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Author Response: We sincerely thank Are for his 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

Referee comments:
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:
page 1099
line 13, insert "the" in front of "observed"
This is corrected in revised text
line 19, replace "$CaCO_3$" with "carbonate".
This is corrected in revised text

page 1100
line 16-24, this paragraph is yet another example of how the concepts of anthropogenic CO_2 and CO_2 are jumbled together. So far we have no proof that the Arctic has a large air-to-sea flux of anthropogenic CO_2 - 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 CO_2 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 CO_2 from the atmosphere. In fact, warmer waters have the higher buffering capacity and hence are more receptive for anthropogenic CO_2 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.
The first couple of sentences in the paragraph are restated to clarify about anthropogenic CO_2 and buffering capacity. Although not discussed in the paper, the Tanhua et al., 2009 paper estimates fairly high water column inventories of anthropogenic CO_2. We wholeheartedly agree that most of this is laterally transported (as Jeansson et al., 2011 have shown), but likely contributed to from air-sea exchange more recently due to sea-ice loss in the Arctic. Low buffering capacity, β, should have read “high” but again, there seems to be a wide range of β values across the Arctic which points to a complicated story (not addressed here though).

page 1101
line 15, replace "a" with "are".
This is corrected in revised text
line 23, Omar et al. (2005) is also an appropriate citation here.

We have added the Omar et al 2005 paper to the text and references

page 1103
Equation (2), please state that this is a simplified definition, and cite full definition, of Dickson et al., (2007) for instance.
We have stated this is the simplified equation in the revised text

line 15, insert "[B(OH)-]" between "where" and "is".
This is corrected in revised text

page 1105-1106.
I am somewhat concerned about the accuracy and precision of alkalinity data. It stated 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, pCO_2 and Omega andCO_2 flux values presented later in the paper.
We used the partially open VINDTA cells for the alkalinity determinations. We also agree that there are analytical uncertainties with regard to ionic strength, and CRM usage with a certified value for open ocean water. In the revised paper, we clarify this to state the anticipated precision for the mixed-later Arctic samples and slightly higher imprecision/inaccuracy for the lower salinity melt waters (similar to TA determination in freshwater/estuarine samples). Given the lack of CRMs for TA values below ~2000 µmol/kg, we had to assume linear fits in calibration in addition to dilution with Q water of CRM's.

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.
We re-checked the computations and Mehrbach et al., 1973 as refit by Dickson and Millero 1987 was used for dissociation constants. There is a convergence for calculated values of pCO_2, pH, etc at lower temperatures using the different dissociation constant, and we have to extrapolate the dissociations constants below temperatures of 1°C. This error is included in our estimates of the errors in computing pCO_2, pH, and Ω.

line 14, only the assumed error in TA is stated. Please include error also in DIC and how this was determined.
In the revised paper, we have recalculated the errors for the mixed-layer and melt

waters, propagating DIC and TA analytical error.

page 1108
line 16, please replace "Concentrations of..." with "The"; pCO_2 is not expressed in terms of concentration.
This is corrected in revised text, and also on page 1101, line 28; page 1108, line 8; page 1124, line 25;

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.
This is rephrased in the revised text.

page 1114 line 36, a "high" is missing after "relative".
This is corrected in revised text.

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 pCO_2 and low pCO_2 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.
We have rephrased some of the sentence in this discussion. In addition to the net ecosystem metabolism (NEM) at play in sea-ice, net ecosystem calcification plays an addition role (i.e., both calcification and dissolution; Ikaite formation and dissolution presumably)

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, 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 km^2, 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 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.
We have rephrased this part of the text. Originally, we were discussing "what ifs" and first estimated an efflux if "all" melt ponds had high pCO_2. But in the following

paragraph, we stated that a proportion of melt ponds had very low pCO_2 values (hence a potential to take up CO_2). Thus the net balance of the impact of melt ponds is highly uncertain as 19 ice stations are clearly not representative of conditions across the Arctic. It is thus remains an open question as to what the *net* melt pond air-sea exchange is, and we have made this clearer in the revised text.
We did use Wanninkhof 1992 for short term flux, and have explained this more in the text. We clarify that caveats associated with such choices, and that the actual flux is likely lower.

Comments on Figures
We have increased label font size and descriptors within the figures to make the figures easier to read given the final paper print size.
Figure 3c, the number "36" appears in the panel, please remove.
This is corrected in revised figure.
Fig 5, mention in caption that similar maps for alkalinity are shown in Fig 6.
This is corrected in revised caption.
Fig 6, annotate also interface melt waters.
This is corrected in revised figure.
Fig 8, the difference between circle and square data points is not explained in caption.
This is corrected in revised 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.
This is corrected in revised figure.
Fig 10, the first sentence of the caption is not correct.
This is corrected in revised caption.
Fig 11, the pCO_2 scale bar is reversed, please change
This is corrected in revised figure.

References not cited in the text
These are included in the revised list of references
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