## Reviewer 1

**Overview and general recommendation**

The study of Sims et al. presents recent (2016-2019) underway measurements of $pCO_2$ 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 $pCO_2$ 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.

**RC1.1** We thank the reviewer for their constructive feedback. We agree with the reviewer's main point and acknowledge that there should be a greater emphasis placed on quantifying the processes controlling $pCO_2$. In response to their comments, we carried out the suggested calculations drawing additional data from Back et.al 2021 for a NCP rate and Else et.al 2022 for concentrations of TA and DIC for Sea ice. We now show the impact of warming on $pCO_2$ using the Takahashi et.al 1993 equation as in Ahmed et.al 2019 and the impact of ice melt as in Meire et.al 2015 and the impact of NCP + flux as in DeGrandpre et.al 2020. This analysis has substantially improved our discussion and we thank the reviewer for pushing us in this direction. Three paragraphs will be added to the main text, these can be found copied below in the minor comments **(RC1.19)**.

**RC1.2** The reviewer also mentions using vertical data from CTD profiles; admittedly we do have a substantial amount (~100 casts) of CTD data from all four of these cruises. However, including that data was ultimately beyond the scope of this manuscript. We are currently working the accompanying CTD cruise data into another manuscript with one aim being to build on what we discuss in this paper. I am sure the reviewer can understand the need to split this work up.

**RC1.3** We did not opt to use any other models besides Ahmed et.al 2019; this is primarily because the Ahmed et.al 2019 model was trained on data specifically from the Canadian Arctic Archipelago. Ahmed et.al 2019 incorporate the majority of the available cruise data for the Kitikmeot Sea into their model (5 of the 7 cruises that pass through the Kitikmeot Sea as of SOCAT v2022, see also response to reviewer 2 (**RC2.15**)). The model of Ahmed et.al 2019 is also what is used in Ahmed et.al 2019b to calculate the region flux (that estimate remains our best estimate of the flux for the Canadian Arctic Archipelago). We now provide a best fit curve to the Bergmann data and compare the slope of this curve to the model of Ahmed et.al 2019 and show there is good agreement between both curves in the first 7 weeks after sea ice melt (**RC1.24**).

**RC1.4** The results were originally written in less descriptive terms drawing heavily on values from Table 1, the results were changed in an effort to improve readability and to reduce repetition with Table 1. The results will be changed throughout the manuscript to match this earlier version and all relative statements will be removed.

**Minor comments**

Line 34: italicize p, subscript 2, for consistency

**RC1.5** Done.

Line 35 Define CAA, preferably on line 21.

**RC1.6** Done.

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.

**RC1.7** Done.

Line 179: "A made to order Sunburst..." reads awkward, please rewrite.

**RC1.8** Now reads as "A commercially available Sunburst systems…"

Line 189: Change "x" to the greek letter *chi*

**RC1.9** Will change for all cases of $xCO_2$.

Line 198: "processed following SOP 5 (Dickson et al., 2007)."

**RC1.10** Done.

Lines 221-224: Any critical problems that warrants a notice in the main text?

**RC1.11** No serious issues that would be relevant to most readers, but there are a few things that are specifically worth highlighting for researchers who might be interested in setting up an underway $CO_2$ system on a ship of opportunity in a remote location like this (e.g. shipping gas cylinders, the ship being unheated and completely freezing in the winter etc).

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.

**RC1.12** We agree this was vague. We have compared the Barrow continuous timeseries with flask measurements from Alert and show the long term mean difference to be very small. We will add the following text.

"For example, we calculate a long term (1985-2019) mean difference of 0.246 µatm between Barrow and weekly atmospheric samples from Alert Nunavut (Lan et al., 2022)."

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 transfer velocity, solubility, 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.

**RC1.13** This is a fair point. I am generally inclined to be explicit with equations and units as that can make the work more accessible for more junior scientists. I agree with the reviewer that in this case SF could be confused with a sea ice scaling and will therefore remove it from the text.

Line 235: Why Nightingale et al. (2000) and not Wanninkhof (2014)/Ho et al. (2006)? Please motivate.

**RC1.14** Whilst the wider scientific community has started favouring Wanninkhof et.al (2014)/Ho et al. (2006) in the last few years, it is worth noting that these parameterisations are largely based on the same data points. The slopes of these parameterisations are also practically identical at the low to moderate wind speed ranges <10 $ms^{-1}$, meaning that the fluxes calculated with this data will be almost identical to if Wanninkhof et.al (2014) or Ho et al. (2006) is used. Indeed Woolf et.al 2019 states that the choice of parameterisation makes only 5-10% difference in the calculation of the global flux. He has also described Nightingale et.al 2000 as “ruling supreme” on twitter, which reflects the fact that after 20 years later it has stood the test of time well. The eddy covariance derived parameterisation of Yang et.al 2022 may even be the best choice available now. I would argue that at the moment there is not enough evidence yet to suggest Wanninkhof et.al 2014/Ho et al. 2006 are more accurate parameterisations than Nightingale et al. 2000.

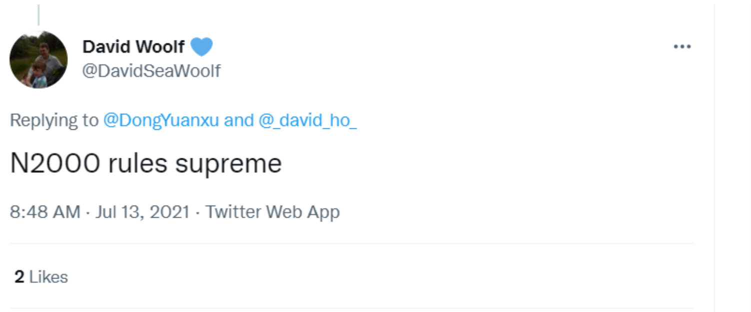


Figure 4: Please consider changing scale of the the y-axes for the different years. I recognize the benefit of having the same scale for all years, but at the same time it is very difficult to make out any fine-scale patterns between the different variables.

**RC1.15** We played around with several versions of this figure including one where the y-axes were not normalised. We really want to highlight the variability in the variables in this figure so feel it best to not edit the figure. We do see the value of being able to see the figure and make out more fine scale patterns, which is why we will include the same figure with unfixed y-axes in the supplement as Fig S7.

Lines 308-309: Please mark the locations of the ONC mooring and Qikirtaarjuk Island observatory also in Figure 2.

**RC1.16** Done.

Figure 5: Please put labels of a), b), c) in the figure.

**RC1.17** Done.

Line 335: How is "good agreement" defined? There is no "good agreement" between the EC tower and the ONC mooring October 2017?

**RC1.18** Extrapolating the 2017 August to October trend in the EC tower $pCO_{2\,(sw)}$ to the end of October (where the ONC mooring measurement resume) does bring the two measurement systems in line. Sudden changes at the EC tower on the 15th of October 2017 could point to contamination of the flux signal by early sea ice as the change is so rapid. Despite being around 1-2 weeks apart we see this as good agreement between the two systems in October 2017 and plan to add the following sentence to the text.

"(average $pCO_{2\,(sw)}$ EC tower for October 11th to 14th is 320.2 µatm and is 311.2 for October 24th to 30th) "

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.

**RC1.19** The following paragraphs will be added to the discussion.

"The processes driving the changes in $pCO_{2\,(sw)}$ can be partially quantified using back of the envelope calculations with several assumptions. The individual impact on $pCO_{2\,(sw)}$ of dilution by melting sea ice, air–sea gas exchange, net community production (NCP) and warming of seawater are explored for the month of August in 2018.

Firstly, the impact of dilution by sea ice melt can be tested by assuming conservative mixing of TA, DIC and salinity as in Meire et al. (2015). For the seawater mixing endmember, surface TA (2034.43 µmol $kg^{-1}$) and DIC (1958.82 µmol $kg^{-1}$), SST (-1.38°C) and salinity (28.64) are taken from seawater bottle data on the 18th June 2018 (Duke et al., 2021) alongside surface silicate (4 µmol $L^{-1}$ ) and phosphate (0.5 µmol $L^{-1}$) from 2018 (Back et al., 2021). Average values from spring 2019 for TA (356.60 µmol kg-1), DIC (340.24 µmol kg-1) and salinity (4.56) in first year sea ice are used for the sea ice mixing end member (Else et al., 2022). Taking a sea ice thickness of 1.8 m and assuming 10% sea ice expansion, would suggest melting all the sea ice would add 1.64 m of seawater, to reach the final salinity of 24.82 (the average recorded value from the *RV Martin Bergmann* measurements) with conservation of salinity (Meire et al., 2015) would require this freshwater to mix with 8.68 m of seawater. The ratio of these two depths can then be used to provide the predicted TA (1768.26 µmol kg-1), and DIC (1702.05 µmol $kg^{-1}$), for the seawater at a salinity of 24.82.Using CO2SYS (Lewis et al., 1998;Van Heuven et al., 2011) the calculated $pCO_{2\,(sw)}$ value for the initial seawater conditions is 368.96 µatm and after the melting of sea ice $pCO_{2\,(sw)}$ is 302.21µatm. The dissociation constants of carbonic acid used in the CO2SYS calculations were those by Mehrbach et al. (1973) refit by Dickson and Millero (1987) and the $HSO_4^-$ dissociation constants from (Dickson, 1990). For these calculations temperature was kept constant. As the average measured $pCO_2$ was 261.19 µatm in 2018, sea ice melt and conservative mixing seawater can account for the majority (66.75 µatm) of the total change in $pCO_2$ (107.77 µatm) from the initial seawater conditions

Secondly, using the same approach as DeGrandpre et al. (2020) an estimate of the individual and combined impact of air–sea exchange and net community production on $pCO_{2\ (sw)}$ can be made using a simple model with the following assumptions: taking the average flux from the 2018 cruise of -21.26 mmol $m^{-2}$ $d^{-1}$, a 40 m mixed layer depth for Dease Strait (Xu et al., 2021), with a density of ( 996.49 kg $m^{-3}$) from SST (-1.38°C) and salinity (28.64), an upper estimate of NCP (6.63 g C $m^{-2}$) which is the average integrated rate for Cambridge Bay during the open water season of 2018 (Back et al., 2021). With this configuration a change in DIC (+0.022 µmol $kg^{-1}$ $hr^{-1}$) due to air–sea exchange and net community production (-0.003 µmol $kg^{-1}$ $hr^{-1}$) can be calculated. Taking the combined change in DIC and substituting the combined change in DIC (+0.019 µmol $kg^{-1}$ $hr^{-1}$) into CO2SYS (Van Heuven et al., 2011;Lewis et al., 1998) with the same initial TA, DIC, silicate and phosphate concentrations as on the 18th June 2018, produces a $pCO_{2\ (sw)}$ change of 0.062 µatm $hr^{-1}$ for one time step. Scaling this DIC change for the month of August, with no other changes in the system, would increase $pCO_{2\ (sw)}$ by 49.7 µatm (with NCP component reducing $pCO_{2\ (sw)}$ by 9.4 µatm and air–sea exchange component increasing $pCO_{2\ (sw)}$ by 61.2 µatm).

Thirdly, using the 4.23 % °$C^{-1}$ Takahashi et al. (1993) constant, the impact of the 0.078 °C $d^{-1}$ warming trend on $pCO_{2\ (sw)}$ can be calculated for the 22 day period from July 31st to 22nd August 2018. Using the average $pCO_{2\ (sw)}$ value of 261.19 µatm and $SST_{(1m)}$ of 4.32 °C, 1.72 °C of warming would predict a $pCO_{2\ (sw)}$ of 280.90 µatm. This increase of 19.71 µatm is less than the 22 day increase of 41.80 µatm based on the 1.90 µatm $d^{-1}$ trend in the 2018 *RV Martin Bergmann* data. The impact of warming can account for approximately half of the change in $pCO_{2\ (sw)}$, the rest of the increase in $pCO2_{\ (sw)}$ could be due to air–sea gas exchange.

To summarise, modelling the processes impacting $pCO_{2\ (sw)}$ can account for much of the observed changes in $pCO_{2\ (sw)}$ in 2018. Sea ice melt can account for a 66.75 µatm decrease in $pCO_{2\ (sw)}$ equivalent to 62 % of the observed change. The warming of seawater by 1.72 °C in the first 22 days of August would increase $pCO_{2\ (sw)}$ by 19.71 µatm. Air sea gas exchange can account for a 61.2 µatm increase in $pCO_{2\ (sw)}$ in the month of August (43.4 µatm if scaled to the first 22 days). NCP can account for a 9.4 µatm decrease in $pCO_{2\ (sw)}$ in August (-6.7 µatm if scaled to the first 22 days). The actual observed change in $pCO_{2\ (sw)}$ in the first 22 days of August was 41.80 µatm which is very comparable to the combined $pCO_{2\ (sw)}$ change from these three processes 56.5 µatm. "

Line 381: Please clarify at which depths the Freshwater Creek plume is typically found.

**RC1.20** We will now state in the text the plume is typically found at < 2m and provide two references for this Duke et.al 2021 and Manning et.al 2020.

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..."

**RC1.21** The sentence will be modified to read as.

"On the 17th August 2017 $pCO_{2\ (sw)}$ was much higher (33.29 µatm) in the Bay. As measurements are similar before (8th /9th) and after (19th/20th) the 17th August, it would appear that this difference is caused by a process only occurring in the Bay; possibly the river plume."

Line 422: change to "...Ahmed et al. (2019) did..."

**RC1.22** Done, the sentence will now read as

"Interestingly, Ahmed et al. (2019) did not observe $pCO_{2\ (sw)}$ values below 300 µatm at any point during the five years of passing through the Kitikmeot Sea."

Line 437: Suggest changing "oversaturation" to "supersaturation" throughout the text.

**RC1.23** Done

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?

**RC1.24** We found this to be a helpful comment. We have now fit a curve to all of our $pCO_2$ data. The equation for which is $pCO_2$= -1.4567($X^2$)+ 19.3708(X) + 261.9529 where X is weeks since ice breakup. The slope of this line is very similar to the slope of Ahmed et.al 2019. The following text will be added to discuss this fit and how it compared to Ahmed et.al 2019.

"Fitting a quadratic equation to the *RV Martin Bergmann* $pCO_{2\ (sw)}$ observations produces the following equation: $pCO_{2\ (sw)}$ = -1.4567($X^2$)+ 19.3708(X) + 261.9529 which can be used to model $pCO_{2\ (sw)}$, where X is weeks since ice breakup. Both models predict very similar $pCO_{2\ (sw)}$ in the first seven weeks after sea ice breakup, the average difference between the models for this period is 21.1 µatm. The models differ more after 7 weeks after sea ice breakup. At 14 weeks after sea ice breakup, the model of Ahmed et .al 2019 predicts a $pCO_{2\ (sw)}$ that is 128.7 µatm higher than the model fit to the *RV Martin Bergmann* $pCO_{2\ (sw)}$ observations."

Line 468: Subscript 2

**RC1.25** Done

Line 499: "air-sea flux"

**RC1.26** Done

**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