

Review of Evans et al. "Calcium carbonate corrosivity in an Alaskan inland sea"  
submitted to Biogeosciences Discussions.

## 1. General comments

This study reports on calcium carbonate ( $\text{CaCO}_3$ ) corrosivity in Alaskan coastal waters and link this to tidewater glacier melt. According to the authors, formation of water with near corrosive  $\Omega$  levels (i.e,  $\Omega \leq 1$ ) below the mixed layer are observed during spring, and later in autumn glacier melt result in surface plumes of water with corrosive  $\Omega$  levels ( $\Omega$  for aragonite and calcite down to 0.6 and 1.2, respectively) and carbon dioxide partial pressure ( $p\text{CO}_2$ ) well below atmospheric levels. The authors suggest that the cumulative effects of glacial melt and air-sea  $\text{CO}_2$  exchange are likely responsible for the seasonal widespread reduction of  $\Omega$  in the Prince William Sound. I find the study interesting, but miss data on biological parameters (e.g. primary production, chlorophyll) to be able to judge the importance of the glacial impact on  $\text{CO}_2$  conditions in the fjord. I find the manuscript suitable for publication after moderate modifications. Detailed comments are provided below.

## 2. Specific comments

Page 14891, line 1-5. In addition to the study of Sejr et al. (2011), there exist another study where the carbonate system has been investigated along a gradient from a tidewater glacier to off shore regions (Rysgaard et al. 2012, Marine Chemistry 128-129, 26-33. Take a look at their Fig 4 and 5. This may be helpful in your discussion.

Page 14894 Result section. In general I find too much discussion and references in this section. I suggest skipping all discussion of data here and move it to the discussion section. This will shorten the section and making this section appear clearer.

Page 14895, line 1-5. I find this figure difficult to understand. It would be helpful if you split up the figure in two, one containing data close to the glacier and the other containing the data for the rest of your stations. Also it would be easier to follow later discussions if you highlight or separate surface data from deeper ones.

Page 14895, line 15-20. Do you have any chlorophyll or florescence data to support that the high TA/DIC was caused by high rates of primary production?

Page 14896, line 6-15. Could the difference between May and September values of  $\Omega_{\text{arag}}$  and  $p\text{CO}_2$  be caused by difference in temperature?

Page 14896, line 21. Think the  $\Omega_{\text{arag}}$  of 0.06 should be 0.6.

Page 14896, line 24-29. You should show salinity data instead of density. Also pH is not shown in your Figure 4 – Check figure legend for Fig 4. You have  $p\text{CO}_2$  conditions as Fig 4c.

Page 14898, line 5-10. Show salinity instead of density in Fig 6.

Page 14898, line 20. Show salinity instead of density in Fig 7.

Page 14898, line 24. It is not clear what you mean with the “ mixed layer depth criterion of  $0.125 \text{ kg/m}^3$ ”. Please specify.

Page 14901, line 15-20. Sea ice melt is not oversaturated in  $p\text{CO}_2$ . See recent publications, Rysgaard et al., (2012) *The Cryosphere*, 6, 1-8. doi: 10.5194/tc-6-901-2012; Geilfus et al., *J. Geophys. Res.*, 117, C00G10, doi:10.1029/2011JC007118, 2012; Else et al., (2013) *Geophys. Res. Lett.*, Vol 40, 1132-1137, doi:10.1002/grl.50268.

Page 14902, line 5-10. Very difficult to follow how your  $10 \text{ mmol CO}_2/\text{m}^2/\text{d}$  is calculated. Provide more details.

Page 14902, line 25. I guess surface melt plumes from glaciers (upwelling) will also provide nutrients to the surface photic zone thereby stimulating primary production and  $\text{CO}_2$  uptake. It could very well be that these upwelling events will result in a net higher  $\text{CO}_2$  uptake and saturation index of  $\Omega$  than the process reported in your study. If you do not have the data, provide some speculations in the discussions on this matter.