In the manuscript by Stachnik et al., data is presented on dissolved and particle speciation of iron (Fe) and manganese (Mn) from waters adjacent to two distinct, but proximally close, Arctic glaciers. These glaciers are distinguished by their setting, where one is marine-terminating and the other land-terminating. The goal of this study was to identify how these differences in glacial setting affect the export, phases, and seasonal dynamics of dissolved and reactive trace metals. To this end, seawater, freshwater, and suspended marine particles were collected and treated using established geochemical treatments. The authors conclude that highly reactive particulate Fe extends further from the glacier source than dissolved Fe, that the marine-terminating glacier produces higher fluxes of highly reactive particulate Fe and Mn which scales with sediment flux and sedimentation (from sediment traps), and that bioavailable fractions of trace metals (micronutrients) input is likely to decline under future glacier retreat.

Overall, this is an important study and one that builds on recent literature, which suggests that the bioavailable flux from glaciers into fjord settings is dominated by reactive particle fractions. In even more detail, the authors include flux and particle speciation that provides important information about the minerology of these fractions, which have been tied to bioavailability in previous studies.

Authors: Thank you.

For the most part, their conclusions are supported by the data, but in some cases are not, and so it is recommended the authors do not try to overstate what the data show.

Authors: We thank the Reviewer for this valuable comment and agree that several interpretations and conclusions should be more closely aligned with the evidence presented. Should a revised version of the manuscript be invited, we would carefully review the manuscript to ensure that all conclusions remain directly supported by the available data and that speculative interpretations are clearly identified as such. We would also consider revising the Conclusions section to better reflect the scope of the study and the limitations of the available evidence.

One major criticism is the incorporation of macronutrient and other data was not done sufficiently to provide any evidence in support of their conclusions, and therefore should be removed (see comments by line). The manuscript could benefit from a judicious culling of detail and figures that do not support the final conclusions and to help the reader, being concise in its presentation. This includes limiting the amount of discussion in the presentation of results, and vis versa, do not present new data in the discussion without systematically presenting it in the results.

Authors: We thank the reviewer for this constructive comment, which is consistent with the concerns raised by Reviewer 1. We agree that the macronutrient and organic carbon datasets, as presented in the original manuscript, were not sufficiently integrated with the primary objectives of this study and did not provide direct support for the main conclusions regarding micronutrient dynamics. Rather than substantially expanding these analyses, we chose to streamline the manuscript and maintain focus on the key processes controlling micronutrient distributions in the glacier-influenced coastal environment. Consequently, we would suggest removing Section 5.1.3, “Role of macronutrients and organic matter distributions: implications for micronutrient cycling”, from the main text. We would also propose removing the associated supplementary discussion (Text S1.3) and related figures (Figs. S7 and S8). Corresponding references and cross-references would be updated accordingly, and the numbering of the supplementary figures would be revised. We believe these changes would improve the overall clarity and conciseness of the manuscript.

There are a few instances where I disagree with the authors interpretation of the data. For example, one major scientific concern is that higher sedimentation fluxes are a result of greater amounts of erosion beneath a given glacier, but this is only partly true because there was no size classification of the sediments, given that coarse-grained sediments sink out proximal to the glacier, compared to small sediments which remain suspended into distal locations. The comments about water mass retention are also not supported because without size characterization one can not determine how quickly they will sink, rather than demonstrating that sediments are retained due to bathymetric constraints.

Authors: We thank the reviewer for this important comment. We agree that particle size is a key factor influencing settling velocity, sediment transport, and sedimentation rates, and that the lack of direct grain-size characterization limits our ability to fully distinguish between enhanced sediment supply and particle sorting processes. Our sampling site was located approximately 1.5 km from the glacier terminus, beyond the zone where the most rapid deposition of coarse glacially derived material is typically observed glacier-fed fjords in Arctic (Meslard et al., 2018; Svendsen et al., 2002; Syvitski, 1989; Trusel et al., 2010). Most of these studies were conducted in Kongsfjorden (Svalbard), where glacier catchments and meltwater discharge are substantially larger than in Hansbukta (e.g., Kongsbreen drains >400 km^2 and discharged ~900 × 10^6 m^3 of freshwater in 2017; (Halbach et al., 2019)). While the inner basins of Kongsfjorden and Hansbukta are of comparable depth (~95 m), Kongsfjorden is characterized by a deeper sill (20-50 m; (Svendsen et al., 2002)). Given the smaller freshwater input in Hansbukta, we consider it likely that most coarse particles settled closer to the glacier terminus and therefore contributed only marginally to sedimentation at our study site. Nevertheless, direct particle-size measurements would be required to verify this interpretation and to assess the potential influence of other sources of particulate material, such as iceberg-derived debris.

We also examined particle-size information derived from UVP observations. During spring, particle-size distributions varied with depth and between sites, indicating differences in particle transport pathways and aggregation processes (see figure below this paragraph, Fig. REV2). Larger particles were observed at intermediate depths in Hansbukta, whereas particle-size distributions were more uniform in Gashamna. These observations suggest that hydrodynamic conditions influenced particle distributions within the water column. However, UVP observations cannot provide direct information on sediment grain-size composition and therefore cannot fully resolve the relative importance of particle sorting versus sediment supply. In addition, the observations were limited to the spring season and do not capture particle dynamics during the summer melt period, when increased glacial runoff, sediment delivery, and stronger stratification may substantially modify particle-size distributions and transport pathways.

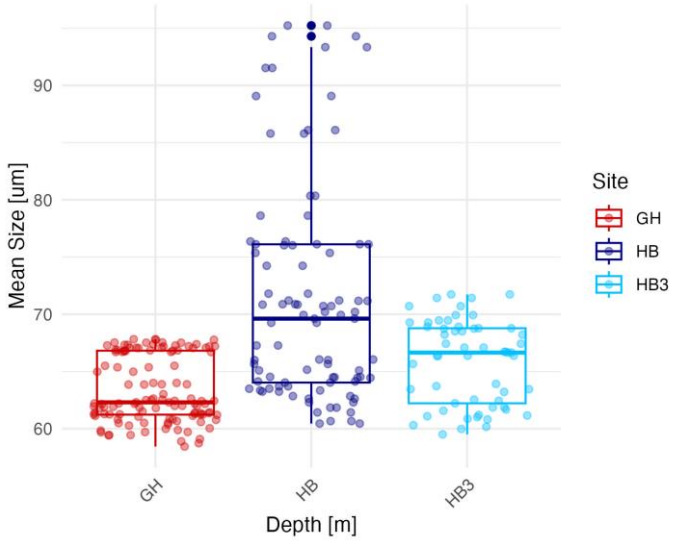


Fig. REV2. The mean particle size was calculated as a concentration-weighted average per 5-m depth intervals based on the UVP6 data, spanning from the concentrations (*C*) through 15 size classes from 50 um to 4 mm. The lower size band of each size class was used as the representative particle size (*S*).

We agree that direct evidence for sediment retention within Hansbukta would require dedicated observations of local circulation patterns and particle transport. Should a revised version of the manuscript be invited, we would clarify that sill-driven retention is an interpretation supported by the hydrographic setting rather than a process directly demonstrated by our dataset. We would also consider strengthening the discussion by referring more explicitly to previous studies showing that shallow sills can influence the retention of glacial outflow and sediment dispersal in Arctic fjords (Bao and Moffat, 2024; Gillibrand et al., 1995; Hager et al., 2022; Zanker et al., 2024). In addition, we would further develop the discussion of particle sorting and deposition processes operating in the vicinity of glacier termini.

No discussion was included about potential other sources of trace metals, such as icebergs and shallow marine sediments.

Authors: We agree that icebergs could be potential source of both micronutrients as suggested by earlier studies (Hopwood et al., 2020; Hopwood et al., 2019; Raiswell et al., 2008b; Raiswell et al., 2016; Raiswell et al., 2006). In this study we have not measured iron and manganese content in sediments in icebergs. We think that icebergs would likely have a rather marginal effect on labile iron and manganese fluxes in this setting. First, the sediment delivery by icebergs is low compared with subglacial discharge, and is estimated to account for a maximum ~5% of total sediments flux to the bay (Moskalik et al., 2018). SPM content in icebergs is likely to be much lower than in the sediment plume from subglacial discharge. Earlier studies on debris content in Hansbreen ice showed very low values, ranging from 0.5 to 0.05%, apart from a 1-2 m thick basal ice (Rachlewicz and Szczuciński, 2000). Secondly, although we did not directly assess Fe and Mn associated with iceberg-derived sediments, available evidence suggests that these materials are unlikely to constitute a major source of reactive Fe and Mn in Hansbukta. Average FeA concentrations reported for iceberg-hosted sediments from Svalbard (~0.16% (Raiswell et al., 2016)) are approximately two

times lower than those measured in Hansbukta SPM, while FeD concentrations occur within a similar range. Coupled with the relatively small flux of iceberg-derived sediments, these observations suggest that icebergs contribute only a minor fraction of the reactive Fe and Mn supplied to the bay. Also, high sediment delivery with ice-bergs may be stronger for ice sheets as compared with Svalbard (Dowdeswell et al., 1998).

In addition, we note that grounded icebergs may affect sediment dynamics not only through direct sediment release during melting but also through seabed disturbance and resuspension of previously deposited material. This mechanism has been documented in glaciomarine environments (ElverhøI et al., 1983; Mugford and Dowdeswell, 2010), and iceberg scour marks have previously been identified in Hansbukta (Ćwiąkała et al., 2018). Although the magnitude of this process remains unconstrained in our study area, grounding-induced resuspension may contribute to maintaining elevated local SPM concentrations.

We agree that the potential role of icebergs in Fe and Mn cycling deserves greater consideration. Should a revised version of the manuscript be invited, we would propose including a short discussion on iceberg-derived micronutrient inputs and sediment remobilization within the broader context of additional controls on Fe and Mn cycling.

We also agree that shallow marine sediments may represent an additional source of dissolved Fe and Mn through benthic recycling and release to the overlying water column. Such processes have been widely documented in glaciated fjords, where the reductive dissolution of Fe and Mn oxides within sediments promotes diffusive fluxes of dissolved metals across the sediment-water interface (Laufer-Meiser et al., 2020; Michaud et al., 2020; Piret et al., 2026). Although several recent sediment-core studies have been conducted in the fjord, they have focused on sedimentation rates, contaminant records, and paleoenvironmental reconstructions rather than Fe and Mn distributions or cycling (Pajda et al., 2026; Zaborska et al., 2017; Zaborska et al., 2020). Consequently, the potential role of benthic sediments as a source of dissolved Fe and Mn in Hornsund remains poorly constrained.

Direct quantification of benthic fluxes was beyond the scope of this study, and our dataset does not allow us to robustly assess the magnitude of sediment-derived Fe and Mn inputs. Furthermore, we did not observe a clear enrichment of dFe and FeA and FeD in bottom waters at either Hansbukta or Gåshamna (Fig. 6C). Dissolved Mn concentrations increased near the bottom at the HB1 and HB1E sites in Hansbukta indicating a potential benthic source, but did not exceed concentrations observed in surface waters (Fig. 7C, Mn profiles). The elevated dMn concentrations observed near the bottom of Hansbukta could potentially represent a signature of Mn benthic recycling and release. This interpretation is consistent with Piret et al. (2026), who showed that Mn is more readily remobilized from fjord sediments than Fe. Nevertheless, this process was not investigated in the present study and was not specifically considered when designing the sampling campaign. In addition, the deepest water samples were collected approximately 5 m above the fjord bottom, potentially missing near-bottom gradients associated with benthic release. Previous hydrographic observations indicate that bottom waters in Hornsund, including Hornsund's inner basins, remain well oxygenated (Drewnik et al., 2016; Korhonen et al., 2024). Such conditions would likely limit the release of dissolved Fe and Mn from sediments to the overlying water column.

Should a revised version of the manuscript be invited, we would propose adding a short subsection discussing the potential release of Fe and Mn from surface sediments and its influence on water-column concentrations. We would place this discussion within a broader section on additional controls on Fe and Mn cycling, alongside the potential role of icebergs.

The authors also make statements about dissolved-particle exchange and active cycling that is not necessarily supported in their data. In these environments, sediment surfaces are strong competitors for binding of metals, and so scavenging of colloidal oxide phases is particularly strong unless there is sufficient time to equilibrate with dissolved organic carbon ligands. For Fe, overwhelming numbers of competitive binding sites on particle surfaces and in particle-rich environments represent important sinks for dFe, rather than a sources.

Authors: We agree with the Reviewer that our original wording may have overstated the role of particulate Fe dissolution and did not sufficiently acknowledge the importance of adsorption and scavenging processes in particle-rich coastal environments (Schroth et al., 2011; Schroth et al., 2014). Should a revised version of the manuscript be invited, we would revise the text to clarify that particulate matter acts as an efficient sink for dissolved Fe and that any dissolved Fe generated through dissolution may be rapidly adsorbed onto particle surfaces, or stabilized by organic ligands (Jones et al., 2025). We would also consider removing wording that could imply active Fe cycling processes not directly supported by the present dataset. Furthermore, in response to related comments, we would expand the discussion of ferrihydrite stability and Fe transformations in seawater, while clearly distinguishing interpretations supported by literature from those directly constrained by our observations.

A good path forward for eventual publication is to focus the presentation of their data to continuously highlight the main discussion about impacts of glacial setting on particle supply and speciation for two important micronutrients. The beginning is strong, but the reader is lost in between the results and discussion (with more results). Finally, please add nuance as to the limitations of not considering important aspects of trace metal sources and particle size dynamics.

Authors: We appreciate the Reviewer's comment regarding the structure and interpretation of the manuscript. Should a revised version of the manuscript be invited, we would consider further improving the separation between the presentation of results and their interpretation, particularly in sections where these are currently closely interwoven. We would also evaluate opportunities to reorganize portions of the Discussion to more clearly relate the observed patterns in Fe and Mn concentrations and speciation to differences in glacial influence, hydrological connectivity, and sediment delivery between the study sites. In addition, we would consider expanding the Discussion to address other potential controls on Fe and Mn cycling that were not examined directly in this study, including iceberg-derived sediment inputs, benthic recycling, and particle-size effects. Such additions would provide a broader context for interpreting the observed patterns while making it clearer which processes are directly supported by the dataset and which remain plausible explanations based on previous studies.

My specific comments by line are below:

31: "reactive" used twice, might use something else

Authors: We would remove "reactive" as suggested.

46: "comprising both" "and/or" does not make too much sense to me when combined

Authors: We would replace “and/or” with “and”.

57: I think you mean, “seasonally” not “seasonality”

Authors: We would correct as suggested.

Fig. 1: It is not apparent what the PDD line represents from the caption. Later in the text it is explained that the PDD is integrated positive degree days over a 6 day moving interval. Please make clearer in caption when data is presented.

Authors: Should a revised version of the manuscript be invited, we would modify the caption to explicitly state that panel B shows the integrated positive degree days (PDD) calculated over a 6-day moving interval during June-August for Hornsund. This change would improve the clarity of the figure description and facilitate its interpretation by readers.

190: It is not clear what the process for this leach is as written. Please explain how each fraction is handled, including if volume is removed between the sequential leaches.

Authors: After completion of the ascorbate extraction, the sample was centrifuged and the ascorbate supernatant was carefully removed without disturbing the sediment pellet. The remaining sediment was then rinsed with 1 mL of ultrapure water, centrifuged for 10 min, and the rinse water was removed and added to the ascorbate supernatant before the dithionite extraction step. Should a revised version of the manuscript be invited, we extend methods section by this fragment:

“Following removal of the ascorbate supernatant, the sediment was rinsed with 1 mL of ultra-pure water (UPW; Milli-Q, 18.2 MΩ cm^{-1}) and centrifuged at 10,000 rpm for 10 min. The rinse water was subsequently added to the ascorbate supernatant before proceeding to the next extraction step. For the next extraction step 1.5 mL of dithionite solution was added to the sediment pellet and samples were shaken at 150 rpm in the dark at room temperature for 2 hours.”

200: How is LOD determined?

Authors: The limit of detection (LOD) was determined according to the 3σ criterion. Following the manufacturer's methodology, the LOD was calculated using the background equivalent concentration (BEC) and the relative standard deviation of the background signal (RSD_{BG}) of the first calibration blanks:

$$LOD = BEC \times 0.03 \times RSD_{BG}$$

This approach is based on three times the standard deviation of the blank signal and accounts for both the variability of the background signal and the analytical sensitivity represented by the BEC. Detection limits were determined separately for each analytical wavelength.

198-204: In the text the units are mentioned to be nmol/kg, but then values are reported as µg/L. Please be consistent, or explain why there are differences in units. I believe you mean that seawater concentrations of dissolved and particle fractions are expressed in molar units per weight seawater.

Authors: This is correct. Should a revised version of the manuscript be invited, we would recalculate all extraction data and report all extraction-derived concentrations in nmol kg^{-1} throughout the manuscript, tables, and supplementary materials to ensure consistency in the presentation of the results.

234: “morphologies” spelling

Authors: We would correct as suggested.

255: This is very interesting! Wouldn't the G glacier take longer from the production of sedments to reach the ocean by your hypothesis? Please clarify.

Authors: We agree that our original wording may have overstated the extent to which the 6-day integration window accounts for differences in sediment transport and delivery between glacier systems. Should a revised version of the manuscript be invited, we would clarify that this approach can only partially account for potential delays in meltwater and particulate matter transfer to the fjord. We would also emphasize that the selection of a 6-day window is primarily based on previous observations from glacier-fed fjord systems (D'Angelo et al., 2018) and should not be interpreted as a direct estimate of sediment transit times in the study area. This clarification would help better distinguish the methodological rationale from process-based interpretations.

272: "*highly* reactive"?

Authors: This would be corrected as suggested.

Fig. S2: Why is one log scale? How do you interpret this in the context of the different sized particles sampled? What is the contribution of particles between 0.45-0.7 µm?

Authors: We thank the reviewer for this comment. The y-axis was plotted on a logarithmic scale to better visualize the full range of SPM-0.45 concentrations, particularly at low values where a linear scale would compress much of the variability. The purpose of Fig. S2 was not to quantify particle-size fractions but to evaluate the relationship between suspended particulate matter determined using the two filtration approaches. The 0.7 µm GF/F filtration (SPM) is a widely used standard method for determining SPM concentrations in fjord and coastal environments, allowing direct comparison with previous studies (Meslard et al., 2018; Moskalik et al., 2018). In contrast, the 0.45 µm PES filtration was used for particulate trace metal analyses following established protocols (e.g., (Hawkings et al., 2014; Stachnik et al., 2025). Because the filters differ not only in nominal pore size but also in material properties (borosilicate glass microfibers versus PES membrane), the comparison cannot be used to determine the abundance of particles within the 0.45-0.7 µm size range.

We note that SPM0.45 concentrations determined on 0.45 µm PES filters were systematically higher than SPM determined on 0.7 µm GF/F filters. The median relative difference between the two measurements was approximately 60%. However, this discrepancy should not be interpreted solely as the contribution of particles within the 0.45-0.7 µm size range because the two filter types differ not only in nominal pore size but also in filtration characteristics and retention efficiency.

Fig. 4: There is no commentary in the manuscript as to why 2024 is much higher in terms of SPM. Please mention.

Authors: Although the focus of the study is on the 2022 and 2023 sampling campaigns, we agree that a brief explanation of the elevated SPM concentrations observed in 2024 could provide useful context. In a revised version of the manuscript, we would consider adding a short discussion of the potential causes of this interannual variability, supported by a supplementary figure summarizing seasonal snow cover, air temperature, and precipitation conditions. Specifically, we would note that SPM concentrations in both Hansbukta and Gåshamna were considerably higher during summer 2024 than in the other study years. This period was characterized by relatively thick spring snow cover and elevated summer air temperatures and precipitation, while exceptionally low salinity in Hansbukta in early September suggests enhanced glacial meltwater input. Together, these conditions

may have promoted greater sediment mobilization and delivery to the fjord. Following figure shows different

Fig. 5: Move down after the figure is called and referred to in the text.

Authors: We would move Fig. 5 according to Reviewer's suggestion.

322: Is the freshwater endmember interpreted as representative for both fjords? That this is the glacio-fluvial input from the subglacial drainage? Please clarify.

Authors: We used the Gåshamna river samples as the freshwater endmember because they are more representative of glaciofluvial discharge. These samples were collected directly from the main rivers draining the catchment and cover both field seasons, providing a robust characterization of freshwater Fe concentrations and their variability. In contrast, only a single freshwater sample was collected from the lateral stream draining Hansbreen in 2023. Although FeA concentrations were within the range observed at Gåshamna and FeD concentrations were higher, the limited sampling does not allow a reliable characterization of the Hansbreen freshwater endmember. Furthermore, the freshwater sample from Hansbreen was collected from a lateral outflow and this is unlikely to represent the integrated drainage of the entire glacierized catchment, and its subglacial source area is poorly constrained. However, both catchments share the same Neoproterozoic metamorphic basement, although they differ in the proportion of carbonate rocks. The controls on Fe mobilization are expected to be broadly comparable across the two catchments, with differences in carbonate abundance potentially leading to only minor site-specific variations.

Section 4.2.1: Is this section discussing the entire water column, or just the surface? upper 30 m? Related, is Table S3 just surface? Table 1? In the captions, please make sure to indicate which samples are being presented.

Authors: We agree that the description of the sampled seawater could be made clearer. Should a revised version of the manuscript be invited, we would clarify that the discussion in Section 4.2.1 refers to observations from the entire water column rather than only surface waters or the upper 30 m. We would also propose revising the captions of Tables 1 and S3 to explicitly indicate the sampled water masses and sampling depths. In particular, we would clarify that the seawater data presented in Table 1 represent samples collected throughout the water column, while the caption of Table S3 would be expanded to specify the sampling depth, year of collection, and station locations.

369: What is meant by "Mn indices converged"?

Authors: By the phrase "Mn indices converged", we intended to indicate that the seasonal difference between spring and summer became less pronounced. Should a revised version of the manuscript be invited, we would replace this wording with a more explicit description of the data. Specifically, we would clarify that median Mn indices remained higher in spring than in summer, but that the interquartile ranges (Q1-Q3) overlapped between seasons. Thus, although spring values generally exceeded those observed in summer, the seasonal distributions were not completely distinct. This revision would avoid potential misinterpretation of the term "converged".

Section 4.2.3: This section is beginning to sound like discussion. Please limit the amount of discussion and move to later sections.

Authors: We agree that the original text contained interpretative elements that would be more appropriate for the Discussion section. Should a revised version of the manuscript be invited, we

would revise Section 4.2.3 to focus more strictly on the observed relationships and reduce process-based interpretations. In particular, we would consider removing statements linking the FeD-SPM relationship to glacial erosion products, interpreting the negative dMn-salinity relationship as evidence of estuarine mixing, and suggesting transformation of reactive Mn-bearing particles into dissolved Mn in seawater. Such interpretations would instead be discussed more cautiously in the Discussion section.

379: Why would it be associated with mixing? Please clarify.

Authors: Our intention was not to imply that particulate Fe was directly controlled by estuarine mixing alone. Rather, the observed relationships were interpreted as reflecting the influence of freshwater-derived material and suspended particles on particulate Fe distributions. Should a revised version of the manuscript be invited, we would clarify this distinction and revise the wording to avoid implying a direct link between particulate Fe and estuarine mixing that is not explicitly supported by the dataset.

380: You are comparing bays and then seasons? This is difficult to understand when reading. What about summer had a stronger correlation? Summer is not being correlated.

Authors: We agree that the original sentence combined spatial (between-bay) and temporal (between-season) comparisons, making the interpretation difficult to follow. Should a revised version of the manuscript be invited, we would separate these comparisons and revise the text to more clearly distinguish differences between bays from seasonal variability. This would improve the clarity of the presentation and reduce the risk of misinterpreting the observed relationships.

Table 5.5: Please remove redundant correlations across the main diagonal of self-correlation. This will make it easier to analyze.

Authors: We agree that the redundant self-correlations on the main diagonal reduce the readability of the correlation matrix. Should a revised version of the manuscript be invited, we would remove the self-correlation values from the main diagonal of Table S.5 to improve the clarity and facilitate interpretation of the reported relationships.

386: Consistently with what? Season?

Authors: We thank the Reviewer for identifying this ambiguity. By the phrase “more consistent”, we intended to indicate that the correlations FeA/FeD vs. MnD remained relatively strong across both the complete dataset (Table S.5) and the summer subset (Table S.6). However, we agree that this wording is imprecise and may be confusing. Should a revised version of the manuscript be invited, we would remove this phrase and revise the sentence to more clearly describe the observed relationships without using potentially ambiguous terminology.

394: Please write out the meaning, “correlation between dMn and salinity was…”

Authors: We would remove meaning and focus on result instead of interpretation.

367-368: I don’t fully agree with this statement and the caveat that an additional source besides freshwaters could be present in this biogeochemically dynamic setting.

Authors: We believe this comment refers to the text around L397-L398 rather than the L367-L368, as it concerns the interpretation of mixing processes and Mn sources. Regarding L397-L398, We agree that dissolved Mn concentrations in coastal waters may be influenced by multiple processes and sources in addition to freshwater inputs. Should a revised version of the manuscript be invited,

we would revise the text to avoid implying a single dominant source or mechanism. We would also consider expanding the Discussion to acknowledge other potential contributors to dissolved Mn, including benthic recycling and iceberg-related inputs, while emphasizing that the present dataset does not allow these sources to be quantified independently. This revision would better reflect the complexity of Mn cycling in glaciomarine environments and avoid overinterpretation of the observed patterns.

410: Please choose a different word here (e.g., integrated sediment accumulation between June and August).

Authors: This would be corrected as suggested by reviewer.

Section 4.2.4: What about MnD? This was not included in the systematic discussion.

Authors: We agree that MnD should be considered more explicitly in the presentation and interpretation of the results. Should a revised version of the manuscript be invited, we would expand Section 4.2.4 to include a description of MnD sedimentation patterns and discuss MnD behaviour alongside the other Mn pools. We would note that MnD exhibits spatial patterns broadly similar to those observed for FeD and shows strong associations with particulate Fe fractions. At the same time, because MnD concentrations are substantially lower than MnA concentrations, we would discuss these results more cautiously and place greater emphasis on the dominant particulate and dissolved Mn fractions that exert the strongest control on the overall Mn cycle.

445: These variables were not presented in the results section.

Authors: Following the recommendations of both Reviewers, we would consider removing the macronutrient and organic carbon dataset from the manuscript. This would allow the study to remain more tightly focused on Fe and Mn cycling in glacier-fed fjord systems and reduce discussion of variables that are only indirectly related to the main objectives of the study. Consequently, we would also consider removing the associated results, discussion, and supplementary material based on these datasets.

446: delete “high”

Authors: We would remove as suggested.

450: Why would snowpack have low dFe? Please include a citation here.

Authors: We agree that our original interpretation was not sufficiently supported by the available data. We did not measure dissolved Fe concentrations in snowpack within the Gåsbreen catchment and therefore cannot directly attribute the low dFe concentration observed in the river endmember to snowmelt inputs. Moreover, published measurements from Hansbreen indicate Fe concentrations of 44 nmol kg^{-1} in snow and 13.6 nmol kg^{-1} in firn (Spolaor et al., 2021), values that are comparable to or higher than those observed in our river samples. Should a revised version of the manuscript be invited, we would revise the text to remove this interpretation and instead discuss the low dFe concentration in the context of geochemical conditions within the river system, including the potential influence of alkaline pH on dissolved Fe concentrations. This revision would ensure that the interpretation remains consistent with the available data and published observations.

459: There is no digestion of this data from the list to the conclusion. Please remove.

Authors: Following the recommendations of both Reviewers, we would consider removing the sentence in question together with the broader macronutrient and organic carbon dataset (including

DOC, POC, and PTN) from the manuscript. This would allow the study to maintain a clearer focus on Fe and Mn cycling, while avoiding conclusions that are not sufficiently developed or integrated into the overall interpretation. Consequently, we would also consider removing the associated results, discussion, and supplementary material related to these parameters.

464: "differences between glacier weathering environments"

Authors: We would change as suggested.

484: Do you mean TdFe? So a leach involving dilute HCl for a certain amount of time? How long were the samples stored acidified prior to filtration and analysis?

Authors: We acknowledge that the term TdFe should be used consistently and that the limitations associated with the TdFe dataset deserve clearer explanation. Should a revised version of the manuscript be invited, we would clarify that TdFe refers to unfiltered seawater samples that were acidified immediately after collection, re-filtered approximately one month later, and analysed after approximately three months of storage. We would also discuss the potential implications of this procedure, including the possibility that storage of acidified unfiltered samples may have promoted partial dissolution of particulate phases and elevated TdFe concentrations. Consequently, we would emphasize that TdFe is interpreted primarily as a measure of total dissolvable Fe and used to provide context for Fe partitioning.

488: Please be more concise here.

Authors: We would shorten and simplify the wording to improve readability while preserving the intended interpretation.

492: "more sensitively" than what?

Authors: By the phrase "more sensitively", we intended to indicate that FeA exhibited stronger spatial differences between the two glacier-fjord systems than either dissolved Fe (dFe) or Berger-leachable Fe. Should a revised version of the manuscript be invited, we would replace this wording with a more explicit formulation, such as "distinguishes more clearly", to make the comparison clearer and avoid potential misinterpretation.

494: I disagree. How does the Berger leach obscure the role of pFe export? The authors are lacking evidence to make this statement, especially since these show similar differences between fjords, since we assume FeA is a component of the Berger leach pFe.

Authors: We agree that the original wording ("obscure") may have overstated the distinction between FeA and Berger-leachable Fe. Our intention was not to imply that Berger-leachable Fe obscures particulate Fe export, but rather that this extraction integrates reactive Fe pools with different degrees of reactivity. Should a revised version of the manuscript be invited, we would therefore revise the sentence to emphasize that FeA may better resolve variability in the highly reactive particulate Fe pool that is potentially of greatest biogeochemical relevance, rather than suggesting that Berger-leachable Fe obscures particulate Fe export.

497: Remove "appears" from everywhere. A good alternative is "shows" or "demonstrates".

Authors: We agree that the use of "appear" should be considered. Should a revised version of the manuscript be invited, we would review the text and replace "appear" with more direct terms where the observations are clearly supported by the data. However, we would retain the term in the description of the SEM observations because the morphology of the particles is inferred from

observation of the images. In this context, the wording “appear as sub-micron-sized aggregates, most often with non-isometric shapes” appropriately reflects the nature of the evidence and avoids overinterpretation.

498-499: Attenuating more slowly than dFe does not demonstrate near-conservative behavior. It would be good to have more information for how slowly, such as through a decay fit equation parameter comparison.

Authors: We agree that a slower attenuation of dMn relative to dFe does not, by itself, demonstrate near-conservative behaviour. In addition, the study by van Genuchten et al. (2022) does not report regression parameters that would allow a quantitative comparison of attenuation rates between fjords. Should a revised version of the manuscript be invited, we would remove this comparison and revise the text to focus on the pattern directly supported by our dataset, namely the decrease in dMn concentrations with increasing salinity. We would also avoid using the term near-conservative behaviour unless supported by a dedicated mixing analysis.

501-505: This sentence is difficult to understand as is. I do not agree this is evidence to support dissolved particle exchange.

Authors: We agree that the observed relationship does not provide direct evidence for particle-dissolved Mn exchange. Should a revised version of the manuscript be invited, we would revise this section to avoid interpretations that extend beyond the constraints of the dataset. Specifically, we would remove references to particle-dissolved exchange and focus instead on the observed negative relationship between dissolved Mn and salinity. We would interpret this pattern more cautiously as being consistent with salinity-driven dilution, while acknowledging that additional processes may also influence dissolved Mn distributions in the fjord waters.

507: “more than an…”

Authors: This would be corrected as suggested.

513-515: Need to separate out results from discussion as much as you can, otherwise risk diluted the clear messages with interpretations of data.

Author: We agree that clearer separation between results and interpretation would strengthen the manuscript. Should a revised version of the manuscript be invited, we would further reduce discussion-oriented content in Section 5.1.2 and limit this section to observations directly supported by the dataset. Interpretative statements would be moved to the Discussion section, where they could be evaluated in the context of previous studies and alternative explanations. This restructuring would help maintain a clearer distinction between results and their interpretation.

521: The is a novel framework, and I think should be elevated. This is a very excellent example of clear presentation.

Authors: We appreciate the Reviewer’s positive assessment of this conceptual framework. Should a revised version of the manuscript be invited, we would consider giving this framework greater prominence in the manuscript, including by highlighting it in the Conclusions section.

Section 5.1.3: This paragraph does not add anything to the conclusions and there is limited interpretation. This could be a different story, but I will suggest of tying back in something about bioavailability. So far this was not done since the introduction. I recommend removing this section entirely.

Authors: Following the recommendations of both Reviewers, we would consider removing Section 5.1.3 entirely. The macronutrient and organic carbon datasets are only loosely connected to the main objectives of the study and are not integrated into the broader interpretation of Fe and Mn cycling.

572: There is no size data which could conflict with the interpretation. This could be due to coarse-sized particles contributing more and therefore sinking closer and quicker to the point source. We know that extensive sorting occurs in fluvial environments, and depending on the distance transported, the effect of particle sorting could be extreme.

Authors: We thank the reviewer for this important comment. We agree that particle sorting can strongly influence sedimentation patterns in glacier-fed fjords. To address this issue, we would expand the discussion of distance-dependent sediment deposition and include references documenting the rapid deposition of coarse material within the first 1000 m of glacier termini (e.g., (Meslard et al., 2018; Svendsen et al., 2002; Syvitski, 1989; Trusel et al., 2010). Our sediment trap was located approximately 1.5 km from the Hansbreen terminus, beyond the zone where most coarse-grained sediment is typically deposited. We would also propose including a comparison with Kongsfjorden, where previous studies have documented intensive sorting and deposition of coarse glacial sediments closer to glacier fronts. Although the inner basins of Kongsfjorden and Hansbukta are of comparable depth, Kongsfjorden is characterized by a deeper sill (20-50 m; (Svendsen et al., 2002) and receives meltwater from substantially larger glacier catchments, resulting in considerably higher meltwater discharge (Halbach et al., 2019). Therefore, if rapid deposition of coarse material occurs over relatively short distances in Kongsfjorden, a similar or even stronger sorting effect would be expected in the smaller Hansbukta system. We nevertheless acknowledge that direct particle-size measurements would be required to fully quantify the contribution of particle sorting. Should a revised version of the manuscript be invited, we would explicitly recognize this limitation and expand the discussion accordingly.

580: What about icebergs and their ability to maintain SPM for longer through melting and grounding events? Currently there is no discussion about their role for the budget of meltwater and SPM, and this is a major oversight for a marine-terminating glacier system.

Authors: We agree that the role of icebergs in controlling suspended particulate matter (SPM) and micronutrient dynamics deserves greater consideration in marine-terminating glacier systems. Should a revised version of the manuscript be invited, we would expand the discussion by including a dedicated paragraph addressing the potential contribution of iceberg melting and grounding to Fe and Mn cycling in study area. We would acknowledge that icebergs may represent a source of micronutrients, as suggested by previous studies (Hopwood et al., 2020; Hopwood et al., 2019; Raiswell et al., 2008a; Raiswell et al., 2008b; Raiswell et al., 2016). However, we would also note that the available evidence suggests a relatively minor contribution to sediment fluxes in Hansbukta compared with subglacial discharge. Iceberg-derived sediments are estimated to account for at most ~5% of the total sediment flux to the bay (Moskalik et al., 2018), while debris concentrations in Hansbreen ice are generally low (0.05-0.5%), except for thin basal ice layers (Rachlewicz and Szczuciński, 2000). Also, although we did not directly assess Fe and Mn associated with iceberg-derived sediments, available evidence suggests that these materials are unlikely to constitute a major source of reactive Fe and Mn in Hansbukta. Average FeA concentrations reported for iceberg-hosted sediments from Svalbard (~0.16% (Raiswell et al., 2016)) are approximately two times lower than those measured in Hansbukta SPM, while FeD concentrations occur within a similar range. Iceberg-

derived sediment delivery may be more important in ice-sheet environments than in the glacier systems of Svalbard (Dowdeswell et al., 1998).

We would also discuss that icebergs may influence SPM dynamics indirectly through grounding and seabed disturbance. Grounding-induced sediment resuspension has been documented in glaciomarine environments (ElverhøI et al., 1983; Mugford and Dowdeswell, 2010), and iceberg scour marks have previously been identified in Hansbukta (Ćwiąkała et al., 2018). Although the magnitude of this process cannot be constrained using the present dataset, it could contribute to maintaining elevated local SPM concentrations and prolonging particle residence times within the bay.

598: How does quantitative indicators support "glacier-ocean connectivity"? Please explain.

Authors: We agree that the term "glacier-ocean connectivity" may be too broad given the spatial extent of our study. Should a revised version of the manuscript be invited, we would replace this expression with "glacier-coast connectivity" to better reflect the scope of our observations. While our results suggest that FeA can be transported beyond the inner basin and across the sill, indicating that the influence of glacially derived reactive Fe is not restricted to the immediate glacier-proximal environment, we did not investigate transport further offshore. Therefore, we would revise the wording to avoid implying connectivity with the broader ocean and instead emphasize coupling between glacier-derived inputs and the adjacent coastal environment.

633: "Bioavailable Fe is…" The authors did not measure the detrital fraction associated with refractory lithogenics through a complete digest.

Authors: We agree that we did not quantify the total detrital Fe pool through complete digestion and therefore cannot assess the contribution of highly reactive Fe relative to total particulate Fe. Should a revised version of the manuscript be invited, we would revise the wording to avoid referring to bioavailable Fe and instead refer to the operationally defined highly reactive Fe fraction. We would emphasize that this fraction represents a potentially bioavailable Fe pool that is substantially larger than the dissolved Fe pool, while acknowledging that the refractory lithogenic Fe pool was not quantified in this study.

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