

Reply to referee #2 of the manuscript  
"High-frequency, year-round time series of the carbonate  
chemistry in a high-Arctic fjord (Svalbard)"

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June 5, 2023

We are very grateful to referee Yuanxu Dong for his/her constructive comments which greatly improved the manuscript. Below is a point-by-point reply (**RC**: referee comment; **AR**: author reply)

**RC:** This work provides valuable long-term carbonate observations and guidance on instrument setup, data processing and quality control for the coastal ocean measurements of carbonate species. The dataset would be a great contribution to the Arctic carbon cycle community and the method will be well-welcomed by the coastal ocean community. I think it is suitable to be published in ESSD upon resolving the minor corrections below. By the way, my expertise mainly allows me to comment on the pco2 and CO2 flux-related contents. Please refer to the other reviewer's comments for the remaining parts. Also, I am not a native English speaker, but the writing looks good to me.

**AR:**

**General and specific comments:**

**RC:** Line 6: Is it possible to use the data present in this study to back-calculate the dissociation constants?

**AR:** As far as we know, this is not possible..

**RC:** Line 7: '...remains unsettled for Arctic waters'. How representative the water at the measured location for the entire Arctic waters?

**AR:** The important point is cold and low salinity waters. "Arctic" has been replaced by "polar".

**RC:** Line 7: Does the stratification related to the ocean depth? If no, suggest removing 'despite the shallow depth'. Also, I did not see any discussion about this in the main text.

**AR:** Stratification (or vertical gradient) is discussed in section

**RC:** Line 10: in the main text, the value is 17. Keep consistency.

**AR:** Now the main text says 16.8.

**RC:** Line 12: 'are understood the least'. Why the least? Any reference for this? I was thinking the Antarctic and Southern Ocean are less understood because of the remote and limited measurements.

**AR:** Agreed but we wrote "among". In any case, the sentence has been modified and now reads *Despite their major importance, Arctic shelves are among the coastal areas which are understood the least.*

**RC:** Line 20: I am curious why the Arctic SST increasing rate right now is not significantly higher than other regions considering the greatest future warming?

**AR:** This is indeed a very interesting question which goes beyond the scope of the present manuscript.

**RC:** Line 48-49: Fig. 1 A, B, C. The figure caption uses the capital A, B, C to represent the subplots. Keep consistency.

**AR:** Done.

**RC:** Line 50-51: I am wondering if this sulfuric acid will influence the pH and carbonate measurements. May quickly dilute by the water mixing?

**AR:** We have added explanation in the revised manuscript: *To prevent biofouling of the sensors, every night at 00:10, a sulfuric acid (4% for 10 min) flush of the entire sensor system was followed by a rinse with freshwater (30 min) prior to switching again to measuring mode. Data were not used for a total duration of 60 min after the initiation of the flush.*

**RC:** Figure 1: worth to check all the figure captions. Here a, b, c, d should be 1, 2, 3, 4 I think. In addition, add (C): Svalbard (A), Kongsfjorden and Ny-Ålesund (B), and (C) observational set-up. . .

**AR:** Corrected.

**RC:** Line 63: ‘The number of outliers discarded was 38 and 41’. How many observations are in total?

**AR:** This information is provided in the revised version of the manuscript. It reads: *The number of outliers discarded was 38 and 41, respectively for  $C_T$  and  $A_T$  (out of a total number of samples of 229 and 236).*

**RC:** Line 98: consider removing ‘. . .’ in the bracket.

**AR:** Done.

**RC:** Line 114: ‘The gas exchange parameterization as a function of wind speed of Ho et al. (2006) was used’. I like Ho et al. (2006), but not sure if Ho et al. (2006) parametrization is the best here at a coastal environment. First, the (Ho et al., 2006) was derived from the open ocean (Southern Ocean) environment, while the (Nightingale et al., 2000) was derived from the coastal sea (the North Sea), which may be better here. Second, the K in the very coastal area (very shallow seawater) might be different from the K in a relatively open ocean (see (Yang et al., 2019)). May not need to change the K parameterisation here, but worth to add this information.

**AR:** The referee is correct. We have added the parameterization of Dobushi et al. (2023) which may be more applicable to shallow, wind-fetch-limited environments. The text has been extensively modified. See sections 2.7 and 3.9 as well as Fig. 11.

**RC:** Line 125: not clear to me which time-series data is mentioned here.

**AR:** We are merely saying that, according to the best of our knowledge, there has been until now high frequency, multi-year time-series.

**RC:** Figure 2: B: Monthly; C: Monthly

**AR:** Done.

**RC:** Figure 4: Here I did not see pco2 plot. Does the middle panel represent pco2? In addition, "aragonite  $\Omega_a$  calculated from AT and AT", two AT, should be a typo.

**AR:** Sorry, this was the wrong figure. Now the right figure, showing the pCO<sub>2</sub> panel, is included.

**RC:** Line 173-175: Looks to me that the performance of Papadimitriou et al. (2018) and Lueker et al. (2000) is similar. Worth to explain why 'the formulations of Papadimitriou et al. (2018) performs better on our data set'.

**AR:** The referee is correct. The text has been revised as follows *In conclusion, the formulations of Lueker et al. (2000) and Papadimitriou et al. (2018) have similar performances with our dataset and generally perform better than those of Millero et al. (2002) and Sulpis et al. (2020). The formulation of Papadimitriou et al. (2018) is seldom used and the de facto standard has become the formulations of Lueker et al. (2000), which we have used in the present study.*

**RC:** Line 175: remove one right bracket.

**AR:** Done.

**RC:** Line 195: 'The relationship between the measured and calculated pCO<sub>2</sub> (blue line) is relatively poor'. What is the reason of this?

**AR:** We are unable to pinpoint a single reason.

**RC:** Figure 5: add A after 5.

**AR:** Done.

**RC:** Figure 6: explain the blue and red dots in subplots A-E. In addition, please check here 'In panels F-J, the orange lines [to add] indicate the medians, boxes show the first and third quartiles and the interquartile range'. Some typos.

**AR:** Done.

**RC:** Line 222-223: 'Temperature is lower by up to 2 °C in the deep layer from January to October and higher by up to 0.3 °C in November and December.' Lower than the surface layer, higher than the surface layer?

**AR:** Yes. Corrected accordingly.

**RC:** Figure 8: Any thoughts on why the salinity and temperature at surface is higher than at deeper water in December? Caption: A, B, C, not a, b, c.

**AR:** Done.

**RC:** Line 232: Figure 5B shows that the calculated pco2 disagree with the directly measured pco2. Not sure how good the calculation here is. Consider providing the uncertainty of the calculated pco2.

**AR:** According to the seacarb function "errors", the typical error in calculating pCO<sub>2</sub> from  $A_T$  and  $C_T$  is 14  $\mu\text{atm}$ .

**RC:** Line 236 & Figure 10: here in the text, surface pco2 > deep pco2, but figure 10 shows opposite. Please check and revise.

**AR:**

**RC:** Line 239: It would be interesting to include some discussion about the surface  $p\text{CO}_2$  & deep  $p\text{CO}_2$  scenario in December.

**AR:**

**RC:** Line 240-242: Here is quite confusing. Does the value 20 missed a '-' (i.e., -20)? 'Correcting for the underestimation of  $17 \mu\text{atm} \dots$ ', I think it is overestimation? But if the uncorrected flux is  $-20 \text{ mol m}^{-2} \text{ yr}^{-1}$ , after the  $17 \mu\text{atm}$  correction, the flux should be higher than 20 in magnitude. Please double check and clarify carefully, because this is quite important for the conclusion.

**AR:** The referee is correct: this sentence was confusing. The text now reads: *For the 9 months when data are available, monthly median  $p\text{CO}_2$  normalized at in situ temperature at 11 m vs 0-4 m are well correlated ( $r^2 = 0.81$ ) but  $p\text{CO}_2$  is higher at the surface than at 11 m, with a median difference of  $17 \mu\text{atm}$  (Fig.10).*

**RC:** Line 243-245: I guess here try to mention the entire Arctic Ocean carbon uptake. But I am wondering how representative for the Arctic Ocean this location is? I feel like this is just a coastal ocean, which is likely different from the relatively open seawater and also different from other coastal oceans

**AR:** Agreed, this is why we compare with estimates for the coastal ocean from the literature.