Reviewer 2

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.

RC2.1 We thank Dr Evans for their constructive feedback. The ONC mooring and EC tower make up a large part of our discussion; however, we recognise that we have not provided all the necessary ancillary information about these observatories in the manuscript. We will now clarify the footprint size and the uncertainties going into the calculation of pCO_2 from the tower (**RC2.9**). We will also provide more information about the representativeness of the ONC mooring and the impact of the freshwater plume (**RC2.2 + RC2.14)**. The main request from the reviewer is that we include available SOCAT data. As we detail below **(RC2.15)**, Ahmed et.al 2019 provide the majority of the SOCAT data for the region (5 of the 7 cruises), and we use their pCO_2 vs weeks of open water curves as these were what was used in the Ahmed et.al 2019b flux estimate. We will clarified our wording in the methods to make it clear we followed the Dickson et.al 2007 SOP (**RC2.4, RC2.5, RC2.6)**. Reviewer 1 also highlighted that the processes driving variability could be elaborated upon. We will make the following additional calculations: the change in pCO_2 due to warming using the Takahashi et.al 1993 equation, as in Ahmed et.al 2019; the impact of ice melt as in Meire et.al 2015; and, the impact of air sea exchange and NCP as in DeGrandpre et.al 2020, see **RC1.19** for the proposed new text.

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.

RC2.2 The mooring is at a depth of 9 m but as the mooring is big the sensor depth is 7 m, for clarity only the 7 m sensor depth is discussed in the paper.

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

RC2.3 Practical salinity is reported, as this value is based on a conductivity ratio it is unitless.

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.

RC2.4 You are correct that the water vapour correction is used to correct for drying. However as the Licor measures water vapour, if the water vapour measured in the Licor is not at 100% humidity, for whatever reason, then this correction to 100% humidity in the equilibrator is still needed. Additionally the correction is additive so where humidity is already 100% the correction to 100% humidity makes no difference at all to pCO_2. You could argue that doing a correction here is redundant, but this is not an error in the approach.

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.

RC2.5 You are correct that the Licor analyses xCO_2 not pCO_2.The analyser output is given in pCO_2 which was converted back to xCO_2 for calibrations. I can see that this has caused confusion; "measured" has been replaced with "the output provided" for clarity here.

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?

RC2.6 We do not actually measure pCO_2@skin so are careful not to present the data that way. We assume that the temperature and salinity skin effects impact pCO_2 and adjust them to what we expect pCO_2 to be at the skin. Skin temperature and skin salinity values were used for all equations needing seawater temperature and salinity as recommended by

Woolf et.al 2016. Woolf et.al 2016 say that "Unfortunately, the correction for salinity is quite uncertain since it will vary according to the circumstances of the salinity change", Takahashi et.al 1993 give a value of 0.94 and Sarmiento and Gruber et.al 2006 give a value of 1, given the associated uncertainty with this correction it was decided that this should not be included.

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?

RC2.7 Nightingale et al 2000 do not provide schmidt numbers, the Wanninkhof et.al 2014 schmidt numbers are the best available. We discuss our choice of gas transfer parameterisation further in **RC1.14**. Wind speed differences between the reanalysis and the locally observed winds were not compared. Any small differences in the wind are unlikely to change the magnitude of the flux as there was a large ΔpCO_2 most of the time.

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.

RC2.8 This is a good point and final will be changed to average. We have used the ±5 µatm from DeGrandpre et al. (2020) which is an absolute value not a percentage. It is difficult to combine this with the uncertainty coming from the temperature correction which we express as a %. The reviewer correctly identifies that we have had to use a pCO_2 value of 300 µatm to get a final/average value in µatm.

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?

RC2.9 This is a very fair request and something that should be in the text or citeable. The footprint size will be provided and the uncertainty on the tower derived pCO_2 will be given in the methods as below

"A eddy covariance flux footprint is the area over which the eddy covariance measurements correspond to and varies depending on atmospheric conditions. Using the Kljun et al. (2015) footprint model Butterworth and Else (2018) show that the footprint of the Qikirtaarjuk Island observatory during spring/summer can be modelled as an ellipse with an upwind axis that varies between approximately 0.75 – 2.0 km and a cross-wind axis that varies between 0.1 – 0.2 km. The effective flux footprint is however much smaller as over 90% of the flux signal comes from within 100 m of the tower. Uncertainty in the $pCO_{2(sw)}$ values derived using eddy covariance arises from uncertainty in the flux measurements (hourly uncertainty of ~20% in the Arctic) (Dong et al., 2021a), uncertainty in the gas transfer parameterisation (~ 5–10%) (Woolf et al., 2019), the small uncertainty in the atmospheric pCO_2 value, uncertainties in k_0 and the schmidt number (including uncertainties in SST and salinity inputs from the 13 m mooring)."

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 CO_2 solubility. Does this, in addition to the spatially integrative nature of the EC tower determined SW pCO_2, contribute to the reported differences from the underway pCO_2 measurements. That is in addition to the surface skin effects? Given underway pCO_2 was adjusted for median surface skin effects.

RC2.10 The following will be added to the text

"The large differences between the methods can not be reasonably explained by changes due to SST. Accounting for a thermal skin of 0.17°C in the RV Bergmann data only alters the $pCO_{2\,(sw)}$ by about ~ 6 µatm based on the 4.23% $°C^{-1}$ Takahashi et al. (1993) constant. Similarly for the *RV Martin Bergmann* to match values from the EC tower the SST at the surface would need to be ~5 °C greater than measured by *RV Martin Bergmann* at 1 m, modelling studies suggest this is unlikely to be the case (Xu et al., 2021). If the SST from the 13 m mooring is not representative of the interface then it is possible that the schmidt number and k_0 may be biased, even if this were the case the magnitude of the impact cannot explain the $pCO_{2\,(sw)}$ differences between the methods."

To really dig into this we would need vertical information about the CO_2 system and temperature and salinity. We are currently working available CTD data up into another manuscript, including those data was unfortunately beyond the scope of this manuscript.

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?

RC2.11 We agree with this, for clarity the sentence will be changed to

"There was high interannual $pCO_{2\,(sw)}$ variability (Table 1), where average measured $pCO_{2\,(sw)}$ was closer to equilibrium with the atmosphere (404 µatm) in 2016 than in 2017 (309 µatm), 2018 (261 µatm) and 2019 (331 µatm) where it was highly undersaturated (Figure 4d).".

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?

RC2.12 Done

Figure 5 and section 4.1: the legend states "surface pCO_2 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.

RC2.13 I can confirm that all the data from figure 4 are shown on figure 5; it may appear this way because of the longer temporal sampling period of the tower and the mooring. There

was not a lot of direct overlap between the Bergmann and tower; this is why it is stated in the text every time there was overlap and what the values were.

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?

RC2.14 The following text will be added in the introduction.

"(Duke et al., 2021) confirmed that the biogeochemical measurements at the ONC site were representative of the offshore during most seasons by comparing discrete dissolved inorganic carbon (DIC) and total alkalinity (TA) samples collected at both 2 and 7 m at the ONC platform and (station) B1. Additionally, following the sea ice melt and river runoff period (2 weeks in early July), the surface stratification at ONC brakes down, and the DIC, TA, salinity, and temperature values measured then again become representative of the surface mixed layer."

Temperature and salinity from the ONC platform are presented in (Duke et al., 2021).

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

RC2.15 SOCAT v2021 were checked for relevant data during the analysis of this data. At the time there was only one cruise of data (2005 measurements from Odin provided by Fransson, A) for the region besides what is presented in Ahmed et.al 2019. The DeGrandpre data were only added to SOCAT v2022 which was released on June 14th, 6 weeks before the submission of this manuscript. Additionally, most of the data presented by DeGrandpre were collected in the Beaufort Sea and the Amundsen Gulf, only one DeGrandpre cruise does a full passage through the Kitikmeot Sea, as that was in 2020 we do not have our own data to compare against. The 2010 to 2016 Amundsen cruises provide the vast majority of the SOCAT data for the Kitikmeot Sea, even in SOCAT v2022 they account for 5 of the 7 cruises. Ahmed et.al 2019 nicely present and interpret these data, the Ahmed et.al 2019 pCO_2 vs weeks of open water curves are used as they synthesise the existing cruise data for the whole Archipelago and these same data are used for the estimation of the regional flux in Ahmed et.al 2019b.

Table from SOCAT data viewer.

expocode	platform_name	platform_type	investigators	qc_flag	socat_version	documentation	download	crossovers	qc flags
18DL**2010**0701	CCGS Amundsen	Ship	"Papakyriakou, T."	D	2019.0N	Documentation	Save As...	Check for crossovers	Examine QC Flags
18DL**2011**0718	CCGS Amundsen	Ship	"Papakyriakou, T."	D	2019.0N	Documentation	Save As...	Check for crossovers	Examine QC Flags
18DL**2014**0707	CCGS Amundsen	Ship	"Papakyriakou, T."	D	2019.0N	Documentation	Save As...	Check for crossovers	Examine QC Flags
18DL**20**	CCGS	Ship	"Papakyri	D	2019.0	Docume	Save	Check for	Examine

150417	Amundsen		akou, T."		N	ntation	As...	crossovers	QC Flags
18DL**20 16**0802	CCGS Amundsen	Ship	"Papakyri akou, T."	D	2019.0 N	Docume ntation	Save As...	Check for crossovers	Examine QC Flags
18SN**20 20**0907	Louis S. St-Laurent	Ship	"DeGrand pre, M."	D	2022.0 N	Docume ntation	Save As...	Check for crossovers	Examine QC Flags
77DN**20 05**0720	Oden	Ship	"Fransson , A."	D	3.0N	Docume ntation	Save As...	Check for crossovers	Examine QC Flags

Section 4.4 title, suggest replace "sink for pCO_2" with "sink for atmospheric CO_2"

RC2.16 Done

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.

RC2.17 We are committed to making these data available and have always planned to submit the data to SOCAT after peer review.