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.

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 $\Omega_{aragonite}$ or $\Omega_{calcite}$ 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 $\Omega_{aragonite}$ as it it shown by that study, we cannot draw any conclusion on the rate of the $\Omega_{aragonite}$ 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.

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.

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.

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?

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?

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.

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.

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.

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.

Line 597: *“**Thus**, the benthic ecosystem of the west Greenland shelf is potentially vulnerable to future ocean acidification and suppression of $CaCO_3$ 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.

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.

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.

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.).

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.

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.