Response to editor

Dear authors,

Thank you for this revised version of your manuscript. I think that reframing your manuscript to focus on DOM composition has been a good decision that reflects the empirical data basis more closely.

We thank the editor and reviewers for their constructive feedback on this revised manuscript. In response, we have carefully clarified the use of mixing models and tracers, moderated our interpretations, and improved the consistency between the different datasets presented. In particular, we refined the manuscript to better reflect a qualitative, process-based interpretation of DOM transformations supported by multiple lines of evidence. We hope that these revisions adequately address the remaining concerns and improve the overall clarity and robustness of the manuscript.

In brief, we worked on:

- **Mixing models and tracers:** We clarified the respective roles of salinity (as a gradient indicator) and stable isotopes (as source tracers), and better described the scope and limitations of the two-endmember mixing approach. We also clarified that the mixing line is restricted to beach groundwater and seawater samples. Sections 3.2 and 3.3.
- **a350 interpretation:** We acknowledged the variability in a350, particularly in seawater, and moderated its interpretation. We clarified that the mixing line is used as a visual aid only and emphasized that conclusions are not based on this parameter alone. Section 3.3.
- **Optical indices (HIX, SUVA254):** We removed inappropriate references to mixing-line behaviour for ratio-based indices and now interpret them qualitatively, explicitly stating the methodological limitations. Section 3.3 and the figure captions
- **DOC–CDOM decoupling:** We clarified the purpose of Fig. 6. The decoupling between DOC and a350 is now presented as a key supporting line of evidence. The corresponding section (Section 3.4) has been retitled to better reflect the underlying processes driving this decoupling.
- **Integration of evidence:** We strengthened the coherence between optical indices, PARAFAC components, and redox-sensitive parameters, emphasizing that interpretations rely on converging lines of evidence.

There are a few points, mostly referring to the mixing model computation and application, that I ask you to still address:

Comment: You outline the computation and usage of a two-end member mixing model. From the current text it remains unclear to what extent salinity and/or water stable isotope data were used for this purpose. For example, lines 191-197 present stable isotope measurements, describe a mixing model based on salinity and suggest application of the model ("this approach") to stable isotope data besides other variables. What exactly was evaluated here with the mixing model? Also in lines 310-318, the roles of stable isotopes vs. salinity remain unclear. What exactly do we learn from stable isotope data that we do not yet know from salinity?

Response: We thank the reviewer for this comment and agree that the respective roles of salinity and stable isotope data were not sufficiently clearly distinguished in the previous version of the manuscript. We note that lines 191-192 are part of the Methods section and describe the analytical approach rather than the interpretation of isotope data and the mixing model approach. Therefore, if we understand the reviewer's concern correctly, it likely arises from a lack of clarity in how the different tracers (S vs. δ^2H, $\delta^{18}O$) are subsequently used and interpreted in the Results and Discussion sections.

In our study, salinity is primarily used to represent the S gradient from freshwater to seawater along the flow path and to describe overall mixing conditions in the STE specifically. However, it does not allow for distinguishing between different supra-permafrost groundwater sources (massive ice melt, active layer groundwater, and infiltrated surface meltwater), as these are all characterized by near-zero salinity. In contrast, stable water isotopes (δ^2H, $\delta^{18}O$) provide key information on the origin and mixing of freshwater sources within the supra-permafrost groundwater system. These tracers allow to differentiate between contributions from massive ice melt, active layer groundwater, infiltrated surface meltwater and thawed underlying permafrost (see figure 2), which cannot be resolved using salinity alone. The simplified two-endmember mixing approach based on stable isotopes (lines 311-317) was therefore used as a first-order estimate of freshwater contribution, while acknowledging the presence of multiple freshwater sources and associated uncertainties.

This approach is further described in Sections 3.2, 3.3 and in the caption of Figure 5, where the respective roles of tracers and the interpretation framework are now clarified. We acknowledge that this distinction between tracers was not sufficiently explicit in the previous version of the manuscript and have revised these sections accordingly to improve clarity.

Finally, we note that the use of a mixing line including beach groundwater and seawater samples was introduced following a suggestion from a previous reviewer, in order to better visualize dilution processes specifically within the nearshore mixing zone rather than across the entire continuum, where dilution patterns were not clearly apparent in the initial version of the manuscript. We have clarified this point in the revised text to avoid any misunderstanding regarding the scope and purpose of this representation.

Line 310 – 320 : Using stable isotope signatures, a first-order estimate of the relative contribution of fresh groundwater to beach groundwater samples can be derived using a simplified two-endmember mixing approach. However, this estimation remains uncertain due to the variability in the isotopic composition of the nearshore seawater endmember, which reflects a wide range of salinities ($5 < S < 20$), as well as the potential mixing of multiple freshwater sources (local and regional supra-permafrost groundwater, and surface meltwater runoff, see Fig. 2) within the supra-permafrost system. To constrain the endmembers, we used the mean isotopic composition of the most saline sample to represent seawater (−74.5‰ δ^2H; −9.1‰ $\delta^{18}O$; S ~ 20), and the massive ice sample to represent the fresh supra-permafrost water endmember (−224‰ δ^2H; −32‰ $\delta^{18}O$). Based on these assumptions, the contribution of fresh supra-permafrost water ranges from approximately 20 to 76 %. These values should be interpreted as indicative ranges rather than precise quantifications, given the simplified assumptions and the dynamic mixing conditions within the system. Although these estimates do not constrain the total SGD flux through the beach system, they indicate that freshwater inputs can represent a substantial fraction of beach groundwater, consistent with previous SGD studies in the Beaufort Sea region (e.g., Connolly et al., 2020; Demir et al., 2024; Bullock et al., 2024).

Lines 339-343: DOC concentrations in seawater samples, and to a lesser extent in beach groundwater, closely followed the theoretical mixing line connecting the freshest and saltiest endmembers. This mixing line, based on the most enriched and depleted stable isotope signatures in the dataset, corresponds to salinity values of 20.08 (N = 1 "seawater" sample) and 0.18-0.30 (N = 8 "beach groundwater" samples), respectively.

Comment: The likely most important mixing models are for DOC and a350. For a350, Fig 5B presents meltwater to have substantially higher a350 values than seawater. Then, how can the mixing model have a positive slope? This graph is fairly important, as evidence of non-conservative behaviour of DOM, i.e. transformation of DOM composition along its flowpath from melting active layer or ice to seawater largely rests on a350 data. Other data like HIX, SUVA254 or PARAFAC components agree with dilution as well as with transformation. In Fig 5B, data shows a lot of noise, hampering a clear interpretation relative to the (likely wrong) mixing line. The manuscript points to samples "below" the mixing line as suggesting selective removal of humic moieties, yet - no matter how the mixing line is drawn - there are equally many samples "above" such a mixing line. This suggests high noise of a350 data and questions the reliability of the mixing model as a basis for interpretation, a point that I would like to see addressed in a revision. Perhaps more importantly: Deviation of a350 data from a mixing line is equally high or even higher for seawater samples as for beach samples - How then can these data be reliably used to infer processes in the beach samples besides dilution with the anyway variable seawater? These thoughts make it clear that despite the expectable existence of within-beach processes like Fe-interactions or microbial degradation, the evidence basis for such processes is not the strongest and this needs to be reflected in the text.

Response: We thank the reviewer for this detailed and constructive comment and agree that the interpretation of a350 data relative to a mixing line requires careful consideration. We would like to clarify that the mixing line shown in Fig. 5B was constructed using only beach groundwater and seawater samples (see the previous comment), and is intended to illustrate dilution processes within the nearshore mixing zone rather than to represent a full endmember mixing model including meltwater or massive ice, as proposed earlier in the revision process. This distinction has now been clarified in the revised manuscript.

We also emphasize that the mixing line was not used as the primary basis for our interpretation. Rather, it was intended as a visual aid to support the identification of dilution trends within the nearshore mixing zone. Throughout the manuscript, we have been cautious in its use and interpretation, and we have further revised the text to ensure this is clearly conveyed. We acknowledge that the variability observed in a350 data, particularly in seawater samples, limits the robustness of interpreting deviations from a theoretical mixing line. In response, we have moderated our interpretation and no longer rely on a350 deviations alone to infer selective removal processes, including the remineralization and the mineral interactions.

Importantly, our interpretation of non-conservative DOM behaviour is also supported by the decoupling between DOC and a350 (Fig. 6), which indicates that CDOM does not behave conservatively within the DOC pool. This decoupling suggests that DOC and DOM are influenced by distinct processes, consistent with compositional changes occurring along the flow path. Accordingly, we emphasize that our interpretation is based on the convergence of multiple independent lines of evidence, including optical indices, PARAFAC components, and redox-sensitive parameters, rather than on a350 behaviour alone. We agree that the evidence for in situ transformation processes should be interpreted with caution and have revised the manuscript to reflect this, framing these processes as being consistent with, rather than conclusively demonstrated by, the available data.

Lines 352 - 357 : As DOC concentrations, the a350 values declined sharply along the flow path, with a median value of 24.0 m^{-1} in meltwater samples and median values of 8.4 and 7.8 m^{-1} in beach groundwater and seawater, respectively. However, unlike DOC, a350 values in beach groundwaters and seawater deviated from the theoretical mixing line, indicating that they did not follow simple dilution between the freshest and saltiest endmembers. The variability of the endmembers, particularly in seawater, limits the robustness of a single mixing model and contributes to the observed dispersion, pointing to processes in addition to dilution. This interpretation is further supported by the decoupling between DOC and a350 (Fig. 6), as discussed below.

Comment: Further, as rightfully noted multiple times by reviewers before, mixing model approaches cannot be applied in a simple linear way in case of qualitative indicators such as HIX or SUVA. For instance, in Fig 5 C, a theoretical mixing model line would have to be a curve. Your current text makes references to the behaviour of samples with regard to HIX in comparison to a theoretical mixing model, e.g. in lines 347-348 and 407-408. However, an appropriate mixing model line, that could serve to back up these statements for HIX, is not presented in the graphs.

Response: We agree with the reviewer that ratio-based indices such as HIX and SUVA254 cannot be evaluated using a simple linear mixing model. Because the original fluorescence areas used to calculate HIX were not retained in the processed dataset, we cannot reconstruct a robust non-linear mixing curve for HIX. We have therefore removed references to deviations of HIX from a theoretical mixing line and now discuss HIX only as a qualitative indicator of changes in DOM humification along the salinity gradient. The interpretation has been revised accordingly to avoid over-reliance on mixing-model comparisons for ratio-based indices.

Lines 854 – 860 : The zoomed-in panels display only beach groundwater and seawater samples, along with a theoretical mixing line (dashed black line) connecting the “fresh” and “salty” endmembers for DOC and a350.These endmembers were defined based on the most enriched and depleted stable isotope signatures in the dataset, corresponding to salinity values of 20.08 (N = 1) and 0.18-0.30 (N = 8), respectively. As the underlying fluorescence and absorbance components used to calculate HIX and SUVA254 indices were not retained, non-linear mixing curves could not be reconstructed. In the boxplots, black lines indicate the medians, whiskers represent the full range of data, and individual points denote outliers.

An important piece of evidence is also the DOC-CDOM (de)coupling, which is an argument based on deviations from a linear relationship of DOC and quantitative optical variables. Here, Fig 6 does not serve the purpose as the log-scale on both axes does not allow to see a linear relationship or any deviations from it.

Response: We agree that the use of log-transformed axes can make the visual assessment of linear relationships less straightforward. As proposed by the editor, we deleted the log-scale. Fig. 6 highlights the decoupling between DOC and a350 across the continuum. This decoupling remains clearly visible, where variations in CDOM do not scale proportionally with DOC.

Comment: Fig 4: reword "showing the mixing line" because the graph does not show a mixing line. Did you ever consider a 3 component mixing model involving massive ice, perhaps using salinity AND stable isotope data?

Response: We considered the possibility of a three-endmember mixing model involving massive ice, active-layer/supra-permafrost water, and seawater. However, the stable isotope data are largely aligned along the regional meteoric water line, which limits their ability to resolve three distinct water sources in a robust way. In addition, salinity mainly captures the freshwater-seawater gradient and does not allow us to distinguish among freshwater sources, as these sources have near-zero salinity. Therefore, applying a three-component model with the present dataset would require assumptions that are not sufficiently constrained. In ongoing works, we are using additional tracers, including $\delta^{13}C$-DIC, to better discriminate among freshwater sources within the supra-permafrost groundwater system.

Comment: Fig 5: The insert graphs of panel B and C cover parts of the axis labels of previous panels.

Response: The position and size of the inset panels have been adjusted to minimize overlap. Despite adjusting the layout and scaling of the figure, some overlap with axis labels remains due to space constraints inherent to the multi-panel design.

Please also take care of a few minor issues:

15: delete "where its behaviour....pathways". Behaviour is unclear.

Response: removed.

16: flowpath instead of transport mechanism?

Response: changed.

27-29: strange sentence link, maybe break apart into 2 sentences.

Response: This has been changed; the sentence can now read as: "…suggests that microbial degradation and mineral-organic interactions likely contribute to DOM transformation, associated with a relative decrease in humic-like and HMW fractions. Alongside these transformation processes, mixing governs bulk DOC behaviour. "

31-32 and 464-465: These buzzword phrases are unclear in their meaning and "or" may not be the right connector.

Response: We agree. We acknowledge that the use of "or" may suggest mutually exclusive processes. However, we intended to reflect that it can operate under different functional regimes, which may be conceptually distinct and, in some cases, opposed (e.g., long-term carbon storage versus active biogeochemical processing and CO_2 production). We have deleted the last sentence in the abstract.

48: maybe "drive" instead of "contribute"?

Response: changed

50-52: what is "permafrost-derived nutrient recycling"? Reword. Last sentence of this paragraph is unclear.

Response: We have reworded "permafrost-derived nutrient recycling" to "permafrost-derived nutrient and carbon inputs", which more accurately reflects the release and utilization of permafrost-derived materials on Arctic shelves rather than their recycling. Additionally, we have restructured the last sentence was clarified. The revised sentences now read:

"Recent studies indicate that land-derived nutrients from coastal erosion drive ~30 to 50% of primary production in the Arctic Ocean, underscoring the critical biogeochemical role of permafrost-derived nutrient and carbon inputs on Arctic shelves (Terhaar et al. 2021; Vonk et al. 2025). Beyond erosion, subsurface pathways also contribute to these fluxes, as thaw depths and coastal hydraulic gradients peak in late summer and remobilize solutes from diffuse, non-point sources into the coastal ocean (Walvoord & Striegl, 2007; Lecher, 2017)."

77: Suggest paragraph break here, i.e. open the next paragraph with the last sentence of this one. What is "the" redox interface?

Response: We agree. We have deleted the sentence. This idea was taken up in the following paragraph.

88: reword, as DOM or DOC are not "elements".

Response: "Elements" has been changed for "compounds".

109-116: consider shortening by one sentence, partly repetitive.

Response: We have condensed the three sentences to avoid repetition. The revised text now reads:

"This study aims to characterize the transformation pathways of DOM released from the thawing of coastal Arctic bluffs and to assess the role of sandy beaches as zones of DOM transport and transformation at the shoreline, with a focus on compositional changes rather than flux quantification, within a system where mixing with recirculating seawater plays a major role in controlling bulk concentrations."

165: delete "in front of the slump system", repetitive.

Response: This has been removed.

301: suggest "concomitantly transported CDOM/FDOM".

Response: Done

324: Where does the remainder of up to 97% of total SGD come from? Does this statement refer to Arctic SGD only?

Response: We agree that the statement referring to the proportion of freshwater relative to total SGD could be misleading and was not sufficiently constrained within the context of our study. As this estimate was derived from studies with different methodological frameworks and spatial scales, it was not directly applicable to our system. We have removed this statement. The revised text now reads:

Lines 319-323 : Based on these assumptions, the contribution of fresh supra-permafrost water ranges from approximately 20 to 76 %. These values should be interpreted as indicative ranges rather than precise quantifications, given the simplified assumptions and the dynamic mixing conditions within the system. Although these estimates do not constrain the total SGD flux through the beach system, they indicate that freshwater inputs can represent a substantial fraction of beach groundwater, consistent with previous SGD studies in the Beaufort Sea region (e.g., Connolly et al., 2020; Demir et al., 2024; Bullock et al., 2024).

336: reconsider "in" the Arctic coastline.

Response: Done

339: data on other variables were not presented yet, statement is premature.

Response: We agree. We have removed the second part of the sentence, as the evidence for additional processes is presented in the subsequent sections. The sentence now reads:

"Dilution of the high-concentration, low-volume fresh groundwater endmember therefore contributes to the observed decline in bulk DOC concentrations."

347: This statement seems unjustified, it misses evidence, must be reworded in particular in light of how mixing models work for qualititative indicators such as HIX.

Response: This section was revisited. See previous comments.

366-368: Reword, "export of...marine...DOM to coastal waters" makes no sense.

Response: We agree. The word "marine" has been removed.

373: Which decline?

Response: We have specified that it refers to the decline in humification and molecular weight of the DOM pool.

376-379: Confusing sentence as it mixes up the main flowpath of this paper with riverine flowpaths.

Response: We agree. We have removed the reference to the Mackenzie River watershed and plume to keep the focus on the main flowpath.

380: This headline would have been appropriate at an earlier point already.

Response : We agree. We hesitated for a long time about whether to include this section in the previous one. However, here we focus on the processes that explain decoupling. We propose changing it to : " 3.4 Drivers of DOC - DOM decoupling along the supra-permafrost flow path"

381: Sentence meaning?

Response: The sentence has been revised.

390: reword "mobilized DOM transformation and export", DOM may be mobilized, transformed and transported, but DOM transformation cannot be mobilized.

Response: We agree the wording was awkward and have revised the sentence accordingly.

412-415: This does not feel like a sentence.

Response: The sentence has been revised.

431: Where these relationships with HIX even linear? Theory says that they should not be linear.

Response: We agree. The C2-HIX relationship is indeed non-linear. We have therefore replaced Pearson correlation coefficients with Spearman rank correlations for all relationships involving HIX (C2 vs HIX: $\rho = -0.90$; C1 vs HIX: $\rho = 0.96$; C4 vs HIX: $\rho = 0.93$; all $p < 0.001$). The text has been modified accordingly in sections 2.4 and 3.5.

440: "this humic signal" refers to C5?

Response: This humic signal" referred to the terrestrial humic-like components C1 and C4, not C5. We have revised the sentence to make this explicit.

447-449: Strange sentence. What is non-Fe-stabilized DOM? What are "redox-active conditions"?

Response: We have revised the sentence. However, the term “non-Fe-Stabilized DOM” has been used explicitly in other studies dedicated to Fe – DOM interaction. We have changed redox-active conditions for redox oscillations.

462: I don´t think you can say much about "persistence".

Response: We agree and have removed "persistence" from the text.

Best regards, Gabriel Singer