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Comment on egusphere-2022-710

Wiley Evans (Referee)

Referee comment on "High interannual surface pCO_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-RC2, 2022

Review of: High interannual surface pCO2 variability in the Southern Canadian Arctic Archipelago's Kitikmeot Sea by Sims et al

Overview: The MS by Sims et al presents new data for the Canadian Arctic Archipelago in the area of the Kitikmeot Sea. The observations are largely collected from a ship-of-opportunity, but also data from a seafloor observatory platform and an eddy covariance flux tower are also presented. Four years of summer observations are used to define this region as a sink for atmospheric CO2. Notable spatial differences in the data were highlighted, as were differences from the seafloor platform and the flux tower. My impression with this study is that the data are unique but I wonder about the discussions around the comparison to the flux tower and the seafloor node, and I can't help but think about the missed opportunity to compare these new results to data in the SOCAT holdings. For instance, a quick check of SOCAT reveals there are underway surface measurements in this area from Mike DeGrandpre for the years of 2017, 2019, and 2020, not to mention the earlier data from Tim Papakyriakou. It isn't clear to me that the ONC data is directly comparable to the surface data given they are measurements from the sea floor, and without understanding the size of the "footprint" of the EC tower-determined surface pCO2, that isn't a directly obvious comparison point either. Perhaps linking to the SOCAT holdings could benefit various sections of the discussion, as well as lead to a discussion about trends beyond inter-annual variability. Discussion of the drivers of the spatial and temporal variability is pretty limited and could be strengthened as well. There are also some corrections that need to be made in the presentation of the methods. I urge the authors to consider these points in addition to my detailed comments below. This paper certainly is worthy of publication in Ocean Science after the authors address these comments, and I really liked seeing the setup on the R/V Martin Bergmann. Best of luck, Wiley Evans.

Specific comments:

ONC seafloor platform depth is reported to be 7 and 9 m on pages 3 and 19, respectively? Which is correct? Seems like a potentially big difference in the stratified Arctic.

Page 7, please report the scale used to present salinity observations.

Line 185, page 9, the need for water vapor correction stems from the fact that drying removes water vapor and this impacts the partial pressure. For instance, unadjusted pCO2 from a GO8050 (that has drying components) would a "dry air" value that needs to be "corrected" to 100% humidity using SST and salinity to compute vapor pressure and adjust pCO2 to a "wet air" value. If an analytical system does not dry, then there is no need for a water vapor correction. Therefor this statement and the application of a water vapor correction to the data is in error.

Line 199, page 9, the LI-840 does not measure pCO2. The measurement is CO2 absorption that is linearized to produce CO2 mole fractions over a broad range. See: https://www.licor.com/env/support/LI-840A/topics/theory.html. Raw xCO2 from the instrument would be calibrated using multiple reference gases, and this calibration function should be a linear fit between the reference gas concentrations and the raw xCO2– not a piece-wise linear fit. The first point here is an easy correction to the text, the second point needs addressing at the data processing level.

Page 9, the authors state in situ temperature and salinity were adjusted for "ubiquitous" skin effects when calculating "interfacial" pCO2. I believe this means pCO2@equilT was adjusted to pCO2@skinT, but is presented as pCO2@SW. Was the relationship from Takahashi et al also used for the salinity adjustment?

Page 9, The Wanninkhof 2014 relationship is used for Schmidt number but Nightingale et al 2000 was used for the gas transfer rate, why is that? Why not use Wanninkhof 2014? Also, was there good agreement between the reanalysis and locally observed wind speeds?

Page 10, expressing pCO2 uncertainty as an absolute value is a bit misleading as certainly the uncertainty is less than 8 uatm at 200 uatm and likely more than 8 at 600 uatm. Instead of “final” could say “average”? Suggest sticking to expressing uncertainty as a percentage.

Page 10, somewhere the size of the footprint of the EC tower needs to be defined. Also, what are the uncertainties in SW pCO2 determined for the EC tower? For instance, the authors use temperature and salinity from 13 m (i.e. not surface and deeper than the ONC platform) to compute Schmidt number and CO2 solubility. Does this, in addition to the spatially integrative nature of the EC tower determined SW pCO2, contribute to the reported differences from the underway pCO2 measurements. That is in addition to the surface skin effects? Given underway pCO2 was adjusted for median surface skin effects (

Page 13, Lines 281-294, 2016 doesn’t look to be “close to atmospheric equilibrium” in Figure 4, though the areal averages in Table 1 indicate conditions were closer to atmospheric levels than during the other years. I was surprised by the degree of variability during 2016 relative to the other years. Maybe this is a point that could be built on RE drivers?

Table 1 legend: the cautionary note seems a bit odd since the comparison between years is done in Discussion section 4.3. Maybe remove this statement?

Figure 5 and section 4.1: the legend states “surface pCO2 from across the Kitikmeot Sea” which I think means all the data in Figure 4. But it doesn’t look like all the data are shown. Suggest to use only data from within the EC tower footprint, whatever that is, so as to be more directly comparable. This might help clarify this section and better support the statement on Lines 330-332. The additional SOCAT data might also help in this section as well.

I don’t’ follow the comparison to the seafloor platform without some understanding of how temperature and salinity also compare. Could this be added to Figure 5?

Sections 4.2 and 4.3 would benefit from comparison with the SOCAT data holdings.

Section 4.4 title, suggest replace “sink for pCO2” with “sink for atmospheric CO2”

Data availability: while I appreciate that the authors are making their data available through Zenodo, and I applaud them for the effort, these data would be a bigger benefit to the community if they are submitted to SOCAT and NCEI. I strongly suggest the authors consider submitting these data to SOCAT.