

Review comments on “Organic Carbon, Mercury, and Sediment Characteristics along a land – shore transect in Arctic Alaska” Giest et al.

Reviewer 2

Accept with moderate revisions

Authors' Reply (AR): Thank you very much for your very constructive and valuable review

Reviewers comments (RC):

Global comments:

Overall the discussion is too centered on describing the results on not enough on interpreting them, although the conclusion is clear and concise.

AR: Thank you. In the revised version (will be uploaded in a second step, including the tracked changes we made) we improved the manuscript basing on your and the other reviewers comments

RC: The abstract and discussion have a big part on mercury but there is no mention in the conclusion. Mercury feels like an added measurements without much justification. There is not much background on mercury, especially on the different form of mercury and their risks.

AR: Thank you very much for your very valuable review. We added the justification on also including mercury to the revised version, as well as to the conclusion. Thank you for raising this point.

RC: In the supplementary methods, L29-45 the authors describe pore-water analysis that are not used or cited in the main manuscript. These results can be combined with the bulk TOC, TN ratios. If these analysis are not used for the interpretation of the results the methods can be deleted.

AR: Thank you very much for your very valuable review. In accordance with reviewer 1, we decided to remove water content and bulk density from the main manuscript. However, since these data might be interesting to some readers, we decided to keep them, along with the porewater analysis, in the supplementary material, which then requires including the respective methodology.

RC: Detailed comments:

L16: “their thawing could lead to the release...” I think that multiple studies have shown that thawing permafrost does release GHG, you should reformulate the sentence, maybe removing “could”.

AR: Thank you, changed accordingly.

RC: L16-17: I guess you did not do any modeling of future impacts of permafrost thaw in this paper, this sentence “warming. To enhance predictions of potential future impacts of permafrost thaw” is overselling the study. Rather you studied the impact of thermokarst processes, which in turn could be useful for predicting future effect of permafrost thaw.

AR: Thank you, we fully agree and changed the sentence to now read “To enhance predictions of potential future impacts of permafrost thaw, a detailed assessment of soil characteristics’ changes in response to thermokarst processes in permafrost landscapes is needed, which we investigated in this study in an Arctic coastal lowland.” (L XX).

RC: L22-23: As it is the abstract I would shorten to “lipid biomarkers (n-alkanes, n-alkanols and their ratios).

AR: Thank you, changed as suggested.

RC: L48: A reference is missing

AR: Thanks for pointing this out, we added a suitable reference here.

RC: L56: rather than “quality” you analyse the source of OM. If you refer to quality then you need to explain what you mean: lability?

AR: Thank you for specifying this. We added “...quality, hence the degree of decomposition, of organic matter (OM) in the different soils as well as differences in OM sources, lipid biomarkers can be used” (L XX) to the respective sentence. However, we disagree that only the OM source is investigated. This holds true for some of the ratios used (ACL, Pwax, Paq), while, if the previously mentioned ratios reveal the presence of similar material, CPI and HPA are used to determine the degree of degradation.

RC: L73-74: In this paragraph you talk about mercury in general without identifying if it is total mercury, methylmercury or elemental mercury that is measured. This is important when

stating the danger to human health and ecosystems as the different mercury form will be more or less dangerous. It should be also mentioned that there is usually a correlation between TOC and total mercury (e.g. Chakraborty et al., 2015).

AR: Thank you, this is absolutely right. Basing on your comment we adjusted the paragraph

RC: L76: OM rather than “OC”

AR: We agree, changed accordingly, thank you for this suggestion.

RC: L109: maybe marine coastal instead of just “marine”?

AR: Great suggestion, thanks, changed accordingly.

RC: L153: I guess you measured d13C on bulk organic matter? If so it would be good to add “The measurement of the $\delta^{13}\text{C}$ signature of organic matter”

AR: Thank you, we changed this to “The measurement of the $\delta^{13}\text{C}$ signature of bulk organic matter, ...” (L XX).

RC: L155: degradation is also a process affecting d13C, as you mention L391,

AR: Thank you. This sentence already says “...can provide information on the sources of OM and its degree of decomposition”, which in our opinion already shows the link to degradation.

RC: L153-158: Which reference standard were used to calculate the ratio? What was the instrumental error/accuracy?

AR: Thank you for emphasizing such details. Vienna Pee Dee Belemnite (VPDB) was used as a reference standard, as is already written in the last line of this paragraph. We added the instrumental accuracy of $\pm 0.15\text{‰}$ here as well.

RC: L190-192: Paq has been used a lot and has many bias, especially in region where floating and submerged vegetation are not fully characterized, such as in the Arctic. In addition, it is rarely used in soils but rather in lake sediment cores. Since the authors are studying different types of environments: soil, coastal, lake, I feel like this ratio does not add much information

as you would expect higher aquatic influence in the marine and lacustrine environment. Similarly for Pwax. In comparison, the degradation indices are much more useful for the study.

AR: Thank you very much for your comment. We agree that the indices have their weaknesses. For a multiproxy approach like ours, we think this adds a puzzle piece more. In this case it is useful to see that the lagoon and lake show an aquatic signal, and the upland a terrestrial signal.

RC: Fig. 3: d13C needs error bars

AR: Thank you for this comment, but we would like to keep it as is for two reasons: First, in such a graph, adding very small horizontal lines to each data point would deem the graph unreadable. Second, since such error bars would only reflect measurement accuracy, which would also be true for most other parameters, they would not increase the value of this graph when the accuracy is reported before. For detailed and accurate information on individual data points, we believe interested readers will anyway consult the measurement result table, published either online or in the supplementary material.

RC: L259: small typo “Mercury [ug ...]”

AR: Thank you, fixed.

RC: L382-383: This sentence is very hard to understand, could you rephrase it?

AR: Thank you, we tried to make this more clear and linking to the preceding sentence, now reading “However, these differences are likely also influenced by OM degradation during unfrozen periods of the thermokarst deposits.” (L XX).

RC: L412. The title of this section is quite hard to guess could you make it more detailed (e.g. Mercury content)? In this paragraph the biomarkers are not compared to mercury content, why not? Right now this paragraph does not seem linked to the rest of the study

AR: Thank you, as also requested by reviewer 1, we changed this section title to “Effects of OC characteristics on environmental mercury”.

RC: L415: can you add the correlation factor between TOC and mercury in the text to make it directly visible to the reader. 0.34 and 0.42 are quite weak correlation. I would rather point

and explain the correlation with grain size (-0.77). Further down in the text, the correlation with d13C is also rather weak (-0.42) and should be mentioned directly in the text.

AR: Than you, changed accordingly

RC: L427: not just “additionally” but rather “mainly”. See above

AR: Thank you; we implemented the suggested change as a result of our changes due to your previous comment.

RC: Figure 7: you can remove the 1:1 correlation point to make the figure more readable

AR: Thank you, we followed your suggestion and removed the blue points in the 1:1 correlation row. Now included as figure 5 in the updated manuscript (basing on the reviewer 1 comment placing figure 4 in the supplement and combining former figure 5 and 6)

RC: L455-456: The ACL difference is mainly indicative of additional primary production on site no? Since the concentration of the alkane is known the authors can mention which alkane increase in the saline and unfrozen deposit compared to the others.

AR: In accordance with comments raised by reviewer 1, we removed the detailed result reporting from this section. Detailed results can be found in the published dataset. However, differences in ACL can also relate to different (plant) species in the different locations. Hence, we prefer to not go into speculative detail here on which of the aforementioned processes affects the ACL in this case. However, we agree that in a manuscript with a stronger focus on biomarkers instead of landscape development, such a comparison would indeed be interesting.

RC: L458: “13” should be superscript

AR: Thanks for pointing this out. However, the respective sentence is not part of the revised manuscript anymore.