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

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We would like to thank both anonymous reviewers for their comments and time to help improve this manuscript.

1. We agree with the review that these are sources of uncertainty that should be given more attention. We have added comments regarding these sources of uncertainty in the revised manuscript. See lines 719–721 in revised manuscript.

While additional sources of uncertainty in AT estimates may be present, we have found similar salinity-AT relationships in arctic waters within the region giving us confidence in supporting the robust correlation we found in our AT measurements.

2. We would like to clarify that this method was chosen because the quantified un-

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certainty is not identified as an error but are the bounds of the flux potential. That is, quantifying the difference associated between the AT estimates yields a much greater difference than the uncertainty associated with the measurements taken to derive AT.

Since salinity values fluctuate from $\sim$ 6 to 38 during the open phase, setting the freshwater effects on the gas transfer velocity yields a lower bound that is approximately identical to those associated with the measurement uncertainty if taken as a proportion of the total uncertainty calculation for pH and applied to AT estimates.

The difference between the two regressions yields an $\sim$ 200 umol kg-1 difference in AT, so we calculated both estimates and presented the in situ salinity estimatesâĂŤwhich yielded the higher valueâĂŤas the upper bound. As mentioned, if we calculate the measurement uncertainty as a proportion of the derived total pH uncertainty and apply this to the AT estimate, the discrepancy yields a maximum value of 40 umol kg-1. This would result in a trivial flux uncertainty given the bounds applied to the upper limit and nearly identical to the estimated lower bound.

However, we have added additional text to clarify this including the uncertainty associated with the measurements if applied as a proportion to the AT estimate. See lines 332–337 in the revised manuscript.

3. We agree with the reviewer and feel we can discuss the difference between open seasons in a bit more detail. We have addressed these comments in lines 521–528 in the revised manuscript.

In addition, please see our response to Reviewer # 1 comment "b".

4. We have included the PCO2 figure as supplementary figure 3 and moved previous fig. S3 to S4.

Figure 1. We have used a new base map and labeled the adjacent lagoons.

Figure 4,6, 7: The legends and figure captions have been changed to "closed".

Figure 7: We have added gridlines to figure 7.

Figure 8: Detrended pH has been added to the caption

Table 1 and S1: We have added the definition to "*" in table 1, which is the calibration bottle sample. We have added the definition of "*" to table S1 which associates the "*" time and date to the "*" values. This indicator was added since multiple timestamps exist for surface and bottom rows.

Figure S2: Another panel with raw salinity data was added to this figure.

L. 202: This has been changed to August 2018, April 2019, June 2019

L. 203: We have provided examples such as temperature and conductivity.

Section 2.3: We have changed the section title to the one suggested by the reviewer: "Seawater chemistry and pH sensor calibration".

L. 230: We have changed this sentence to clarify we are referring to all benchtop salinity measurements. These salinity measurements refer to the discrete bottle samples collected and reported in table S1.

L. 234: All temperature measurements were recorded from the SeaFET thermistor. We amended this sentence to reflect this.

L. 300: We have changed R2 to R2.

L. 318: We have corrected this to "Wanninkhof (2014)".

L. 329-330: The detrend was performed by subtracting the mean of the best fit line. The purpose of performing a detrend here was to account for potential correlation with other parameters we did not measure in this study. Please see lines 352–355 in the revised manuscript as well as our response to reviewer 1 comment "a".

Section 2.6: We have edited the title of this section and some of its content due to our responses to reviewer 1 comment "a" and reviewer 2 comment: 329-330. We believe

the title and content of this section better describe its utility and placement.

L. 408: We have changed the p-value to < 0.001.

L. 482: We have amended this sentence to indicate "instances and the running average of pHT were found to be > 8.05 across the salinity range from 5 to 30".

L. 561-563: We have amended this sentence and the one that follows to clarify this point. See lines 595–599 in revised manuscript.

L. 572-582: This is difficult to quantify. If we assume our linear regression between salinity and AT is the same between salinity and DIC due to cyroconcentraion, derivations result in an $\sim$ 0.08 pH unit decrease where the ratio between AT:DIC is 1.0217. However, this does not account for the potential of ikiate formation which would modify the AT:DIC at a 2:1 ratio. To observe the decrease in pH found in this study, we would need to observe an AT:DIC ratio of 0.985 which cannot be completely resolved from ikaite precipitation due to saturation state thermodynamics.

If we assume a steady NH_4^+ concentration during the open season and steady accumulation from remineralization, the change between August 2018 (noting that closed began $\sim$ 21 October 2018) to April 2019 NH_4^+ at the surface is equal to $\sim$ 20 ug L-1. Assuming a stoichiometric relationship between N and C to be 16:106 this would be equal to $\sim$ 130 umol kg-1 increase in DIC over this period. While the PO_4^{3-} values are a bit anomalous, we note that these values depend on the N:P ratio in the remineralized OM and sediment flux of solutes.

We have amended this section to reflect this response including the stoichiometric approximations. See lines 615–625 in the revised manuscript.

L. 600-606: There is evidence for low concentrations of oxygen and low pH to limit nitrification rates (See Middleburg and Levin 2009 and Laverock et al. 2017). We believe our discussion regarding this is feasible, particularly given the high concentrations of ammonium found in the bottom waters and potential of oxidation of the reduced

nitrogen when O2 levels increase.

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2020-358/bg-2020-358-AC2-supplement.pdf

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-358, 2020.

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