We would like to thank the reviewer for their thorough and precise work. We believe that we have been able to respond robustly to the various points raised. In this new version, we have clarified and streamlined the manuscript in several points to address the reviewer's suggestions:

1. Key statements on beaches and groundwater were moderated to better reflect the data and the dilution effect in seawater samples.
2. Fe-spiking experiments have been removed from the manuscript; main text has been shortened.
3. Terminology (DOC, DOM, CDOM, "terrigenous") clarified throughout the text.
4. Figures, and boxplots specifically, were revised to present data along the aquatic continuum : massive ice – meltwater – beach groundwater - seawater.
5. pH discussion and the DOC–CDOM relationship were revised for accuracy and clarity.

Details are provided below.

General comment:

The revisions have added much clarity and I especially appreciate the new Figure 2 and new text passages based on the recently published Kipp et al. 2025. In summary, the new version has made it much easier for me to follow the story and understand the system. However, in my view the manuscript needs another revision round. Apart from the minor comments below, there are two main issues that need to be addressed:

1. The abstract and conclusion state quite confidently that meltwater DOM is microbially degraded and immobilized by Fe oxides to low beach groundwater values. But the data (apart from a messy C2 plot) point to beach groundwater being mainly a mixture between melting ice and sea(river)water. There are no volumetric estimates for meltwater production to estimate if the DOM-rich meltwater is able to leave a traceable signal in the beach groundwater inventory. Neither are there estimates of beach groundwater residence times or seawater infiltration rates (based for example on the Kipp et al. radioisotope data). Therefore, it is difficult to judge which water masses primarily make up the beach groundwater compartment. As such, it is not entirely plausible why turnover, and not just dilution, is the reason for the DOC and aCDOM350 decrease from melt- to beach porewater. It's ok to not know the final answer, but this uncertainty should be reflected in the abstract and conclusion.

Thank you very much for your constructive remarks. Upon re-reading the paper and with some perspective, we fully agree that this comment is very appropriate. We have moderated our

statements and nuanced them to better align with the database presented here. We believe that sandy beaches and the groundwater flowing through them act as important biogeochemical reactors when activated as observed in temperate and tropical beach systems; however, additional data will be needed to explore this further.

Now we can read:

Abstract : line 23-32 [The inverse correlation between humic- and protein-like components, together with elevated concentrations of dissolved total Fe and elevated dissolved inorganic carbon (DIC) in beach groundwater, suggests that microbial degradation and mineral-organic interactions jointly contribute to the degradation and retention of supra-permafrost groundwater DOM pool before it reaches and becomes diluted in coastal waters. These findings highlight the potential role of nearshore and intertidal zones, particularly subterranean estuaries where supra-permafrost groundwater mixes with recirculated seawater, as key regulators of DOM composition and persistence. However, whether Arctic beaches primarily act as transient filters, permanent carbon sinks, or dynamic biogeochemical reactors remains an open question, underscoring the need for further research into their contribution to the transfer of carbon from permafrost to the ocean.]

Conclusion : [This study reveals rapid declines in DOC and CDOM concentrations along the diffuse and non-point source supra-permafrost groundwater flow from coastal permafrost bluffs through sandy beach sediments and to adjacent nearshore seawater. The findings highlight the importance of microbial degradation and mineral-organic interactions in transforming and retaining supra-permafrost groundwater DOM pool. These processes occur primarily within STE, where they selectively degrade humic-like, high molecular weight DOM and alter its composition before it reaches and becomes dilute with coastal waters. Our results highlight the potential role of nearshore and intertidal zones, particularly subterranean estuaries where supra-permafrost groundwater mixes with recirculated seawater, as key regulators of DOM composition and persistence. However, whether Arctic beaches primarily act as transient filters, permanent carbon sinks, or dynamic biogeochemical reactors remains an open question, underscoring the need for further research into their contribution to the transfer of carbon from permafrost to the ocean. Given accelerating permafrost thaw and coastal erosion, understanding these nearshore processes is critical for constraining Arctic carbon budgets and predicting their role in global climate feedback]

2. The Fe-DOM precipitation experiment is still a bit murky: Was Fe added to a final t0 concentration of 20 mM, e.g. up to two orders of magnitude more than DOC? How many samples were used, and how were they chosen? And - if Fe-precipitation primarily removes aCDOM350, would that not make meltwater DOM look more like sea ice DOM (high-DOC, low-aCDOM350) rather than beach groundwater (high-aCDOM350, low- DOC)? It also seems as if the highest Fe concentrations were found in oxic melt- and seawater (Figure 7A, though it is difficult to see), probably as

dissolved Fe-organic ligand complexes. Therefore, the Fe-DOM precipitation potential at beach groundwater redox interfaces does not seem to be so high.

We agree with the reviewer. Spiking the samples with excess iron does not allow us to reproduce the natural environment; it merely enables us to assess the DOC–Fe and CDOM–Fe reactivity. After careful consideration, we decided to remove the sections and information on the Fe experiment.

The manuscript has therefore been revised, and the relevant sections removed from the main text, which also helps to streamline the document.

Minor comments:

Abstract, line 25: acidic pH? Isn't the pH between 7-7.9? Microbial turnover is lower at acidic pH (<7), but Fe-Ox precipitation is favoured by low pH because there is less competition for Fe between hydroxides and DOM.

We agree with this comment. Only one beach groundwater samples exhibited pH_{NBS} values lower than 7 (e.i. 6.55) but the average pH value for beach groundwater samples is 7.55. The term "acidic pH" has been removed in the abstract section and the text in the discussion revised.

Lines 77-78 vs. lines 81-82: Citations are sometimes in alphabetical and sometimes chronological order. Please unify.

We revised the citations inserted in the text and used the chronological order.

Table 2 looks like an Excel table and the Fe values have comma decimals. Please update and correct.

Ok, have been changed.

Line 233-236: How much Fe is excess Fe? Is 20 mM the concentration at t0 Or was a 20 mM spike added to the samples to mimic the maximal natural Fe concentrations (~1 mM)?

The experiment was removed.

Line 253: How big is the intertidal zone? The tidal amplitudes are rather small, no?

The size of the intertidal zone is controlled by beach geomorphology (slope, mainly driven by the seasonal sediment budget), tidal amplitude, and wind regime. In the Inuvialuit Settlement Region, tidal amplitude is low (~0.3 m). At Tuk Island, the width of the intertidal zone (also referred to as the "discharge zone") reached several tens of meters in the summer of 2019 but was restricted to only 1–2 m in 2021. The information was added at the end of the *site description* section as follow:

Line 242 : [The width of the intertidal zone along these beach systems varies from a few meters to several tens of meters, depending on oceanographic conditions (e.g., wind regime and seasonal beach morphology).]

Lines 296-298 Is there evidence of Fe(IV) in the system? I am not aware of any groundwater study with these kinds of Fe species. Also, I am not sure what the sentence means and how it is connected to the rest of the chapter. Fe concentration ranges for the sample types should be reported in chapter 3.1 since they are useful to understand the general physicochemistry.

I'm not sure I understand the point here. Is the reviewer questioning the sentence on lines 350 to 352: [*The absence of light and the combined fluctuations in pH, salinity, and redox conditions in beach groundwater samples were conducive for a complex network of redox reactions, including the remineralization of organic matter and the cycling of iron between its oxidized (Fe(III,~~-IV~~)) and reduced (Fe(II)) states)*] ? Fe(IV) exists only as a reactive and transient intermediate, often associated with oxygenated or enzymatic complexes, but not as a stable mineral oxide. The sentence was corrected.

Fig. 2: Nice figure! But what are the blue dots? There is no A and B in the figures like in the text. Mark approximate locations for meltwater, porewater and ice sampling.

Figure 2 has been modified to incorporate the suggested information. The blue points represented/imitated melt ponds. The information was added in the figure caption.

Lines 328-330: I am not sure why microbial degradation should proceed at the same rate for optically and non-optically active proportions of DOM. Aromatic compounds are photoactive but chemically very stable. Also, from here on different names (DOM, CDOM, aCDOM350) are used for presumably the same fraction (aCDOM350).

We assume that the reviewer's question concerns this sentence, from lines 383 to 385: [*While mixing (or dilution) and microbial degradation affect both DOM and DOC at similar rates, processes such as photo-oxidation and organo-mineral interactions can selectively modify optically active and non-optically active components of DOM.*] Based on the comment, we understand that there is

confusion regarding the use of the terms DOC, DOM, and CDOM. We have therefore tried to clarify this sentence, as well as the rest of the text.

In this phase, we made a shortcut based on previous studies in subterranean estuaries, in the absence of photo-oxidation. However, along aquatic continuum, kinetic processes that included mixing, photochemical oxidation, mineral interaction and microbial degradation, can operate at different rates on DOC and CDOM fractions of the DOM pool which in result can cause a decoupling and nonlinear relationships between these two pools (Stedmon and Nelson,2015; Massicotte et al., 2017). So we removed that sentence. Now you can read :

Line 439 - 441: [Microbial degradation, photo-oxidation and organo-mineral interactions mechanisms selectively degrade specific organic compounds, and they lead to shifts in CDOM composition or a loss of CDOM along the land-ocean continuum (Nelson et al., 1998; Del Vecchio & Blough, 2004; Weyhenmeyer et al., 2012; Nelson & Siegel, 2013).]

Line 924 - 926 : Weyhenmeyer, G.A., Fröberg, M., Karltun, E., Khalili, M., Kothawala, D., Temnerud, J.,Tranvik, L.J., 2012. Selective decay of terrestrial organic carbon during transportfrom land to sea. Glob. Chang. Biol. 18 (1), 349–355, https://doi.org/10.1111/j.1365-2486.2011.02544.x, 2012.

Lines 353-354 Why are the BIX and FI data not shown, if they support an important conclusion?

The BIX and FI data are not shown because they remained relatively stable along the continuum. Instead, we focus on the PARAFAC components, which consistently align with and support the CDOM and fDOM data.

Line 368-369 Link 2025 is not in the literature list. Perhaps Kipp?

Exact! The citation was modified.

Line 402-403 The experiment does not consider all tDOM but only the optically active part. The unreactive tDOM largely remains as seen in the DOC plot. Since meltwater has low salinity and does not leach marine material, so any DOM in meltwater is by default terrestrial.

As mentioned above, we have clarified our use of terms, which was indeed causing confusion. Regarding meltwater collected at the foot of permafrost cliffs or in meltwater pools, the reviewer makes a valid point! I (GC) realize I had an oceanographer's bias. We were careful in using the term "terrigenous," as it truly cannot be anything else.

Line 407: You mean aCDOM350?

We are not exactly sure where in the text this applies, but we have clarified these terms as thoroughly as possible throughout the manuscript.

Figure 7: Use normal numbers on y axes, not scientific notation. Fetot is subscript in the caption but not in the figure itself – stick to one style. The figures appear compacted and lines are skewed. Is this a result of the pdf build? If not, please correct it. It makes it difficult to see the data point distributions.

The figure has been reworked.

Figure 8: The non-colored (non-filled? Because they have color..). symbols represent what, DOC and aCDOM350 of the original samples before Fe addition? Why does aCDOM350 of ground- and seawater go up after Fe addition, and why do all three different sample types have the same aCDOM350 after Fe addition?

This figure was removed from the manuscript.

Figure 9: Salinity is still denoted as Sp. Use S or even better write “Salinity” because there is sufficient space on the axis (it is not that much longer than “Type”).

The figure has been reworked and is now Figure 8.