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Interactive comment on "Sea-ice melt CO_2-carbonate chemistry in the western Arctic Ocean: meltwater contributions to air-sea CO_2 gas exchange, mixed layer properties and rates of net community production under sea ice" *by* N. R. Bates et al.

Anonymous Referee #3

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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 can not recommend publication of the manuscript in Biegeosciences in its present form.

Specific comments

p 1103, l14: 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.

p 1103, l24: comment on nitrate sources are inconsistent with the incomplete definition of TA, cf. the comment above on the definition of TA.

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

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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.

p1112, l25: It is assumed that the mixed layer water at 30 m was "fairly representative". Please specify what this means.

p1112, l25: 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.

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.

p. 1113: The model equations are missing.

p1113, l8: 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.

p1113, l 11: 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.

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.

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.

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.

Technical comments

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

Interactive comment on Biogeosciences Discuss., 11, 1097, 2014.

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