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Interactive comment on “Sea-ice melt CO₂-carbonate chemistry in the western Arctic Ocean: meltwater contributions to air-sea CO₂ gas exchange, mixed layer properties and rates of net community production under sea ice” by N. R. Bates et al.

A. Olsen (Referee)

are.olsen@gfi.uib.no

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This is a well organised and well written contribution on interactions between sea ice and ocean carbon processes in the Arctic. I recommend that it can be published with minor revisions.

My comments are:

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page 1099

line 13, insert "the" in front of "observed"

line 19, replace "CaCO₃" with "carbonate".

page 1100

line 16-24, this paragraph is yet another example of how the concepts of anthropogenic CO₂ and CO₂ are jumbled together. So far we have no proof that the Arctic has a large air-to-sea flux of anthropogenic CO₂ - which is the message conveyed by the first sentence of this paragraph. Rather on the contrary, the air-sea flux appears low, while most of the anthropogenic CO₂ is transported into the region with the ocean currents (e.g. Jeansson et al, 2011). And the "low buffering capacity", mentioned does not facilitate uptake of CO₂ from the atmosphere. In fact, warmer waters have the higher buffering capacity and hence are more receptive for anthropogenic CO₂ than colder waters.

The authors should be well aware of these concepts and I encourage them to make a clearer distinction between natural and anthropogenic carbon in this paragraph.

page 1101

line 15, replace "a" with "are".

line 23, Omar et al. (2005) is also an appropriate citation here.

page 1103

Equation (2), please state that this is a simplified definition, and cite full definition, of Dickson et al., (2007) for instance.

line 15, insert "[B(OH)-]" between "where" and "is".

page 1105-1106.

I am somewhat concerned about the accuracy and precision of alkalinity data. It stated

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in the text that an (open-cell) VINDTA was used. This follow the Standard Operating Protocol 3b of Dickson et al. (2007), which is "...suitable for assaying oceanic levels of total alkalinity (2000-2500 umol/kg)". This is much higher than some of the levels encountered here. Please state what - if any - adaptions were made to method to ensure accuracy in low salinity sea-ice melt. Following on to that, CRMs were used to calibrate the measurements of TA. I am concerned whether the calibration factors are transferable over the wide range of ionic strength from high salinity Pacific waters of the CRMs to low salinity sea-ice melt waters.

Given these issues, the reported TA may very well have an accuracy and precision poorer than the 2 ± 0.5 umol/kg reported here. Please assess these issues, and propagate their consequences for uncertainty of calculated pH, pCO₂ and Omega and CO₂ flux values presented later in the paper.

page 1106

Lines 11, dissociation constants of Roy et al., (1993) were used to calculate full carbonate chemistry from the measured DIC and TA data. First, the authors state that these constants are better at low temperatures. Please insert citation for this statement. Further the Roy et al constants are only defined for salinities down to 5. Section 3.3.1 states that even lower salinities were encountered. Please assess uncertainty associated with this.

line 14, only the assumed error in TA is stated. Please include error also in DIC and how this was determined.

page 1108

line 16, please replace "Concentrations of..." with "The"; pCO₂ is not expressed in terms of concentration.

page 1112

line 13, the use of a plus sign in the parenthesis here (DIC+TA) is unfortunate, as it

can be confused with the sum, if I do not misunderstand, please replace with "and". If I have misunderstood, please rephrase paragraph.

page 1114 line 36, a "high" is missing after "relative".

page 1118

line 27, onwards. I do not think that a transition from net autotrophy to heterotrophy is a valid alternative explanation, since, as evaluated from Fig 8, it is mostly TA that separates the high pCO₂ and low pCO₂ ponds from each other. If biology was the cause then I would expect that differences in DIC would be the cause, and not TA differences. Hence, alkalinity generation from sea-ice appears most viable.

page 1120

Please provide the details of how the air-sea flux calculations are carried out. In particular, provide details on how the transfer velocity is determined, and if the equation used (e.g. Wanninkhof '92) is applicable for the conditions in the melt ponds, which have no fetch and not white capping - which is important for increasing k values over the open ocean.

Further, I question the realism of the estimate as the total carbon content of the melt ponds is likely 1-2 orders of magnitude less than the inferred efflux of 22-216 Tg/yr as the following calculation shows:

Assuming MIZ of 12 million km², and melt ponds covering 50 % of this area. And if we assume an average melt pond depth of 0.5 m, and a DIC concentration of 56.4 umol/kg (from page 1114, line 4), then:

Melt pond surface area: 6 e12 m²

Melt pond volume = 6 e12 m²*0.5m = 3 e12 m³

Melt pond volume * Carbon concentration = 3 e12 m³ * 56.4 e-6 mol/1e -3 m³ = 1.9 e11 mol DIC, which corresponds to 2.3 e12 gC, or ~2.3 Tg

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Hence, the melt ponds doesn't contain enough carbon to support the efflux that the authors present, the paper should be moderated regarding this issue.

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Comments on Figures

Figure 3c, the number "36" appears in the panel, please remove.

Fig 5, mention in caption that similar maps for alkalinity are shown in Fig 6.

Fig 6, annotate also interface melt waters.

Fig 8, the difference between circle and square data points is not explained in caption.

Fig 6 vs. Fig 8, it appears as if some data in Fig 8 are not present in Fig 6, please check your plots, or make distinction in data selection clear.

Fig 10, the first sentence of the caption is not correct.

Fig 11, the pCO₂ scale bar is reversed, please change

References not cited in the text

Jeansson, E., A. Olsen, T. Eldevik, I. Skjelvan, A. M. Omar, S. Lauvset, J. E. Ø. Nilsen, R. G. J. Bellerby, T. Johannessen and E. Falck, The Nordic Seas carbon budget: Sources, sinks and uncertainties, *Global Biogeochemical Cycles* 25, GB4010, 2011.

Omar, A. M., T. Johannessen, R. Bellerby, A. Olsen, S. Kaltin, C. Kivimae, and L. G. Anderson. Sea ice and brine formation in Storfjorden: implications for the Arctic wintertime air-sea CO₂ flux., in *The Nordic Seas: an integrated perspective*, AGU Geophysical Monograph 158 (eds. H. Drange, T. Dokken, T. Furevik, R. Gerdes, and W. Berger), 177-188, 2005.

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