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Biogeosciences Discussions

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

Anonymous Referee #1

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Comments to Author:

1 Overview

In this study, Miller et al. describe an entire calendar year of pH, temperature and salinity changes in the Kaktovik Lagoon, Alaska. Using time series of current speed and photosynthetically active radiation, they look for the mechanisms driving these pH changes. They then go further and, using wind speed and atmospheric pCO2 measurements, provide estimates of CO2 fluxes to the atmosphere in the ice-free period. The study reads very well, is scientifically interesting, novel, and should be published given addressing a few points detailed below. .

2 Major points

a. The pH dependency on salinity and temperature could be quantified more rigorously than with the present regression. As is, it is unclear how exactly this regression was performed, why it was done like that, etc. The approach of Hagens and Middelburg (2016), who wrote a paper on how to attribute pH variability to governing factors, seems more robust and I would encourage the authors to look in this direction. Looking at Fig. 2, it is hard to believe that salinity and pH are not correlated in the first open phase, as stated on line 372. What was the rationale for applying a 7-day running average on the pH time series? Why not doing it also on the salinity and temperature time series and look for correlations in the smoothed time series instead of in the noisy ones?

b. The authors should go further in the interpretation of the CO2 flux estimates. Clearly, PAR is not correlated with CO2 flux estimates, and something else than biology must govern the high CO2 flux variability. Looking at Fig. 9, it seems that changes in CO2 fluxes are caused by storms and other weather events, rather than biological processes. What about adding current speed (from supp. Fig. 1) on Fig. 9? It would give a better idea whether weather events induce are translated into water turbulence or not. In a shallow lagoon (4.4 meters max) such as that, storms would likely resuspend sediments, releasing DIC in the bottom waters and lead to short term CO2 efflux from the lagoon to the atmosphere. Measuring transmissivity would have helped to quantify sediment resuspension through time. The differences between the CO2 flux and the pH time series, in terms of behavior and possible controlling mechanisms, should be emphasized more in the discussion, abstract and possibly title.

c. Doing the integral over time of the CO2 flux to/from the atmosphere would allow to put a nice number on the source or sink behavior of this lagoon. Given that, another point to consider for discussion would be: as the ice extent drops in the next century due to temperature increase, how could that affect CO2 air-water fluxes in these lagoons?

d. The whole frequency analysis and its results were quite unclear to me. Specifically, it would be very helpful to indicate or highlight on Fig. 7 the range of frequencies that are representative of given natural events (e.g., tides, seasons, etc.). .

3 Minor points

Title: Consider something more meaningful/informative than “non-linear extremes”

L. 56: Define LTER

L.59: Here and elsewhere: because pH is on a log scale, it is a bit meaningless to simply give the magnitude of a pH change (see the technical note from Fassbender et al., 2020, published recently as a preprint in Biogeosciences). For instance, instead of saying “pH varied with a difference of 0.2 units” without giving the actual pH value before, say “pH varied from 7.8 to 8”.

L. 65: “Efflux” is unclear, specify in which direction it’s going. Given the large error bar, we can’t actually say that it’s going in any particular direction. . .

L. 66: Explain what are the “geomorphic differences”

L. 105: Specify “specific water mass mixing patterns”

L. 108: Acidification results in “lower” pH and saturation state, but not necessarily “low”

L. 110: Here and in the next sentence: which calcium carbonate mineral are you talking about?

L. 118: Here and elsewhere: writing pCO2 with a capital P is strange, it is usually written “pCO2”

L. 146: “Meteorological events” is too vague

L. 186: Can you show the Arey and Jago lagoons on Fig. 1?

L. 207: Were sediments also retrieved in April, June and August?

L. 235: About using the Lueker et al. constants: Dinauer and Mucci (2017) showed that carbonic acid dissociation constants (K1* and K2*) of Cai and Wang (1998) seem

to be more adapted to low-salinity environments such as this lagoon. Papadimitriou et al. (2018), Millero et al. (2002) and Sulpis et al. (2020) all obtained K1* and K2* values that are lower than those from Lueker et al. (2000) in cold waters such as those from the present lagoon. Each of these estimates, including those from Lueker et al., come with associated uncertainties, but what is rarely taken into account is the uncertainty associated with the choice of constants that one choses for an analysis, because it’s very hard to quantify. This additional “hidden” source of uncertainty should be discussed here, especially given how extreme this lagoon is in terms of temperature and salinity.

L. 279: What is a “sidelobe attenuation”?

L. 290: I didn’t get “Measurements identified as below the freezing point of water”

L. 305: Can you show the Barrow and Barter Island airport on the map in Fig. 1?

Eq. 2: what are the brackets around U2 for?

End of section 2.5: I didn’t understand what the upper/lower bound uncertainties are and what is the link with Eq. 1

Fig. 6: replace y and x by the actual variable names. Are x1 and x2 both salinity?

In the end, what were the porewater measurements for? .

4 References

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