignin polymer into individual phenols.

Taking these things into consideration we will now rephrased this line into: “More degraded lignin yields more acids relative to aldehydes in the CuO extraction process, which is reflected in a higher Vd/VI and Sd/SI ratio.”