

***Interactive comment on* “The Seasonal Phases of an Arctic Lagoon Reveal Non-linear pH Extremes” by Cale A. Miller et al.**

Anonymous Referee #2

Received and published: 1 December 2020

The authors present the results of annual pH measurements in an Arctic lagoon. They found that salinity-pH relationship differs between observations periods, and that the lagoon acts as CO₂ source to the atmosphere during the open season. These observations and findings are interesting and valuable. I recommend that this paper be published in Biogeosciences with minor revisions.

Specific comments:

1. AT is estimated from S- AT regression line based on 3 samples and is used to calculate CO₂ flux. Possible errors in CO₂ flux are estimated by comparing estimations using S_{sensor}-AT and S_{bottle}-AT. However, S change due to mixing with seawater, mixing with river water, melting and formation of sea ice can change S-AT relationship. This is of particular concern during the melting season (open 2019) when no AT sam-

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pling was made. Also, high concentration of organic matter in some coastal regions can significantly contribute to AT. This may be important for the lagoon system. These uncertainties should be discussed in the text.

2. Also about the error in CO₂ flux. I do not understand why uncertainty associate with freshwater Sc and AT-S regression should represent lower and upper bound? How about additive errors?

3. Different pH-S relationship between two open periods is interesting but not clearly explained in the text. Open 2018 is in the fall, when vertical mixing can be enhanced by cooling and storms. Accordingly, air-sea CO₂ exchange can result in relatively stable pH (and pCO₂) despite the change in salinity. On the other hand, open 2019 is in melting season when stratification, warming, dilution, and remineralization at the bottom can largely change pH. Can this explain the difference?

4. I suggest authors to add a figure showing estimated pCO₂ time series. It would help to better understand seasonal variability in carbonate chemistry.

Minor points:

Figure 1: The station location and text in the picture are hard to see. What about changing to white? Please show locations of Arey and Jago lagoons and Beaufort Sea.

Figures 4, 6, 7: Change “iced” and “ice-cover” to “closed” to be consistent with the main text.

Figure 7: It is hard to compare position of peaks among figures. Adding vertical lines for some important frequencies should be helpful (1 and 2 d⁻¹?).

Figure 8: State “detrented pH” in the caption.

Table 1 and S1: Explain what * means.

Table S1: Add in situ salinity used for AT-Sin situ regression in the table.

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Figure S2: May be better to show original invalid data. Otherwise, this is exactly the same as Figure 2c.

L202 “April, August and June” : Change to “August 2018, April 2019, June 2019, and August 2019”

L203: Specify “physicochemical parameters”

Section 2.3 title: “Seawater chemistry and pH sensor calibration”

L230: “All salinity measurements were . . .” Does this mean “Salinity of all AT/pH samples were...”?

L234: Which temperature was used?

L300: Superscript “2” of “R2”.

L318: (Wanninkhof, 2014) should be Wanninkhof (2014).

L329-330: I could not understand how pH was detrended for open 2018 period when there was no correlation between pH and S. This may be due to my misunderstanding. Please explain how “detrended pH” was obtained in more detail.

Section 2.6 “statistical applications”: With this generic title, it is hard to understand what this section describes about. Information in this section would be better moved to the section 3.3, when this application is needed. In that case, first two sentences can be removed as they are already explained in the section 3.1.

L408: p-value <0.000 should be <0.001 . It cannot be negative.

L482: It is said that “in the open phase of 2018, pH values were observed to be >8.05 despite the striking range of S from 5 to 30.” However in figure 2, pH varies from <7.7 to 8.4 in open 2018 period.

L561-563: Please modify the text to state that cryoconcentration of DIC (and AT) itself lowers pH. Readers may misunderstand that cryoconcentration of DIC lowers pH only

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via ikaite formation.

L572-582: Some quantitative analysis is needed. According to my test calculation using CO₂sys, cryoconcentration of DIC, AT, S cannot explain observed large decrease in pH. From very high nutrient concentration in closed season, remineralization seems to have the largest impact on pH decrease. This effect can be roughly quantified from increase in nutrients with RFR. If possible, please add some quantitative analysis about ikaite formation.

L600-606: Is the winter oxygen concentration of 5 mg/L (~160 $\mu\text{mol/L}$) low enough to limit nitrification?

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