Author Comment to reviewer 2:

1. General comments

This study reports on calcium carbonate ($CaCO_3$) corrosivity in Alaskan coastal waters and link this to tidewater glacier melt. According to the authors, formation of water with near corrosive Ω 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 Ω levels (Ω for aragonite and calcite down to 0.6 and 1.2, respectively) and carbon dioxide partial pressure (**p**CO_2) well below atmospheric levels. The authors suggest that the cumulative effects of glacial melt and air-sea CO_2 exchange are likely responsible for the seasonal widespread reduction of Ω 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 CO_2 conditions in the fjord. I find the manuscript suitable for publication after moderate modifications. Detailed comments are provided below.

We very much appreciate the reviewer's comments, and note that there were not coincident primary productivity measurements with the data presented in our manuscript. However, we now show ΔO_2 (difference from saturation) values that were only previously discussed. We have added a new figure that links ΔO_2, TA/DIC ratios and NO_3 concentrations to show that when ΔO_2 is positive, NO_3 is drawdown and TA/DIC ratio is elevated. We also now present nitrate concentration profiles from Icy Bay to add to our discussion of year-to-year differences and to note that there was no upwelling signal apparent in the NO_3 data. We now also include MODIS chlorophyll monthly climatologies in order to highlight the minimal observed phytoplankton abundance (in terms of satellite chlorophyll) during the period of glacial discharge (summer). These pieces of evidence suggest that phytoplankton production does not play a major role in determining the saturation levels of $CaCO_3$ in PWS surface water at the time of our measurements, but the satellite data highlight that the period of our observations are perhaps out of phase with times when phytoplankton production may be more of an important driver for offsetting $CaCO_3$ corrosivity (i.e. April spring bloom).

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.

Thank you very much for this suggestion, we were unaware of this recent paper and found it helpful in our introduction and 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.

We have restructured the manuscript and combined the Results and Discussions sections. We then divided the Results and Discussion into 6 sub-sections. The new structure helps make our manuscript much 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.

Surface data closest to the glaciers were the measurements with the lowest salinities, and we have made this clearer in the text. Following comments from the 2 reviewers, we split this figure up such that the TA versus salinity panel is its own figure and now includes DIC versus salinity. A second panel was added to this figure to show TA/DIC ratio versus salinity. Surface data (measurements collects shallower than 9 m) are now highlighted as bold circles in all plots. pH, Omega and pCO2 are now on a separate figure, also with surface data highlighted.

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?

We lack primary productivity measurements but now include the oxygen data that was previously only discussed. We have added a figure to show the link between TA/DIC ratio and $\Delta O2$ values.

Page 14896, line 6-15. Could the difference between May and September values of **Ω**arag and **p**CO2 be caused by difference in temperature?

We noted in the text that higher pCO2 values for a given Omega (i.e. data following above the May fit) were likely the result of temperature because the observed 5 degree temperature increase would drive a 60 uatm increase in pCO2 with only a 0.05 unit increase in Omega. The largest variability in Omega for a given pCO2 occurred at low pCO2 values and was due to instances of depleted carbonate ion, and these measurements were made adjacent to tidewater glaciers. Warming would have the opposite effect on carbonate ion, causing a concentration increase.

Page 14896, line 21. Think the **Ω**arag of 0.06 should be 0.6.

Corrected, thank you.

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**CO2 conditions as Fig 4c.

We have changed figures to show salinity instead of density, and have made the correction to the figure legend.

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

We have changed figures to show salinity instead of density.

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

We have chosen to leave density anomaly in Fig. 7 (now Figure 10). This is because we adjusted our discussion of this figure to express that the mode water had the same T and S, and therefore density anomaly, characteristics, and that it is associated with cold layers. Highlighting the region where temperature and density anomaly are the same implies salinity is also the same.

Page 14898, line 24. It is not clear what you mean with the " mixed layer depth criterion of 0.125 kg/m3". Please specify.

That is the mixed layer depth criterion based on the density change from the surface value used by WOCE, and we use it here to define the mixed layer depth in PWS. We have clarified in the text that it is the change from the surface density anomaly value.

Page 14901, line 15-20. Sea ice melt is not oversaturated in **p**CO2. 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.

Thank you for these references, and greatly helped with our discussion. We have corrected our previous statement about sea ice.

Page 14902, line 5-10. Very difficult to follow how your 10 mmol CO2/m2/d is calculated. Provide more details.

That is the coastal ocean sea-air CO2 flux for the Gulf of Alaska reported in Evans and Mathis (2013). We have clarified in the text that this number comes from that manuscript.

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 CO2 uptake. It could very well be that these upwelling events will result in a net higher CO2 uptake and saturation index of **Ω** than the process reported in your study. If you do not have the data, provide some speculations in the discussions on this matter.

The nitrate profiles adjacent to the glacier in Icy Bay do not show high values at the surface in the low salinity melt plumes. The effect of turbulent mixing on surface nitrate levels may be diluted by the addition of glacial melt. Satellite chlorophyll data appear lower over the entire portion of PWS that contain tidewater glaciers relative to the portion that does not, suggesting to us, at least from this coarse surface perspective,

that primary productivity isn't playing a large role in setting omega levels. We have added these points to our discussion.