Review of manuscript: Sedimentary insights into organic matter alteration in Arctic Alaska's saline Permafrost.

General comments

The manuscript of Seeman and co-authors present organic matter characteristic (TOC, alkane and alkane-derived proxies) in deposits along a salinity gradient including permafrost and active layers in Arctic Alaska. They target a unique type of deposit, rarely studied so this wealth of data is definitely interesting to publish. However, I would recommend presenting the proxies used in this study a bit better, especially their limitations.

Author's response (AR): Thank you very much for your positive and constructive feedback and support.

Generally the discussion is hard to follow and the manuscript would benefit from a bit more organization in this part, maybe grouping the type of observation together rather than for each type of deposit? Or maybe focusing more on the effect of salinity on the observed degradation?

AR: Thank you very much. Basing on your comment we now introduce for better readability in the first discussion section (4.1) subsections. Concerning the regrouping of the parameter we kindly disagree and hope to convince you that only grouping after landscape position enables the clear interpretation of degradation with landscape evolution.
Because this study applies a multiproxy approach, discussing each parameter individually would overemphasize methodology and blur the integrated landscape interpretation and joint changes that are key to our study.

Specific comments

L39-40: "Therrmokarst processes are accelerating in the Alaskan tundra (Chen et al., 2021) while Nitze et al. (2017) describe a thermokarst lake drainage trend." I'm not sure I clearly understand this sentence, especially the use of "while".

AR: Thank you for pointing this out, indeed the sentence was formulated unclear. It is now reformulated to: "Thermokarst processes are accelerating in the Alaskan tundra (Chen et al., 2021), and Nitze et al. (2017) describe a concurrent trend of thermokarst lake drainage." (line 38-39).

L42: can you explain what is "saline permafrost"?

AR: Thank you for indicating that this paragraph needs clarification. It has now been adjusted (line 42-50). Saline permafrost refers to perennially frozen sediments which contain salts. These sediments are vulnerable to thaw, as the thawing point is depressed in these deposits.

L45: same for "unfrozen cryotic conditions"

AR: The paragraph has been adjusted as mentioned before. Unfrozen cryotic conditions refer sediments which are unfrozen although temperatures are below the freezing point.

L58: could you give a range of expected salinities

AR: The salinities observed in coastal permafrost environments range between freshwater and hypersaline conditions. This information is now added (line 61-63): "Since coastal permafrost regions can vary substantially in their salinity - between fresh and hypersaline porewater conditions - (Jenrich et al., 2021) , more differentiated investigations concerning different salinity levels are needed.".

L59: what do you mean with “differentiated”? More detailed, or with different techniques? It would be good to refer to the techniques used before in Giest et al. 2025 and expend on what will be newly applied in this study.

AR: Thank you for pointing out this unclarity. The sentence is now re-formulated. Giest et al., (2025) only differentiated between saline and non-saline sediments. Since salinities may vary substantially, however, different salinity levels should also be differentiated from another. This is the new approach implemented here.

L62-64: I agree with the authors that CPI has been increasingly used but it has major bias, in particular in region with old rock deposit that can lower the CPI. It would be good to present the organic carbon proxies and their limitation to be clear with the readers. Similarly d13C, D14C, C:N ratios have bias that need to be presented (heterogenous source effect, post deposition transformation, …).

AR: Thank you for raising this important point. We fully acknowledge that each individual proxy used in this study has inherent limitations and potential biases, including those related to source heterogeneity and post-depositional alteration.
This consideration was a key motivation for applying an extensive multiproxy approach, including n-alkane biomarkers, rather than relying on single parameters. In addition, our statistical analyses are not based on individual proxies alone; instead, we apply a multivariate PERMANOVA framework that integrates the full proxy suite, thereby reducing the influence of biases associated with any single parameter.
To address the reviewer’s concern more explicitly, we have now added a brief overview of general proxy limitations in Section 2.2.2. Proxy-specific limitations that are particularly relevant for our dataset (e.g. alteration of ACL values due to carbon degradation) are discussed in detail in the discussion section (see response below).

L77-78: is the temperature average from a meteorological station? IF so which one and how close to the study area is it?

AR: Yes, the data presented originates from the meteorological station in Utqiaġvik. Details of the data can be found in Rawlins (2021), as cited accordingly. The distance between the town and the study area is about 10 km (line 78).

L83-86: I’m not sure this part on vegetation in the region is needed as all the sites are mainly aquatic

AR: The upland and the DLB have mainly terrestrial vegetation. Some basic vegetation information is crucial as a background for the alkane proxies as this topic is picked up in the discussion (section 4.1). According to your comment we see shortening potential, thus the information provided is now slightly reduced.

L90: 22 and 50% of what is covered by thermokarst and DLB?

AR: The sentence is now reformulated: “Thermokarst lakes cover about 22 % and DLBs 50 % of the landscape (Hinkel et al., 2003; Jones et al., 2022).”

L154: for this part the subsamples were freeze dried?

AR: Yes exactly. As now adjusted in the sentence before, samples were freeze-dried after porewater extraction. Subsequent sample treatment followed with freeze-dried samples.

L159: Since the samples were not acidified TOC determined with a SoliTOC can be overestimated as some carbonate already burn before 900C. This is not an issue but should be acknowledged.

AR: Thank you for bringing this up, we are also aware of this. For marine sediments or samples with a substantial inorganic carbon content we would precede with sample acidification before quantification. However, as mentioned, for our samples this is not an issue and is therefore not mentioned in the manuscript.

L171-172: “Stable carbon isotope ratios are commonly applied as a proxy for organic matter origin and degradation in permafrost regions (e.g., Alewell et al., 2011; Strauss et al., 2015).”, as in the paragraph before this technical explanation should come after the method explanation.

AR: Thank you for pointing out this inconsistence. The paragraph is now adjusted accordingly.

L170-176: Can you give the standards used for this analysis as well as the measurement error.

AR: The $\delta^{13}C$ measurements were run at the AWI ISOLAB facility which uses lab specific standards (external standards) according to the procedure described in Schwamborn et al 2022 : “External standards were used to control the instrument precision and the range of replicate stable carbon isotope measurements was generally less than 0.15‰.”. We now cited this reference, but would like to keep the paper concise and therefore refer to the reference here.

L180: How was the radiocarbon dating conducted, what pre-treatments were done on the samples? Were the bulk sediment samples acidified?

AR: Detailed laboratory procedures can be found in Mollenhauer et al. (2021) as cited in the manuscript. We would like to keep the paper concise and therefore refer to the reference here.

L185: “eluted” or “extracted”

AR: Thanks, the term has been changed to “extracted”.

L190: how was the medium pressure liquid chromatography performed, with which solvents?

AR: Medium pressure chromatography uses n-hexane over silica columns. This information was added to the paragraph.

L201-202: “transformation effect”, do you mean degradation?

AR: Indeed. The wording has now been adjusted.

L211-212: another limitation of the ACL and n-alkane proxy is the heterogeneity and potential overlap of the source, see the review of Diefendorf et al., 2011

AR: Thank you for stressing this point. The limitations are now mentioned specifically: ACL limitations concern the blindness towards gymnosperms, overlapping chemotaxonomic patterns of different source material and potential post depositional alteration through degradation (Diefendorf et al., 2011; Jongejans et al., 2020; Struck et al., 2018; Zech et al., 2021).

L214: There is an odd over even predominance in terrestrial vegetation. In hypersaline environment the contrary can be observed (e.g. Li et al., 2024 Salinity impacts on *n*-alkanes in lake sediments of the Badain Jaran Desert, Northwestern China: Implications for paleoclimate reconstruction; Samantaray and Sanyal 2023 Effect of salinity on the preservation of plant-derived *n*-alkyl

compounds in the terrestrial-aquatic interface). This effect of salinity might be relevant for the study site.

AR: Thank you for pointing towards these references. Li et al. (2024) found that salinity enhanced alkane degradation. An odd over even predominance still occurs, as found in our study. The fact that salinity itself might already accelerate carbon degradation is now included in the discussion (section 4.2, line 517-518).

L259-264: Since TOC varies so much between units (35 to 5%) it would be more informative to express concentration normalized by TOC (ng/gOC) so that differences between units actually reflect different alkane concentration and not just the TOC effect.

AR: Thank you for this suggestion. The study aims at providing absolute/in-situ numbers, which is the total alkane content per gram sediment. The relative distribution of TAC contents among the samples does not change significantly if normalized with TOC contents (see Pangaea data source: https://doi.pangaea.de/10.1594/PANGAEA.983966). However, TACs normalized to TOC contents are now reported in the results additionally.

Figure 3: I don't see any red point in the figure, which incubation is referenced in the caption? It is not described in the method. I think the 14C ages should be added next to the depth to give a better idea of the period captured by the cores.

AR: Thank you for pointing this out. This is now corrected. Since radiocarbon ages are already presented in Figure 2, we agreed to not repeat those data points in Figure 3.

Paragraph 4.1. This paragraph has a lot of results instead of discussion and can be shortened by moving the core unit description into the results section. The interpretation of the different thaw process and organic matter input fits well in the discussion.

AR: This section has now been slightly shortend. We think that core units are beneficial for the discussion, as it is well possible to lead the reader through the argumentations with these.

L397-399: There is no explanation of the claim that ACL values support a shift from grass to a more mixed vegetation in the early Holocene.

AR: Thank you for noticing. The sentence is now reformulated: "Considering the ACL and *n*-alkane ratio, it could be inferred that this slowly occurring shrubification throughout the Holocene can be confirmed by our *n*-alkane analysis due to the tendency towards lower ACL and *n*-alkane ratio values in Holocene samples compared to the deepest (Lateglacial) biomarker sample (Fig. 3a)". Expected value ranges for different vegetation types can be found in the method section.

L399-403: Paq limitation is presented but just brushed aside without any reason (how is Betula shrub input influencing Paq for example?). In general this whole paragraph investigation the changes in ACL and Paq is not well described and there is no clear support in the text or in the figures.

AR: Thanks for pointing out, we adjusted this accordingly. Betula shrubs limit the power of Paq since these are reported with mid-chain lengths just as aquatic vegetation (see Weber and Schwark, 2020 and methods section) and the chain lengths are used to calculate Paq. This means, that Paq might also pick up Betula shrub signals. Together with the clarifications of your last point, the paragraph is now improved.

L413-414: What is the consequence of finding brackish talik sediment in a lake that was previously described as fresh? I get the point but this is not clearly explained. Also when did East Twin Lake experiences a transition to brackish water?

AR: The consequence of brackish sediment in the freshwater lake is, that the lake will likely turn brackish with continued subsidence. In East Twin Lake sediments an increase of electrical conductivity by about 200 % occurred between 2016 and 2022 (Jones et al., 2023), highlighting the temporal dynamic of this process. The paragraph has now been adjusted accordingly.

L417: Is the Teshepuk lake area far from the studied sites?

AR: It lies about 120 km east of our study area. This information is now included.

L417-418: Could Paq also indicate increased input from Betula as mentioned in the paragraph before? Which would fit with the info from ACL and n-alkane ratio?

AR: Yes, Betula might play a role here, too. Potential terrestrial vegetation input is mentioned subsequently by arguing that the ACL and *n*-alkane ratio provide evidence for this.

L419-423 "At this point, it needs to be stressed that in our study the ACL decreases with decreasing CPI values ($r = 0.79$, $p < 0.001$), meaning that the vegetation signal is influenced by organic matter degradation (strongest in ETL). This is a commonly observed process (e.g., Jongejans et al., 2020; Struck et al., 2018), which needs consideration when interpreting *n*-alkane proxies." This statement is coming a bit late and can be presented in the results already or at the beginning of the discussion. Why are the authors still using it if the main control on ACL is OM degradation?

AR: Thank you for pointing this out. The limitation of the ACL is now added in the methods section. It is mentioned again at this point because degradation is strongest in East Twin Lake. The ACL is still a valuable tool for vegetation reconstructions, since it is an alternative/addition to classic paleo proxies such as C:N and d13C.

L432: Can you indicate again what material has been dated for this site? The age difference could be due to the type of material.

AR: In our study (Table S8), as well as in Brown et al. (2003) plant remains were dated to determine peat sequestration onset. Therefore, the age difference cannot be explained by type of material.

L452-453: Would it be better to compare your lagoon with north American lagoon TOC and TN data like those in the Tuktoyaktuk area?

AR: Yes, we additionally included now the Teshepuk Lake area (Giest et al., 2025) and the Mackenzie Delta region (Jenrich et al., 2025).

L466-467: Can you give some numbers? In general in part 4.2. it would help the reader to get some numbers, averages …

AR: Thank you for this suggestion. Numbers have been added to the section, but we also tried to keep the section tight without repeating too many results.

L469-470: Can you give a reference for " δ 13C values become lighter (less negative) with degradation"

AR: Yes, Strauss et al. (2015) is cited in the sentence.

L479-480: I don’t think there is much of a difference between 6.5 and 6.9 for a CPI value. If you think this is a significant difference, can you cite similar setting where such a small difference has been interpreted.

AR: We would not state that this is a significant difference but we think it might be important to point out this difference as degradation under unfrozen cryotic and saline conditions may be relevant here. At this point, we therefore also cite now Li et al. (2024), which is the publication you suggested earlier.

L495: please give standard deviation and number of point when you give an average for transparency.

AR: Standard deviations are now added to numbers.

Technical corrections

Throughout the text: There are some space missing before references, likely dues to reference formatting (for example L75).

AR: Thank you for pointing this out. Missing spaces have been added now.

Throughout the text: avoid the formulation “we” and use passive sentence throughout the text

AR: Thank you for this comment. We agree that passive constructions are commonly used in methodological descriptions. We therefore adjusted selected sentences in the Methods section, while retaining first-person plural elsewhere to ensure clarity and consistency. A full conversion to passive voice throughout the manuscript was not undertaken.

L31: “the polar north” could instead be “the poles”

AR: It should be “polar north” in this case, as this sentence refers to the Arctic.

L33: “which relate […] to climate change” maybe to be more precise write to “temperature change”

AR: The wording has been changed to “global warming”.

L37: I think what matters most here is that these plains are low elevation? Instead of “vast”?

AR: Yes coastal plains are of low elevation, but the large extent is also relevant to mention to stress the importance of these regions. Therefore, the wording is kept as is.

L42: ”Furthermore” is maybe not needed here as there is a new paragraph starting

AR: “Furthermore” is now removed from the sentence.

L48 “coast” instead of “coastlines”

AR: Plural is required here, since the processes generally affect Arctic coasts. Thus, the sentence is kept in its current form.

L608-609: The font differs for the last sentences

AR: The text is correctly formatted now.