Review of Influences on Chemical Distribution Patterns across the West Greenland Shelf: The Roles of Ocean Currents, Sea Ice Melt, and Freshwater Runoff.

General comment. This work investigates inorganic carbon, macro and micronutrients along the West Greenland Coast, an area facing enhanced changes due to global warming, mainly enhanced glacial melt (both from glaciers and sea ice). It should be highlighted that it is not common to have such a wide spectrum of biogeochemical parameters, and to get insights from such an extensive set of parameters, the authors followed an original approach: PCAs. The sampling strategy is well adapted, as mentioned before, the number of parameters is impressive, the analysis and data treatment are also solid and state of the art, the PCA's approach is original and solid. The authors know the area and what they are doing. However, I have some relatively minor concerns.

We sincerely thank the reviewer for the helpful and constructive comments. These suggestions will significantly improve the revised manuscript, particularly in terms of the presentation of the results. We greatly appreciate the time and effort the reviewer has dedicated to providing this valuable feedback. Thank you for your kind words!

Sometimes, the suggestions or conclusions are not always as strong as the wording suggests, so I would recommend some changes in the text. For example, I found the wording "corrosive", especially in line 563, inappropriate. I understand that it might appear necessary to have some attention-grabbing sentences/ideas, but the point is that if we consider that corrosive waters apply to waters with Ωaragonite or Ωcalcite below 1, there are NO corrosive surface waters during that study (Figure S10). If there is no question that the glacial meltwater promotes the decrease of Ωaragonite as it it shown by that study, we cannot draw any conclusion on the rate of the Ωaragonite change, any perspectives of how it will change in the future and when. So I would refrain from this alarming tune. The work is interesting enough without having to oversell the conclusions, I believe.

Original sentence: Apart from the Disko Bay area, the surface water of stations closest to the coast of Greenland were corrosive with lower buffering capacities (refer to Sect. 3.4).

Proposed changes: Apart from the Disko Bay area, the surface water of stations closest to the coast of Greenland had lower Ω Aragonite values with lower buffering capacities (refer to Sect. 3.4).

- ➔ Additionally, we will add specific values to line 207. This gives a better comparison between stations 6/11 and station 17 in the following sentence.

Original sentence: The higher CT:AT ratios of coastal stations coincided with higher Revelle factors and lower Ω Aragonite values, reflecting the lower buffering capacity of coastal waters.

Proposed changes: In surface waters of coastal stations 6 and 11, higher CT:AT ratios (0.920 and 0.925) coincided with higher Revelle factors (12.8 and 13.3) and lower Ω Aragonite values (1.93 and 1.82), reflecting the lower buffering capacity of coastal waters. In contrast, the surface waters of Disko Bay (station 17; 3 m) had lower CT:AT ratios (0.907), caused by low CT values of 1972 µmol kg^{-1}, corresponding to low pCO_2 values of 255 µatm. Coupled to this, the pH is high (8.19) and the Revelle factor low (11.85), indicating the higher buffering capacity of this water, which is also reflected by a maximum Ω Aragonite value of 2.17.

There is evidence, well shown in the manuscript, of a higher CT:AT ratio in sea ice meltwater. The impact of ikaite is correctly addressed, and enhanced CT is well mentionned. This high CT is ascribed to enhanced organic carbon remineralisation below sea ice. Why not, but I suggest mentioning that this excess CT can also come from remineralisation within the ice itself. As it has been correctly mentioned elsewhere, there is an enhanced concentration of trace metals in sea ice. This enhancement comes from both the atmosphere and the scavenging of organic matter in sea ice during its growth. So, it is not only the trace metal concentration that is enhanced in sea ice, but also the organic matter concentration. This comes with enhanced respiration and release of CT within the ice. It could be stated in the manuscript in part 4.4.

Original sentence: Possible explanations for this high surplus in CT could be the release of CT derived from organic carbon remineralization under the ice (Tynan et al., 2016; Bates et al., 2009) and the inflow of additional CT transported through the CAA to Baffin Bay (Shadwick et al., 2011b; Henson et al., 2023; Azetsu-Scott et al., 2010).

Proposed changes: Possible explanations for this high surplus in CT could be the release of CT derived from organic carbon remineralization under the ice (Tynan et al., 2016; Bates et al., 2009) or within the ice as sea ice typically shows higher concentrations in OM than the underlying water (Vancoppenolle et al., 2013). Another source could be the inflow of additional CT transported through the CAA to Baffin Bay (Shadwick et al., 2011b; Henson et al., 2023; Azetsu-Scott et al., 2010).

- ➔ Additional change to L521

Original sentence: However, the inflow of Pacific-origin waters and remineralization under the ice led to overall higher CT:AT ratios, resulting in higher pCO2 and lower pH and Ω Aragonite values in surface waters.

Proposed changes: However, the inflow of Pacific-origin waters and remineralization under and within the ice led to overall higher CT:AT ratios, resulting in higher pCO2 and lower pH and Ω Aragonite values in surface waters.

Also, despite the very extensive set of parameters, I regret that there are many indirect assumptions about the productivity of the water masses, while a simple measurement of Chl a would have been probably more robust. I presume that if the information is not presented here, it is because it is unavailable. Otherwise, it will be more robust in my mind to show the phytoplanktonic biomass as well, to discuss the productivity of waters, especially at that time of the year.

We currently do not have access to the Chl a data or phytoplanktonic biomass. As this was an interdisciplinary cruise, additional results may be published in the future by other research groups, which could then relate to this publication. Currently, we only have the oxygen and AOU data available.

Minor remarks

Line 169 The calculation was based on the median potential temperature of WGSW (3.23°C) within a depth range of 30 to 50 m, using observed salinity and AT, and the in situ pCO2 as input parameters. Is there any suggestion to chose that value as reference?

A temperature of 3.23°C refers to the median potential temperature of WGSW (*n* = 465) from the CTD data, filtered for a depth range of 30–50 m. This provides a solid reference value for the temperature of this water mass within this depth range. We will add this information to the text.

Line 235 “The concentration of dFe and dNi in surface waters (Fig. S 9c and e) followed a similar pattern as the surface water nutrient concentrations (Fig. S 7). It is not clear to me to which pattern you are referring. I presume it is the decrease in concentration at the surface. But the pattern is not very clear to me, at least for dNi. For instance, it is much clearer for dCd, which appears to me to have the clearest "nutrient-like" vertical profile. So, why are you mentioning specifically dNi and not dCd?

Yes, we understand how this is misleading.

Original sentence: The concentration of dFe and dNi in surface waters (Fig. S 9 c and e) followed a similar pattern as the surface water nutrient concentrations (Fig. S 7). The concentrations were higher close to the coast, decreased with distance along each transect and increased again towards the west of each transect. The surface water concentrations of dCd were rather uniform and increased only towards the west of each transect (Fig. S 9 g).

Proposed changes: The concentration of dFe and dNi in surface waters (Fig. S 9 c and e) were higher close to the coast, decreased with distance along each transect and increased again towards the west of each transect. The surface water concentrations of dCd were rather uniform across the shelf and increased only towards the west of each transect (Fig. S 9 g).

→ There is a difference in the coastal stations. Coastal freshwater runoff was not a source of dCd, while providing dFe and dNi.

3.4 Carbonate system parameters. Line 255 to 280. I have no concern about your general description of results, but my attention has been caught by the shift in CT:AT at the surface waters that might be addressed, especially station 23 (or 24), 14 and 05, with related impact on Revelle factor and aragonite saturation. I am wondering if you can address this striking/questioning feature.

We think this is referring to L275:

"The westernmost surface waters of each transect showed lower AT and CT values with overall highest CT:AT ratios in surface waters. Consequently, pCO2 values and Revelle factors followed this trend and were higher at these stations, whereas pH and Ω Aragonite values were lower."

We addressed this in chapter 4.4 from L500 onwards by calculating salinity-normalized AT and temperature-salinity-normalized CT. We found that even though sea ice meltwater is diluting AT and CT, it provides a small source of AT and CT to the surface waters (the sea ice endmember is not 0). Overall, the CT:AT ratios are higher, so there must be an additional source of CT, which we concluded to be remineralization under the ice* and the inflow of Pacific-origin waters with high CT values.

We concluded this in the following statement (L520):

"Our results show that the dissolution of ikaite in sea ice acted as a source of AT, providing a small geochemical buffer to meltwater-influenced surface waters (Jones et al., 2021). However, the inflow of Pacific-origin waters and remineralization under the ice* led to overall higher CT:AT ratios, resulting in higher pCO2 and lower pH and Ω Aragonite values in surface waters."

*see proposed changes above: and within the ice

Line 329 "and sea ice 330 meltwater as a dCu source". While the source of Cu from glacial freshwater seems compelling from Figure S12, this is not the case for meltwater sea ice, as the value of dCu is the same at salinity 33 and 30.5-31. I acknowledge that we can expect sea ice meltwater from sea ice, according to current understanding of trace metals in Antarctic sea ice and Arctic sea ice (Tovar-Sánchez et al., 2010). But this does not seem obvious to me in the current survey, at least from the figures S12, despite the PCAs.

Yes, this is a very good point. The influence of sea ice is evident when examining the surface water concentrations, particularly at Stations 24 and 30 (Fig. S9f) compared to the adjacent stations. The surface water concentrations of dCu are higher towards the west of Transects 4 and 5. Specifically, at Station 24, the concentrations are 206 ng/L at 3 m and 204 ng/L at 36 m, compared to Station 22, which has 157 ng/L at 6 m. Similarly, at Station 30, the concentration is 185 ng/L at 3 m, whereas at Station 28, it is 161 ng/L at 3 m.

Original sentence: The distribution of dCu correlated negatively with salinity and was influenced by coastal freshwater input and sea ice meltwater as a dCu source (Fig. S 12 d).

Proposed changes: The distribution of dCu correlated negatively with salinity and was mainly influenced by coastal freshwater input (Fig. S12d). Similarly, sea ice meltwater can be identified as a dCu source to surface waters (Fig. S9f), with dCu concentrations increasing towards the west of Transects 4 and 5.

Figure 4 a). I found it confusing to plot longitude from low to high, as this implies that the West is on the left and the East is on the right, especially as there is a map in the same figure, meaning that the East is on the left in fig 4e) and on the right in fig 4a.

We will switch the axis in 4 a) and put a compass symbol W-E and in the other diagrams S-N to improve readability and logic of the figure.

Line 410. How robust is the comparison of AOU values in WGSW and AW? It is not clear to me if you are comparing similar depths. I believe this is not the case, and you are comparing the whole data set without considering the depth. I am not comfortable with that, as depth is one of the first predictors of AOU. In addition, the AOU is also affected by air-sea exchange, which depends on ice cover. So, in my opinion, the suggestion that warmer shelf waters being more productive than the colder AW, is not well supported by the comparison of AOU, even if I expect that is indeed correct.

Yes, this is completely true. We addressed a similar point by focusing on a depth range of 30-50 m for NOx and CT. Please see below for the proposed changes, including some rearrangement of the following sentences. However, we are still discussing this point and might remove this argument.

Original sentence: Lower AOU values in WGSW (-4 µmol L-1) than in AW (36 µmol L-1) indicate that the warmer shelf waters are more productive than the colder AW.

Proposed changes: As AOU is a depth-dependent variable, we propose examining a depth range of 30 to 50 m for a better comparison between WGSW and AW. The median AOU values within this range are -6 µmol L^{-1} in WGSW and 7 µmol L^{-1} in AW. This indicates that the warmer shelf waters are more productive than the colder AW. As primary production was not measured directly in this study, we support this argument with a study by Krawczyk et al. (2021), that found the highest values of chlorophyll *a* along the south-west Greenland coast and the lowest values along Baffin Island. This is also in agreement with overall lower nutrient concentrations in WGSW than AW (refer to Sect. 4.1.2 and Table 2), as nutrients are removed from the water column through PP. Fig. 4 a shows the NOx concentrations against longitude in a depth range of 30 to 50 m, including literature data from the Green Edge cruise in 2016 (Bruyant et al., 2022). There is a clear difference between lower NOx concentrations in WGSW on the shelf and higher NOx concentrations in AW further offshore in areas that were still ice-covered. This gradient in nutrient concentrations is caused by the direction of the sea ice retreat from east to west and has been linked to higher productivity and species richness along the southern coast of west

Greenland and in Disko Bay, compared to Baffin Island and northwest Greenland (Krawczyk et al., 2021; Lafond et al., 2019). We believe that our sampling near the end of the summer season was crucial for this vast difference in water mass composition. Shelf waters had already become depleted in nutrients, while further offshore biological processes only increased recently because of the retreating sea ice (refer to Sect. 4.4). With depth, respiration returns nutrients to the water column as seen by high AOU values in WGIW (62 µmol L-1) and overall higher nutrient concentrations (refer to Table 2).

Line 597: “Thus, the benthic ecosystem of the west Greenland shelf is potentially vulnerable to future ocean acidification and suppression of CaCO3 saturation states.” There is a bit of mixing, and confusion introduced there, in my opinion. There is only on single data below the aragonite saturation at 700m. This does not correspond to the condition on the shelf. What means “suppression of CaCO3 saturation states”. Is it possible to “suppress” any saturation state? I am not a native English speaker, but the expression is a bit odd to me. I would have expected something like undersaturation in Aragonite, decrease in saturation state, but not a suppression.

The information about corrosive conditions and Ω Aragonite undersaturation is already given in L596, so we are changing the sentence.

Original sentence: Thus, the benthic ecosystem of the west Greenland shelf is potentially vulnerable to future ocean acidification and suppression of $CaCO_3$ saturation states.

Proposed changes: Thus, the benthic ecosystem of the west Greenland shelf might be negatively impacted, particularly becoming unfavorable for shell-forming marine organisms.

Line 604: I appreciated the part “We hypothesize that this return flux decreased with increasing distance 605 from the Greenlandic coast as the amount of exported biogenic and terrigenous material available for remineralization and reversible scavenging also decreased. Further studies looking into Baffin Bay sediments are needed to disclose benthic fluxes of trace elements and reactions occurring therein. Our study provides a first insight into the importance of benthic inputs for trace element cycling on the west Greenland shelf and Baffin Bay.” The evidence of the process, as derived from the PCAs is not fully compelling. I am not against the idea, but at that point, I appreciate that it is mentioned as only a suggestion.

This is only an idea so far but since a similar process was found in the East Sea, we thought this could be a hypothesis. More information about this is given in L384: “A similar observation was made for dMn, dFe, and dCo in the Ulleung Basin of the East Sea by Seo et al. (2022). Concentrations were significantly higher on the slopes and bottom layer of the Ulleung Basin in contrast to the Japan Basin, which the authors associated with a large sedimentary release by OM degradation caused by the high OM content of the shelf sediments (Seo et al., 2022).”

Figure S3. Something is wrong with the plots. A large white square masks a large part of them. I don't know where the depth scale of the plot is.

These are water column profiles of Transect 3. Station 15 is displayed separately because it is further south than the other stations. Although the depth scale is the same on both graphs, we recognize that this presentation can be confusing. We will remodel the plots to make this clearer.

I found the Table S5 odd, with values missing (max salinity station 15 to 30); Pot. Temp minimum station 8 to 17, and so on. In addition, I would have expected a single value as minimum or maximum, rather than a range of values. Per se, a minimum value is only one value. The values are given for only some stations and not all. For example, there are no salinity values for 6 to 10, but there is a maximum potential temperature value for that station, but not a minimum value. It looks a bit random. Also, I'm wondering if the table would benefit from being rotated 90°. I mean, one line per station, and then each column corresponding to a parameter (min salinity, maximum salinity…., etc.).

For this table, we examined the overall minimum and maximum values across the entire data set, specifically the 1% lowest and highest values overall. However, we understand how this presentation can be misleading. We will adapt your recommendations and create a table with the minimum and maximum values for each station.

Figure S1. This plot is made with only 2 stations parallel to the coast, while all other plots are based on transects of at least 5 stations perpendicular to the coast. I don't think that it is very useful to have it. And I'm sure the structures exhibited by the contour lines for salinity and oxygen have no physical meaning and can be removed.

We will change these plots to vertical profiles with two lines for St. 1 and 2 (similar to Fig. S 6).

Figure S9, S10 and others. I won't cut the symbol above the 0 depth. It makes the surface a little less easy to read. It looks like you applied a "blank" patch above the 0-depth line.

We will make the space above 0 m bigger.