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:

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.

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

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.

Line 108: needs a space "*31in the Arctic*"

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.

Materials and Methods

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

Line 184: what is the final $HgCl_2$ concentration?

Results

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?

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

Discussion

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.

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?

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

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.

Line 433: remove to from "*a very similar to slope to the ones*"

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?

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

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.

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.*"

Figures:

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.