EGUsphere, referee comment RC1
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Comment on egusphere-2022-710

Anonymous Referee #1

Referee comment on "High interannual surface *p*CO_2 variability in the Southern Canadian Arctic Archipelago's Kitikmeot Sea" by Richard Peter Sims et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-710-RC1, 2022

Review of egusphere-2022-710:

High interannual surface pCO2 variability in the Southern Canadian Arctic Archipelago's Kitikmeot Sea.
Richard P. Sims, Mohamed Ahmed, Brian J. Butterworth, Patrick J. Duke, Stephen F. Gonski, Samantha F. Jones, Kristina A. Brown, Christopher J. Mundy, William J. Williams, Brent. G. T. Else

Overview and general recommendation

The study of Sims et al. presents recent (2016-2019) underway measurements of *p*CO_2 in the Kitikmeot Sea of the Southern Canadian Arctic Archipelago. By employing a suite of sensors in a custom-built setup onboard a smaller research vessel based in the region, they were able to survey *p*CO_2 shortly after ice breakup. They also surveyed less frequented shallow bay areas where few, if any, measurements were made previously. The authors estimated the CO_2 air-sea flux and found the region to be a net sink in summer, with substantial interannual and spatial variability. The authors discuss their results in the context of data from two nearby ocean observatories (a mooring and an eddy covariance tower) on local and regional scales. The authors also discuss interannual variability and large scale seasonal trends, putting their results in context of other recent studies from the region by the authors. One of the key findings of the study is that the surface pCO_2 values at the time of ice breakup and ice melt is important in constraining the magnitude of the air-sea flux throughout the summer ice-free season.

The presented datasets (supplemeneds and to be published at Zenodo) are from an extremely data-sparse region of the Arctic and, as is also stated by the authors, constitute an important new baseline for gaining a better understanding of the role the region plays in the uptake of atmospheric CO_2. The study is important, timely, and well motivated. The manuscript is well written overall, and the presented method is sound and descriptive. General issues need further attention from the authors. Addressing these comments below in a revision, I have no reservation for the manuscript to be published in Ocean Science. Please find my major comments below, followed by some minor comments that are referred to by the line numbers in the manuscript.

Major comments

My main concern with the manuscript is the general lack of concretely identified procceses and controls that may explain the observed results, which makes the manuscript read more like a data descriptor report. Without further information from measurements of ancillary variables from the underway system or from CTD/Rosette systems in the vertical, I recognize that it is difficult to both identify and quantify controlling processes. However, I urge the authors to try to expand on this effort to make the study even more useful to the Ocean Science Community. It would be helpful if the results could be put in to context of different controlling processes, even if it means theoretical calculations and approximations. What is the expected response in pCO_2 when the temperature or salinity changes over the observed ranges? What effect would mixing of fresher waters with more saline waters have on the non-coservative behaviour of pCO_2? This exercise can be readily estimated from theoretical calculations (see minor comments). Did the authors consider applying any other models than the fitted relationship between pCO_2 and weeks since ice breakup from Ahmed et al. (2019) to explain the observed values (see minor comments)? What I am trying to convay is that it would significantly strengthen the study if the results can be put in a much more clear and quantitative context, despite missing information from additional variables. My second major concern is that the results are mainly described by relative statements without actual numbers backing up the statements (although listed in Table 1), e.g., "...there was large interannual variability...", "...was generally lower than...", "...values were much lower...", "...highly undersaturated...". This makes it difficult to digest the results in a meaningful way. Please consider including values/ranges/numbers and avoid relative statements.

Minor comments

Line 34: italicize p, subscript 2, for consistency
Line 35: Define CAA, preferably on line 21.
Lines 49-50: If possible, please provide an original reference (e.g., Jakobsson (2002)) to this areal statement as there are many different definitions around. Bates and Mathis (2009) do not include such a reference.
Line 179: "A made to order Sunburst..." reads awkward, please rewrite.
Line 189: Change "x" to the greek letter *chi*
Line 198: "processed following SOP 5 (Dickson et al., 2007)."
Lines 221-224: Any critical problems that warrants a notice in the main text?
Line 228: "...should be quite similar" Please avoid such relative statements. For example, compare observations from Barrow/Alert NOAA GML Carbon Cycle Cooperative Global Air Sampling Network. The difference between the two station means (1985-2021) is 3.8 ppm.
Line 234: The scaling factor (SF=0.24) is superfluous as it is inherently included in the calculations when the flux is given in mmol m-2 d-1, based on the given units for the gas tranfser velocity, solibility, and partial pressure difference. Suggest to omit SF as it may be confused with a scaling factor for sea-ice cover, although the study concerns open water.
Line 235: Why Nightingale et al. (2006) and not Wanninkhof (2014)/Ho et al. (2006)? Please motivate.
Figure 4: Please consider changing scale of the the y-axes for the different years. I recognize the benefit of having the same scale for all years, but at the same time it is very

difficult to make out any fine-scale patterns between the different variables.
Lines 308-309: Please mark the locations of the ONC mooring and Qikirtaarjuk Island observatory also in Figure 2.
Figure 5: Please put labels of a), b), c) in the figure.
Line 335: How is "good agreement" defined? There is no "good agreement" bewtween the EC tower and the ONC mooring October 2017?
Line 377: "Dilution by low $pCO_{2(sw)}$ ice meltwater", remove the subcripted "(sw)". Please consider undertaking the exercise of theoretical calculations on the non-conseravtive behavior of pCO_2 during the mixing of "fresh" and saline water, following Figure 11 in Meire et al. (2015). This could be useful in the discussion on how much a salinity change could/would lower the pCO_2 during mixing of waters of different salinities.
Line 381: Please clarify at which depths the Freshwater Creek plume is typically found.
Lines 387-389: The sentence starting with "On the 17th August 2017..." is very long and reads somewhat awkward. Suggest to break it up and rewrite the part "...this would point to this being due to something only happening in the Bay..."
Line 422: change to "...Ahmed et al. (2019) did..."
Line 437: Suggest changing "oversaturation" to "supersaturation" throughout the text.
Line 460: Would it be helpful to derive your own similarly fitted model (pCO_2 vs. weeks since ice breakup) for Kitikmeot Sea? Did you consider applying a different model, like the one (Figure 3) by DeGrandpre et al. (2020), to try and explain some of the observed results?
Line 468: Subscript 2
Line 499: "air-sea flux"

References

Jakobsson (2002): https://doi.org/10.1029/2001GC000302
Meire et al. (2015): https://doi.org/10.5194/bg-12-2347-2015
DeGrandpre et al. (2020): https://doi.org/10.1029/2020GL088051