Interactive comment on "Inorganic Carbon Cycling and Biogeochemical Processes in an Arctic Inland Sea (Hudson Bay)" *by* William J. Burt et al.

Anonymous Referee #1

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This work presents a nice data set from a region where few data has been collected, especially when focusing on the inorganic carbon cycle and how this is impacted by input from land. It is largely well written and the figures illustrate the results. However there are some minor issues that the authors should consider before publication.

One is an aspect that often is sees in the literature. The sentence that start in line 25 of page 1 reads that the Arctic Ocean is particularly vulnerable to OA because of ... low buffer capacity in cold waters... The low buffer capacity means that DIC is not changing much for a given change in pCO2, which also means that the concentration of CO32- does not change as much as it will in warmer water of less equilibrium DIC concentration. Thus the saturation state of calcium carbonate changes less in cold water compared to warm water for a given increase in pCO2. So even if the saturation state is lower to begin with does this mean that these waters are more vulnerable? It

is best to be more specific when making this kind of statement.

A principle issue is that one analyse seawater but determine (or measure) TA, DIC or any other constituent. When analysing TA it means that one determines what TA consists of. One can claim that this is minor thing and that the word "analyse" often is used in this way, but that is not an excuse for not being correct.

Page 6, line 22. It is suggested that excess TA and salinity is added by brine, and on line 36 that sea ice melt result in a negative deviation of TA. This is highly likely considering the data and other observations from the Fox basin, but can one thus conclude that this result show that no ikaite (calcium carbonate) is formed and trapped in the sea ice. I would urge the authors to expand on this topic.

On page 7 the term "benthic carbonate input" is used. I assume that what is meant that metal carbonate dissolves at the sediment surface and that the carbonate ion is added to the bottom water. If so add "ion" after carbonate.

Next to last sentence of the first paragraph on page 8. It is extremely important to take samples for salinity determination from the same flask as where the chemical samples are collected in stratified waters. This cannot be stressed enough and this contribution again well illustrates this.

The section starting on line 12 at page 9. It would be interesting if the authors discuss how the observed C-13 signal is impacted by either decay of marine produced organic matter or terrestrial produced organic matter. Can the data be used to distinguish these sources?

Next paragraph. Here I have difficulties to understand how bottom water with high TA and DIC can be mixed up into the surface to give the high omega values without also mixing up salinity. Is it not more likely that the dissolution of metal carbonates occurs in the river drainage basin and is transported out to the estuary? Is there information on the mineral compositions of the different drainage basins that can add

to the observations?

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