

Many thanks for the opportunity to review “Trace metal distributions in the transition zone from the Greenland Ice-Sheet to the surface water in Kangerlussuaq fjord (67N)”. The aim of this paper is to present new nutrient and trace metal data from this West Greenland fjord, in the context of the impact of glacial meltwaters and sediments on the supply of important elements to the ocean. More datasets and discussion surrounding this important topic are welcome in the literature and will be of great interest to your readership. I found this paper very clear and enjoyable to read. I only have a few main suggestions for clarifying the methods section, and a few additional minor comments to be addressed.

Main comments:

I would like to see more details regarding the trace metal and nutrient analyses in the methods section.

Line 138 states that the samples were calibrated against a “pre-made solution”. I think the authors need to give more details about this solution (commercial? In-house? What was its composition?). In particular, matrix matching is very important for ICP measurements (i.e., making sure that calibration standards have the same composition, as close as possible, to samples). This means that the calibration standards would need to be made up to an appropriate salinity using artificial seawater (and different standards may well be needed for river vs fjord samples). It would be good to verify if the authors have considered such matrix effects (at least show that they do not impact the accuracy and precision of the final measurements).

Line 138 states that “certified standards” were measured. Please include details of, and measurements of, these reference materials as this will help determine the accuracy (and precision) of the sample analyses.

Line 137 states that each sample was measured three times – what do these replicates indicate about analytical precision?

Line 138 says that “Background levels” were analysed, but what do these background levels represent? MilliQ water that was processed the same way as the samples (i.e., a laboratory blank) or an instrumental blank?

Section 2.4. Were reference materials run for the macronutrient samples? If so, please report these data as well. If not, this needs to be stated. What is the precision of the measurements in each case?

Note that the data table in the appendix (Table A2) does not show the individual measurements or the uncertainties associated with each measurement. I would either put in the data for each replicate or (probably more straightforwardly) include a standard deviation or standard error.

Apologies if I’ve missed it, but I can’t find the original macronutrient data in the paper submission portal. I would suggest including these too, or at least a link to a published and openly accessible dataset.

My general suggestion would be to make these datasets available in a usable format i.e., .csv format or similar. (I would recommend an external data repository to do this for full accessibility).

Minor comments:

Line 34: I found the addition of the two sentences on toxic metals a little out of place. There is more literature out there on toxic metals and glacial weathering that are not referenced here, and two sentences doesn't really do the topic "justice". Given that these elements were not discussed in the manuscript, I would suggest taking these sentences out.

Line 49: The authors rightly point out that there is iron limitation in regions of the North Atlantic in summer, but it might also be interesting (and useful for the paper) to mention that there is evidence also for season silicon limitation of diatom production in this region and elsewhere in the Arctic ¹⁻³.

Line 124: Some of this methods section (e.g., "surface salinity showed a minor change along the transect...") fits better in the results section. I would suggest the authors check through their methods and move any results to the appropriate section.

Line 205: I agree with the authors that the observation that the highest concentrations of many of the trace metals were where the freshwater content was highest, indicating an "impact from runoff". Do the authors mean glacial runoff specifically, or all types of runoff? As stated in the methods section, Fw also reflects freshwater from precipitation and sea-ice melt. Could these other sources be complicating the picture here? Or can the authors argue that Fw is dominated by glacial runoff? (which I suspect it probably is!). It might be worth just clarifying that here.

Line 284: I agree that the macronutrient data points towards additional sources due to lake runoff or in situ dissolution, but I would suggest expanding "remineralization of organic matter" to include "remineralization of organic matter and GRF", or similar, to be consistent with Lines 289-290.

Section 4.2. could be expanded. I would suggest to the authors to include some brief comparisons with published findings on macronutrients in Greenlandic fjords (similar to that in Section 4.1).

References:

- 1 Krause, J. W. *et al.* Biogenic silica production and diatom dynamics in the Svalbard region during spring. *Biogeosciences* **15**, 6503-6517 (2018).
- 2 Krause, J. W. *et al.* Silicic acid limitation drives bloom termination and potential carbon sequestration in an Arctic bloom. *Scientific Reports* **9**, 8149 (2019).
- 3 Ng, H. C. *et al.* Detrital input sustains diatom production off a glaciated Arctic coast. *Geophysical Research Letters* **51**, e2024GL108324 (2024).