Interactive comment on "The recent state and variability of the carbonate system of the Canadian Arctic in the context of ocean acidification" *by* Alexis Beaupré-Laperrière et al.

Leif Anderson (Referee)

leif.anderson@marine.gu.se

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This manuscript includes a very thorough data assessment of the carbon system parameters in Canadian Arctic waters and clearly shows the large variability both in time and space. It confirmed already published conclusions that much of Arctic shallow waters are undersaturated with respect to calcium carbonate, especially aragonite. The basis for this fact is both lowering of salinity by river runoff and sea ice melt in the summer, as well as degradation of organic matter, both in the runoff and in the shelf seas. A valuable contribution is evaluation of "time of emergence" that shows the need for several decades of data in order to separate variability from trends.

Another novel approach is to compute the biological contribution to DIC time variations in the top 25 m, i.e. how much of the ocean acidification signal that is of biological activity. They find this to be about one quarter; a number that I argue has to be taken with great care. The reason is that the DIC-reference computation assumes a constant difference in air-sea pCO2 with time. They mention this but do not relate this assumption to the sea ice coverage before and at the time of measurement. In this region of very temporal variation in sea ice coverage this could have a substantial impact. In the manuscript the decrease in sea ice coverage in the Canadian Basin is mentioned as a cause of elevated uptake of atmospheric CO2, so the authors are aware of this aspect. One could use the satellite images to assess potential changes in the sea ice cover of the region at the times of interest, if these data have enough special and temporal coverage. Considering the fact of changing sea ice coverage I am not sure that the statement on line 504 that the calculated delta DIC-bio is beyond the uncertainty. Further, the computation of delta DIC-bio, as described from line 480, is described in a complicated way. I had to read this section several times before understanding what had been done. My understanding is that DIC-ref is computed from TA and pH-t at the starting date, and then corrected for the increase in DIC by the increasing atmospheric pCO2, assuming a constant air-sea delta value. This could be described in a strait forward way.

Generally this manuscript is well written, but there are several more elements that need more care.

In line 48 it reads "of these phenomena". What do these refer to?

Line 109 reads "and expanding undersaturation". Of what? I assume omega, but as CO2 is also discussed in this section one has to be explicit.

Line 165. m-Cresol purple was used for pH determination. Specify if it was purified version or not as this have significant impact on the accuracy, even when calibration against TRIS buffer. Information on the magnitude of correction of the measured value

after calibration should be added.

In line 228 DIC-bio is mentioned before it has been defined. Hence move it to the section where it is defined.

Sentence starting on line 242 does not specify depth range. I assume it refers to the surface water, and if so it should be specified.

Line 258. I don't see the meaning in giving mean values of here as the range is so large and the mean is very much depending on the number of samples included in the different sub-regions. This also refers to the data on line 295.

Sentence starting on line 282 is confusing. What do you rule out? That air-sea exchange has not increased the omega value, or that primary production has not? Specify.

When it refers to Fig. 7 in the text it would be useful to also give the letters of the individual sub-figures.

Line 311. I assume that the pCO2 and pH-t values are at in situ temperature, but it would be nice to have this specified.

The statements starting with the sentence on line 355 needs some more explanations. As salinity is not presented (more than as averages in the table) the reader cannot judge the statement of weaker stratification. So how does this weakening agrees with the statement on line 359 of low salinities from freshwater accumulation followed by efficient mixing? I would assume that addition of freshwater to the surface would strengthen stratification and thus hampering mixing. Please clarify.

Line 362. It states that the entire water column is undersaturated, but in the figure not all are. Same goes for line 549 in conclusions. Change.

Sentence starting on line 439. Such conditions would gain on being compared with the dominating atmospheric pressure fields at the time (and before) the observations.

Suggestion ending the paragraph on line 449. I would challenge this explanation as the 2004 pCO2 data seems quite close to atmospheric levels, while the 2008 data are substantially below. Also the data at 140 m (?) show a difference between the years that is similar. The two options to get undersaturated CO2 is cooling or primary production. How do you get this signature down to 300-400 m depths? I can only see that there must be two different water masses present (if the data are correct that I do not doubt).

Line 464. Make 2 and A subscript in pCO2 and omega A

Line 608. The statement that the interannual variability should be relatively small at these depths is not necessarily true. In this dynamic region the contribution of different water masses might vary significantly also at these depths.

Finally I was surprised that there is no reference to the below article that is highly relevant to this contribution. Azetsu-Scott, K., A. Clarke, K. Falkner, J. Hamilton, E.P. Jones, C. Lee, B. Petrie, S. Prinsenberg, M. Starr and P. Yeats, 2010. Calcium carbonate saturation states in the waters of the Canadian Arctic Archipelago and the Labrador Sea. Journal of Geophysical Research, 115:C11021, doi:10.1029/2009JC005917.