

Anonymous Referee #3

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Author Response: We sincerely thank Referee #3 for their thoughtful and thorough comments that have improved the paper. We have added statements in blue below that detail our response to each comment. All the comments and their corrections were minor in nature, and we have added a few clarifying statements in the introduction, method and discussion

General comments

In this study Bates et al. presents measurements from melt ponds in the Arctic Ocean, and the observations provide insight into the carbon chemistry in these relatively large areas during the melting period. They show that these melt ponds may, in general, act as sources of CO₂ to the atmosphere, with some observations of very high pCO₂- concentrations, although a large variability among the different sites also were observed. They also show that water below the ice is characterized by a low pCO₂ due to a relatively large alkalinity, in accordance with previous studies of carbon chemistry in sea ice. The presented observations of melt pond and below ice concentrations are very interesting.

However, this study also includes a section of model calculations of sea ice concentration which is very speculative. The description of the model calculations are not clear, as described in detail below, and the applied assumptions about under-ice conditions are not supported by the presented data, but only referred to as "fairly representative". However, the model calculations and the involved water types are critical for the model results and, therefore, these should be explained more carefully and supported by data or be modified accordingly. Also, a summary figure of a number of previous studies is presented without describing how the calculations were made or how the seasonal coverage was determined although it is a simplified presentation of results from very different studies. Thus, I find that the simplified model calculations of sea ice concentrations needs to be clarified and the results modified accordingly and also that the presentation of previous studies should be clarified and modified. Therefore, I cannot recommend publication of the manuscript in Biogeosciences in its present form.

We thank the reviewer for their comments. In the revised manuscript we have added detail to the mass-balance approach, and assessments of under-ice conditions. The approach is a simple mass-balance (given the relatively small amount of sea-ice stations) and as we any approach (mass-balance/dynamical model) has it's caveats. We have clarified and explained more carefully the sources of the terms used, and modified the summary figure accordingly.

Specific comments

p 1103, 114: The definition of total alkalinity is not complete, please clarify that terms are missing. Also there are errors in the equation: H⁻ should be H⁺ and B(OH)⁻ should be B(OH)⁻⁴.

A reference to the fact that this is a simplified equation of TA is given and borate corrected.

p 1103, 124: comment on nitrate sources are inconsistent with the incomplete definition of TA,

cf. the comment above on the definition of TA.

This is corrected in revised text.

p 1112 The model description should be clarified. How is salinity used in the calculations and also the usage of the percentage sign is not a common notation. The expression that "% proportion of seawater and melt water is expressed here with a range of 0.00 to 1." is confusing, and the usage of "%" here should be avoided. There is a reference to both "melt water" and "sea ice melt" water and if it is sea ice melt water how can the assumption of a salinity of zero salinity for the sea ice be justified? If the sea ice salinity was applied it would change the fraction calculations. This is a basic assumption in the model and it needs to be clarified.

In the revised paper, we have clarified and explained more carefully the usage of the terms used.

p1112, 125: It is assumed that the mixed layer water at 30 m was "fairly representative". Please specify what this means.

This is the mean mixed layer depth beneath the sea-ice stations based on water-column profiles. This is clarified in the text.

p1112, 125: A reference is made to the "residuals" in the equations but there are no residuals in equation 5 and 6, please clarify. Also explain why an iterative method was necessary.

We have clarified this statement in the revised paper.

p. 1112: It seems that it would be more appropriate to measure the concentrations in the sea ice rather than calculate it from a model. A comment about why this approach was taken would be interesting.

A few sea-ice cores were measured, but only DIC was determined due to the small volume of sample recovered. Unfortunately, we had insufficient sample volume to analyze for TA on board the ship soon after sampling (and there are issues with transferring DIC/TA samples from sealed melt bags to glass bottles).

p. 1113: The model equations are missing.

The mass balance equations are included

p1113, 18: it is assumed that DIC and TA below the mixed layer at 30 m depth at each station are representative of winter/early spring water on the shelf. There is no support for this assumption. I would expect that the variability is rather large in this area, so is this a valid assumption? Please support this assumption further.

We have expanded on this statement to illustrate previous and concurrent data that indicate that subsurface waters below 30 m are representative of winter-early spring water that have not undergone substantial biogeochemical modification with regard to T, S, DIC and TA. There will of course be variability and caveats to this approach, but the errors in the DIC/TA and T/S space of this representative "winter/early spring" water were propagated into the mass balance approach. Several presentations at the OSM 2014 meeting in Hawaii and papers in review discussing the ICESCAPE project also indicated from T,S, nutrient data/models, that the waters (subsurface; >30 m deep)

beneath the Chukchi Sea sea-ice stations was representative of remnant winter water still present on the shelf at the time of sampling.

p1113, 111: The calculation of the excess DIC and TA in the interface water rely on the assumptions discussed above. Also a reference is made to the mixed layer, but how is the vertical stratification below the ice? How deep is the mixed layer and how does it compare to the values used in the model below the mixed layer? More information is needed about these calculations because the residual from the simple model calculation is subsequently interpreted as a sink or source term of DIC or TA.

We have clarified details in the revised paper, including details on vertical stratification, mixed layer depth.

Figure 1: The explanation of this conceptual figure is superficial. In Figure 1A a flux from the ice to the melt-pond is shown - please explain? What is the meaning of the dashed lines - does the mixed layer not reach the terminus of the ice-shelf? Is sea-ice melt only occurring at the front of the ice-shelf and not below? Is the interface melt water trapped in a hollow below the ice - was this observed? Figure 1B, please explain the flux from the melt pond to the ice. In principle, elevated or lowered pCO₂ can be explained with both net-heterotrophy or autotrophy, so please explain why the two situations imply the shown distributions of biotic sinks and sources? I find that this figure needs to be clarified, and potentially simplified.

The figure's conceptual framework is clarified in the revised paper to make it easier to understand.

Figure 11: A) The figure contains many processes and question marks. Also it would be illustrative to show that the ice density is close to the water density in the drawing of floating ice.

The figure's conceptual framework is clarified in the revised paper to make it easier to understand

Figure 11 B) Several of the cited studies include sea ice observations from various regions and periods, and the simplifications of all these studies in this figure requires a detailed explanation of how this was done. Some of these studies are represented in a way that are misleading, at the best.

Additional detail is included in the revised paper. There is little data in the literature on sea-ice and only one other paper that reports "limited" data on melt ponds. After this paper was submitted, another paper on Greenland sea ice was published (Sogaard et al., 2013), and this will allow additional comparisons to be made. The calculations are included in the revised paper, since we had to used reported data to compute pCo₂ and pH, for example.

Technical comments

In general, I find that the figures are very difficult to read in the printed version due to very small font sizes.

The figure labels and sizes have been adjusted in the revised paper to account for the size of a print ready version.