

Dear Reviewer 1,

**We would like to thank you for taking your valuable time to read and re-evaluate our manuscript once again. We really appreciate all you have contributed to improving this work. Thank you.**

Summary:

Thank you for your hard work to respond both reviewers' comments. I find that the revised version of the manuscript has been greatly improved and most of the concerns I brought up in the previous round review have been addressed. Although I recommend the publication of the current version of the manuscript, I still have some minor comments for the authors to consider.

Minor comments:

Line 36: ...wind-driven mixing event...

**Done.**

Line 383: How do you define the "relaxation events"? When  $\Omega_{\text{arag}}$  is  $>1$  and lasts for a few days during summertime?

**We did not use an absolute value as a definition. As stated in the manuscript, we looked at that exhibited relatively higher pH and  $\Omega_{\text{arag}}$  and lower  $p\text{CO}_2$  compared to the overall mean.**

Line 409: Figure 8a does not present temperature data

**The red line does represent temperature data (see right y-axis).**

Line 420: Figure 3? and 8

**Replaced Figure 8 by Figure 2b and c, 3a and 8**

Line 450: Figure 9?

**No, Figure 9 focuses on winter months. We added instead: (Figure 2d and 3b-d)**

Line 451: 1) O<sub>2</sub> doesn't decrease much over the ice-covered period. 2) The O<sub>2</sub> concentration data in Fig.9 from SBE63 is not accurate (too high) and misleading since only the relative change in O<sub>2</sub> was considered.

**This corresponds with what we stated:**

**At the same time, NO<sub>3</sub> slowly increased and O<sub>2</sub> decreased, which points to slow organic matter remineralization (Figure 9). Short-term variability in  $p\text{CO}_2$ , especially in January of all three observed years, was also reflected in salinity, O<sub>2</sub> and NO<sub>3</sub> (Figure 9) and**

could be attributed to advection, as the CEO site is adjacent to contrasting regimes of flow and hydrographic properties (Fang et al., 2020).

Line 489: ... the western Beaufort Seas...

**Done.**

Line 545: ...endmembers (Rheuban et al., 2019).

**(Rysgaard et al., 2007) is the right citation here since we used sea ice endmembers from them.**

Line 545-558: It is not clear how the authors conduct a Taylor decomposition analysis through they referred the method to Rheuban et al., (2019). I think it would be better to briefly introduce it in the Method section.

**We feel that we gave an extensive explanation on how we applied Rheuban et al., 2019 on line 529 through 552. The Taylor expansion is explained step by step in Rheuban et al., 2019 and can be easily applied thanks to their effort to make it understandable. We therefore don't think it is necessary to additionally explain it here, especially given the other reviewer's comment on how technical our text is.**

Figure 10: The black line needs to be defined. The different colored lines are not readable.

**We revised the figure with dashed lines and switched the red line to turquoise to make it readable to people who have issues distinguishing red from green. The black line is defined now in the caption: Contributions of changes in salinity (red), temperature (blue), biogeochemistry (pink), and freshwater mixing (green) to changes (black, relative to the mean of the timeseries) in pH,  $\Omega_{arag}$ , and pCO<sub>2</sub> were computed following Rheuban et al. (2019). The grey dotted line illustrates an estimated residual term.**

**Dear Reviewer 2,**

**We would like to thank you for taking your valuable time to read and re-evaluate our manuscript once again. We really appreciate all you have contributed to improving this work. Thank you.**

General comments

The authors respond well to reviewers' comments. However, there are a few points I'd like to ask the authors to review (see Specific Comments and Technical Corrections).

Despite the well-revised manuscript, I still have the impression that the manuscript is difficult

to read. This is probably due to the fact that technological investigations and scientific results appear together in one manuscript. Nearly half of the manuscript is devoted to observational techniques and data recovery. I agree that the description is important in examining scientific results, but too much. I recommend to divide the manuscript into a technological and a scientific part, the former being submitted as a technical paper. Or it may be better to summarize the technical description in an appendix. I strongly recommend that authors choose one of the above alternatives.

**We appreciate this comment. However, we believe that it is very important to keep the technical aspects of the observatory and analysis of the data combined with the scientific results.**

Specific comments

Are values of pCO<sub>2</sub> and pH in situ or at sea level?

**We changed the sentence to: “pH is reported in total scale and at *in situ* temperature and depth for the entirety of this paper.”**

Lines 83-84: “as a result of organic matter”. Organic matter itself does not influence pCO<sub>2</sub>. Organic matter remineralization?

**Corrected – thank you!**

Lines 386-392: By close examination of Figs. 2 and 3, the relaxation event seems to have occurred in August or later. Or not 2020 but 2018? Check this point.

**Yes – it should have been 2018 and not 2020. This is corrected.**

Lines 568-571: I feel something contradicted in this part. The assimilation implies consumption of NO<sub>3</sub>, i.e., decrease of NO<sub>3</sub>. From the NO<sub>3</sub> increase of 7.6 umol kg<sup>-1</sup>, is an increase of TA?

**Thank you for catching this. It should read:**

**However, with an observed NO<sub>3</sub> decrease of 7.6 umol kg<sup>-1</sup>, we would expect an increase of TA by 7.6 umol kg<sup>-1</sup>.**

Section 4.4: The first half of the discussion in this section seems unnecessary. The progression of ocean acidification is based on model results, but because of the short observations at the CEO site, it is unreasonable to compare model results with the CEO observations. I recommend deleting this part and limiting the discussion to the relationship between longer open water seasons and the relaxation events.

**We agree and deleted the first paragraph.**

Technical corrections:

Lines 55-56: “National Snow and Ice Data Center, 2017”

**Replace with (National Snow and Ice Data Center, DiGirolamo et al. (2022))**

Lines 78, 81 and 85: “Bates, 2015” is not found in the reference.

**The citation was added.**

Lines 100 and 1156-1163: “Moore et al., 2022”. In the reference, there are two “Moore et al., 2022”. Distinguish each other.

**The “Moore et al., 2022” sea ice paper had to be removed.**

Line 291: The published year “2017” is different from that in the reference list at line 1025.

**Thanks for pointing this out. Reference year was corrected to 2017.**

Line 313: “k1 k2”, “K1\* and K2\*”

**Done.**

Lines 345 and 346: Is “a mean of 0.0008” same to “mean difference of 0.0008”? If so, why is it repeated?

**Agreed – we deleted the parenthesis.**

Line 356: “SKQ202014S”, is this a cruise code?

**We added “cruise”.**

Line 359: “was fit to pHdisc calc”, “was fit to pCO2disccalc”?

**No, it is correct as written.**

Table 1: Latitude and longitude should be expressed with “N or S” and “E or W” respectively according to a conventional style.

**Done.**

Figure 3: The uncertainty envelope is not visible, especially in the alternating gray. I recommend changing the colors.

**We changed the background gray to yellow in both figures (2 and 3).**

Figure 10: I cannot see the colored lines, especially the blue, green, and gray lines. I recommend inserting symbols.

**We revised figure 10 to make the lines more visible. However, the blue, green and gray lines are staged on top of each other and therefore hard to visualize.**