Interactive comment on "Inorganic carbon dynamics of melt pond-covered first year sea ice in the Canadian Arctic" by N.-X. Geilfus et al.

Anonymous Referee #2

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This paper describes an interesting and well-conceived project of high scientific value. The study is principally well conceived and considering the locality and limitations of sea ice research has yielded interesting results. I have the following comments and suggestions:

Abstract.
The summary is concise but needs language correction there are two points, which I think are misleading. Melt ponds are a product of snow melting as well as sea ice melting. See your introduction. There should be some mention as to the role or not of the biology in the melt pond or around it. See Results and discussion.

→*As mention in the introduction (P7487 L16-17), melt ponds are mainly due to the melt of snow, and, in advanced stage of melt, due to sea ice melt. In the abstract, we add this notion, as asked by the reviewer. Now we can read: 'As sea ice melt progresses, melt ponds form, mainly from melted snow, leading to a low in situ melt pond pCO_2 (36 µatm). The percolation of this low pCO_2 melt water into the sea ice matrix dilutes the brine resulting in a strong decrease of the in situ brine pCO_2 (to 20 µatm).' We don't think we should mention the role of the biology in the abstract as the role of biology, in this study, has been suggested to play a minor role in the inorganic carbon dynamic. We believe that the abstract should remain focus on the main process describe in the manuscript.*

Introduction.
The introduction is fine showing a good review of the literature, and provides a solid background. However I find that the aspect that melt ponds are sometimes highly productive sites, biologically speaking is not accounted for in the introduction e.g. Phytoplankton production from melting ponds on Arctic sea ice: Lee et al. 2012.

→*We don't believe we should go for a complete review of the melt pond productivity in our introduction as the manuscript is not focused on this subject. However, the concentration reported in Lee et al 2012 ranged from 0.1 to 2.9 mg Chl a m^{-3} or 0.1 to 2.9 µg L^{-1}. The range of concentration we observed in this study is 1.11 to 23.15 µg L^{-1} in bulk ice and 0.08 to 0.41 µg L^{-1} in melt ponds. As the figure 9 suggests, the role of biology in our study was minor compared to other processes (dilution and dissolution of calcium carbonate). Therefore, we will keep the introduction focus on the main processes.*

Study site, material and methods:
The study site appears to be representative of typical Arctic sea ice, as described. Methods described are fine, although I find the way the authors arrive at the average ice thickness disconcerting. To determine the ice thickness from cores does not account for the tremendous irregularity of sea ice and especially under ice topography. This is particularly crucial when the data are used to assess melting rates or growth of sea ice. The authors

should mention these constraints. A simple transect done with an auger and tape could have improved the results considerably.

→*It is true, a simple transect will do the job more properly to estimate the ice thickness. However, a simple transect done every 3-4 days on the sampling site will ruined the all site by accelerating the melt. Off course we could do this transect at a respectable distance from the sampling site, but then someone, somehow will ask about the legitimacy at these measurements, outside of our sampling site. Moreover, during our sampling, we took ice core for: temperature – salinity – bulk ice pCO_2 – TA and TCO_2 (results described in our study) and additional core for others studies. So our estimations of the ice thickness is already done based on, at least 4 ice cores, on our sampling site.*

The fact that biological activity was not really accounted for is also disconcerting. Particularly since samples were apparently poisoned to curtail biological activity. Reference to some measurements done under the ice Page 7501 lines 26-29 is not all that convincing or adequate. See Results.

→*Poisoned samples are standard procedure to store samples for TA, TCO_2 before analysis. And we can't really count on the possibility that we will have no biology to avoid this procedure.*

Results:
Judging by the type of fieldwork involved in this study, the authors have done a good job to compile an excellent data set.

→*Thank you*

See my comments on the determination of ice thickness.
The authors need to reflect on the deficiencies regarding their measurements. See comments regarding the biological properties and lack of measurements
→*Chl a has been measured during our survey and range of concentration are provided in the manuscript (same range of concentration as the Lee et al (2012) paper suggested by the reviewer). However, as suggested by the figure 5, the brine pCO_2 changed independently of the variation of Chl a and according to the figure 9, the main processes responsible for the changed in TA and TCO_2 observed in the study are the dissolution of ikaite crystals and the uptake of CO_2. Therefore this study confirms that biology has a minor role in the inorganic carbon dynamic during the melt of the ice, as already suggested by Søgaard et al (2013) and Glud et al (2014).*
Now we can read, in the discussion: 'The mean concentration of algal biomass (Chl a) in bulk sea ice decreased from decreased from 23.15 $\mu g\ L^{-1}$ in June 4 to 1.11 $\mu g\ L^{-1}$ on June 12 and Chl a concentration in melt ponds ranged from 0.08 to 0.41 $\mu g\ L^{-1}$ (unpublished data, C. Mundy and V. Galindo). The loss of the biomass could result from the warming and melting of the ice [Zeebe et al., 1996; Galindo et al., 2014]. These concentrations are in the same range as those reported by Mundy et al. [2011] and Geilfus et al. [2012b] on melting landfast sea ice in the Beaufort Sea. From the brine profiles in Figure 5 and from the trend of the sea ice samples in Figure 9, it looks like brine dilution and calcium carbonate dissolution are the main factors controlling CO_2 exchange during our observation period. It should however be underlined that most of the calcium carbonate dissolution trend holds in only 4-5 samples located in the top 20

cm of the sea ice cover. In the lowest range of nTA and $nTCO_2$ (< 500 µmol kg^{-1}) which correspond to 80% of the sea ice cover (including the bottom Chl a rich 10 cm layer), the ice samples pull the trend to the left of the calcium carbonate dissolution line, suggesting an increasing influence of the algal CO_2 uptakes, strong enough to maintain the bottom ice and brines pCO_2 at low values, in the close vicinity of the nearly saturated water values at the ice-water interface. This biological effect on the TCO_2 is however probably limited to the decaying very bottom section of the sea ice cover [Søgaard et al., 2013; Glud et al., 2014].'

Discussion:
As mentioned previously I find that the omission of biological data or properties weakens the paper. Even if it were only to relate to these in the discussion and consider the significance or insignificance with potential consequences for the budgeting the authors carry out.

→*See previous comment.*

The discussion is good but there are quite a few language errors, which I have not pointed out specifically considering that there are mother tongue authors involved in this publication.

→*Hopefully we did the appropriate correction.*

The point that biological properties apparently only marginally affect the carbonate system, which is based on data from the literature, may in fact be so but this needs more precise discussion. The problem is that it appears that the only biological data obtained were by Mundy. What about the melt ponds?

→ *See respond above.*

I think that the discussion can be condensed somewhat by reducing the number of assumptions.

Conclusion:
Again I think that the conclusions, as the discussion would benefit from reducing the assumptions and speculation where possible.

Figures:
Figures are ok

Literature ok