<u>Review RC1 (Sébastien Moreau)</u>

Review of the paper entitled "Variability in sea ice carbonate chemistry: A case study comparing the importance of ikaite precipitation, bottom ice algae, and currents across an invisible polynya" by Else and co-authors

General comments:

I commend the authors on a very interesting paper describing the variability in the sea ice carbonate chemistry between 2 Arctic sites, one of which being an invisible polynya. In particular the authors collected several cores at each site to perform a statistical comparison of the carbonate chemistry in the various sea ice horizons. They observed differences in the upper and bottom part of the sea ice mostly. The authors also observed that biogeochemical processes such as air-ice CO_2 fluxes, ikaite precipitation and primary production only played a little role in the distribution of carbonate species, and that physical processes linked to ice growth and brine drainage played the most important role. This is a very interesting and nicely written paper that contributes to the understanding of carbonate chemistry in the sea ice and, thus, the role of sea ice in the carbon cycle of polar regions. I give below numerous minor comments to improve the manuscript. Therefore, I suggest publication in The Cryosphere if the authors can improve the few minor corrections given below. I hope these comments will help the authors to strengthen this very interesting manuscript, as it will bring a significant contribution to the understanding of the sea ice biogeochemistry and the role of sea ice in the carbon cycle of polar regions.

Best regards,
Sébastien Moreau
Norwegian Polar Institute
Tromsø, 9007, Norway

Specific Comments:

Abstract:
RC1.01 Line 31: "biology did not have a noticeable impact", in fact as I commented below in the discussion, it is possible that primary production and respiration had opposite effects on the carbonate chemistry at the bottom of the sea ice, which led to this lack of detectable effects of biology on the carbonate chemistry. I suggested below that you investigate regressions lines only for the bottom ice section in your Figure 8.

We took this suggestion, and it significantly aided our interpretation of the bottom ice horizon. Please see our response to comment RC1.14, but more importantly our response to Reviewer 2's comment RC2.01, which was similar. For the abstract, we adjusted the line in question to read:

"Carbonate chemistry in the bottom horizon could largely be explained by the salinity distribution, with the strong currents at the polynya site potentially playing a role in desalinisation; biology appeared to exert only a minor control, with some evidence that the ice algae community was net heterotrophic."

Introduction
The introduction is very nicely organized, nicely written and a pleasure to read.

RC1.02 Line 89: we did show that this outflux of CO2 at the surface of the ice was due to the lack of permeability of sea ice below this few centimeter-thick layer (Moreau et al., 2015). Moreau, S., M. Vancoppenolle, B. Delille, J.-L. Tison, J. Zhou, M. Kotovitch, D. N. Thomas, N.-X. Geilfus, and H.

Goosse (2015), Drivers of inorganic carbon dynamics in first-year sea ice: A model study, J. Geophys. Res. Oceans, 120, doi:10.1002/2014JC010388.

Thanks, this paper does present a good discussion of the controls of outgassing. Manuscript has been revised to read:

"During ice formation, some brine is rejected upwards, allowing CO_2 to offgass to the atmosphere (Geilfus et al., 2013; Nomura et al., 2014). In the frazil crystal structures of surface ice, the high brine fraction creates high enough permeability (relative to layers immediately below it) for this outgassing to occur over a layer several centimetres thick (Moreau et al., 2015)."

RC1.03 Line 108: needs a space "31in the Arctic"

Correction made.

RC1.04 Line 115: "in all cases, this limited effect was primarily due to the lack of deep brine convection in winter most Arctic and Antarctic regions". This sentence is a bit misleading I find. For the Southern Ocean, oceanographic studies have only identified the formation of Antarctic Bottom waters (AABW) in four localized areas which are the Ross Sea, the Metz polynya, Cape Darnley and the southwestern Weddell Sea. Both NEMO-LIM and MPIOM/HAMOCC produce deep winter convection events in these areas in the Arctic and Southern Oceans, which is consistent with the following sentence (line 115 to 118). So perhaps you could rephrase this sentence a bit.

Yes, we see the point the reviewer is making. That sentence did leave the wrong impression. We have revised this section to read:

"Using a one-dimensional model with more detailed biogeochemical processes, Mortenson et al. (2018) similarly found that ikaite precipitation and dissolution did not have a large impact on annual air-sea CO_2 flux budgets due to a lack of deep convection at their study site. However, both Grimm et al. (2016) and Moreau et al. (2016) concluded that the sea ice carbon pump is important in specific polar regions (i.e., deep water formation areas), and could be important on long timescales (e.g., during the glacial/interglacial transitions)."

Materials and Methods

RC1.05 Line 145: can you give a range of the nitrate concentrations typically observed there?

Yes, we have added the following:

"The region is severely nutrient limited, with nitrate concentrations amongst the lowest measured anywhere in the Arctic (1.0 – 1.2 mmol L^{-1} beneath the pycnocline in summer (Back et al., 2021); 1.3 mmol L^{-1} beneath sea ice (Dalman et al., 2019)), however a modest bottom-ice algae bloom still occurs in the spring, peaking in late-May (Campbell et al., 2016)."

RC1.06 Line 184: what is the final HgCl2 concentration?

We added the following, which is consistent with how the Dickson SOP describes HgCl additions:

"Samples were then fixed with 80 uL of saturated mercuric chloride (0.05% of the total sample volume, as per Dickson et al., (2007)) to prevent biological activity, and sealed using chlorobutyl-isoprene rubber stoppers and aluminium caps (Jiang et al., 2008)."

Results

RC1.07 In Table 1, the bottom Chl-a is reported in mg/m2 instead of concentration. Can you explain in the Materials and Methods section how you converted the measured Chl-a concentration to an integrated biomass?

We have added the following note to the methods section:

"*Concentrations were converted to biomass (e.g. Chl a expressed as mg m*$^{-2}$*) by multiplying by the length of the core section collected (in this case, 5 cm or 0.05 m).*"

RC1.08 Table 2 is the same as Table 1 while I understand it should present the summary of biogeochemical variables.

Wow, we are very sorry about that. The updated table should be as follows:

Table 2: Summary of biogeochemical variables measured at the two sites, across the three horizons. Mean (x̄) and standard deviation (σ) are presented. Sample depth is the mid-point of the core sections collected for each horizon. For each horizon, at each site, *n*=15.

			Thick FYI (TFYI)					Polynya (POLY)				
			Sample Depth (cm)	Salinity (PSU)	TIC (µmol kg^{-1})	TA (µmol kg^{-1})	TA:TIC	Sample Depth (cm)	Salinity (PSU)	TIC (µmol kg^{-1})	TA (µmol kg^{-1})	TA:TIC
HORIZON	Top	x̄	5	5.43	281.63	333.24	1.18	5	6.79	343.33	403.56	1.17
		σ	0	0.42	21.98	30.16	0.05	0	1.18	61.83	78.13	0.05
	Middle	x̄	89	4.63	331.55	345.91	1.05	73	4.88	341.89	363.22	1.06
		σ	4	0.46	29.77	31.39	0.03	8	0.90	34.60	40.88	0.04
	Bottom	x̄	176	7.89	538.37	551.60	1.02	142	6.73	450.93	478.71	1.06
		σ	9	0.39	20.55	26.08	0.02	16	0.71	52.45	51.65	0.03

Discussion

RC1.09 Line 317: here you refer to Chl-a concentrations rather than biomass, so I would suggest to keep it consistent, using either concentrations or biomass throughout the manuscript.

Thank you, good point. Given that we are reporting in mg m-2, we have made the adjustment to biomass throughout the document.

RC1.10 Line 327-342: could surface flooding of sea ice be another explanation for the higher salinity, TIC and TA observed in the POLY site? Or do you think it is not a plausible explanation?

It's plausible enough to be worth mentioning. We have added the following:

"Another potential cause of differences at the surface between sites is flooding, the process that occurs when relatively thick snow depresses the ice surface below the freeboard level, allowing seawater to flow over the ice. Flooding is less common in the Arctic than the Antarctic due to relatively thinner snowpacks and thicker ice (Provost et al., 2017), and during our study period we observed positive freeboard at all sites. But given the deeper snowpack at POLY, it is possible that flooding occurred earlier in the season when the ice was still quite thin. As described by Eicken et al. (1992), this would lead to higher surface salinity."

RC1.11 Line 372: miss the citations dates in "(e.g., Nomura, Nomura)"

This has been corrected, and the following references have been added to the reference list:

Nomura, D., Yshikawa-Inoue, H., Toyota, T.: The effect of sea ice growth on air-sea CO2 flux in a tank experiment, Tellus B: Chemical and Physical Meteorology, 58(5) 418-426, 2006.
Nomura, D., Yoshikawa-Inoue, H., Toyota, T., Shirasawa, K.: Effects of snow, snowmelting and refreezing processes on air-sea ice CO_2 flux, Journal of Glaciology, 56(196) 262-270, 2010.

RC1.12 Line 360-379 and Figure 7: this is a very interesting result and challenges our typical view of the precipitation of ikaite crystals in sea ice. I agree with you that ikaite crystals must have been displaced to explain the lower TA with respect to salinity in the upper part of the ice. Perhaps you should also explain to readers that, if present, ikaite crystals would have dissolved during the melting of the ice, which would have taken the measured TA values closer to the theoretical dilution line.

We thought it would be best to add this to the methods section, and added the following text:

"At room temperature, ikaite crystals are highly unstable and dissolve back into the meltwater, which is why samples collected using this method are best described as characterizing "total" (as opposed to dissolved) inorganic carbon."

RC1.13 Line 433: remove to from "a very similar to slope to the ones"

Correction made.

RC1.14 Line 436-438: it's also possible that bacterial respiration acted in the opposite way than photosynthesis, keeping the TIC values higher. Perhaps you could also mention this hypothesis.
In fact, when looking at Figure 8, I wonder if the regression lines would be different if only considering the bottom ice sections? Would it be closer to the theoretical line for photosynthesis/respiration effects on TIC and TA? Perhaps you could add these specific bottom sea ice regression lines to the Figure as well?

Reviewer 2 asked made a similar suggestion, and it is a very good point. I will summarize our response here, but please see our response to comment RC2.01 for complete details:

-It was difficult to put all the regression lines on Fig. 8, so we made a new table (now Table 4) which reports the slope at each horizon. You can find this table in our response to RC2.01.
-In sections 4.4 and 4.5, we included a discussion of the different ice horizons, which did clarify the role of biology (actually suggesting a stronger impact of respiration) on the bottom ice horizon. The new text is in our response to RC2.01.

This was a very useful suggestion, we are glad both reviewers brought it up.

RC1.15 Line 458 and Figure 8: could you also indicate the values of the slopes for the regression lines on the figure itself?

Correction made

RC1.16 Line 464: this is then probably due to the loss of the ikaite crystals which you described convincingly in the first section of the discussion.

This section has been changes slightly in response to Reviewer 2's comment RC2.01. However, we have also added the following sentence which directly addresses this point:

"Or perhaps more precisely, ikaite crystals are lost to the underlying seawater at a similar rate to brine rejection."

RC1.17 Line 472-473: this sentence is missing words it seems "when small-scale heterogeneity is accounted for by averaging the results can be surprisingly similar."

We have re-written this sentence as:

"What our results show is that despite large differences in the physical and biological characteristics between two locations, when small-scale heterogeneity is accounted for by averaging the results can be surprisingly similar."

Figures:
RC1.18 Figure 1: a zoom-out insert would be nice to have to place Cambridge Bay on a larger map. In addition, the text is very small in the figure, and so are the transect and stations dots.

This figure and its caption have been revised to address these suggestions:

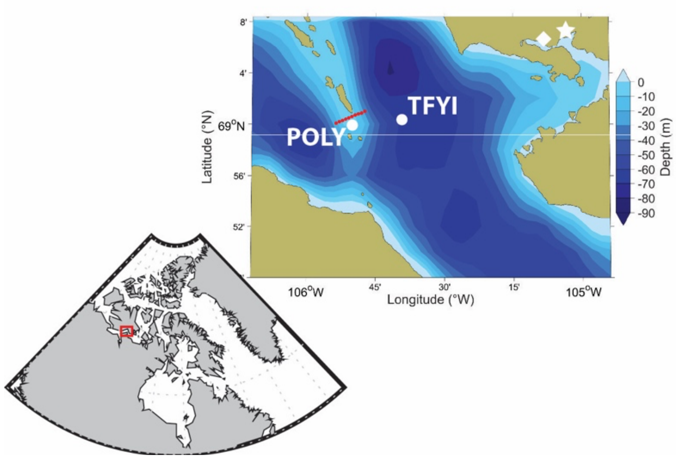

Figure 1: Study area (including bathymetry), showing the location of the two sampling sites. The thick first year ice site (TFYI) was located at 69.01°N, 105.66°W. The polynya site (POLY) was located at 68.99°N, 105.84°W. The proximity to Cambridge Bay (white star), the nearest Environment Canada weather station (white diamond), and the snow depth/ice thickness transect (red circles) is also shown.