

Interactive comment on “Calcium carbonate corrosivity in an Alaskan inland sea” by W. Evans et al.

Anonymous Referee #1

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I. General comments

In this manuscript, the authors report a data set of carbonate system measurements in an Alaskan Inland Sea in spring and autumn from 2009 to 2012, with special attention to the impact of tidewater glacial melt on the marine carbonate system and CaCO_3 corrosivity (i.e. saturation state Ω). They observed the seasonal changes in Ω in the upper layer ocean, e.g. in the surface plumes of glacial melt and in the mode water formed in proglacial fjords. For the waters affected by glacial melt, decoupling of pCO_2 , pH and Ω is discussed together with the positive feedback of the atmospheric CO_2 uptake which may further decrease Ω . The authors also discuss the potential farther-reaching impacts of these processes due to physical transportation. Overall, the subject of this manuscript meets the general interest of Biogeosciences. The data are

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of good quality and the discussions are well organized. I thus support the publication of this work after a moderate modification. Please see below for my detailed comments.

II. Specific comments

1. The authors suggest that the addition of glacial melt would exacerbate CaCO_3 corrosivity, but the cause of this decrease in Ω seems not been discussed. The authors are encouraged to discuss the underlying controlling mechanisms of this phenomenon (what aspect of Ω is affected? $[\text{Ca}^{2+}]$, $[\text{CO}_3^-]$ or $K'sp$?).

2. The use of “glacial melt” is a bit confusing and the sources of the freshwater in the study area need to be described more clearly. In the introduction, the authors emphasize the “direct glacial discharge at ocean/glacier interfaces which is free from additional alteration that would occur in proglacial streams draining mountain-terminating glaciers”. This leaves the impression that only this kind of glacial melt will be discussed in this manuscript. However, in the ‘study area’ section, the authors state that “glacial runoff, either as a direct input to the sound or indirectly via proglacial streams, contributes an estimated 60% of the freshwater discharged to PWS”. And in Fig. 9, they discuss the mixing process involving the glacial melt from the nearby land-terminating Bench Glacier (is this a proglacial stream?). The authors should better describe the sources of the freshwater especially in the fjords where the most corrective surface waters were observed. Although share a common salinity ($S=0$), the chemical properties of the direct glacial discharge may be significantly different to those of the proglacial streams, so do their influences on the seawater carbonate system. If there exists different sources of freshwater, it seems to me that the fjord under study is analogous to an estuary system with multiple tributaries. It is important to distinguish the different impacts of various sources of freshwater. Current data set in this work may not be able to address this issue, but it is a problem needs to be studied in the future.

3. Salinity is the indicator of the surface plumes of glacial melt, and the salinity data should be presented in the corresponding figures. If the plumes is strictly limited to

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surface, the depth of the plume should be given.

4. Fig. 2 clearly shows the data points of the most corrective surface waters in the fjords which are characterized by low salinity, low TA, low pH, low Ω and low TA/DIC ratio. Fig. 2A suggests that these points at the low salinity end are well below the linear TA-salinity relationship that most surface waters obey. Could this be a TA removal (indicated by the low TA/DIC ratio) or another segment of the TA-salinity mixing line? The TA-salinity regression could be extrapolated to get an intercept at $S=0$, which gives an indicate of the end member property of the freshwater. In Icy Bay, the intercept can be compared to the reported TA in the glacial melt from the nearby land-terminating Bench Glacier.

5. As pCO₂ and pH are more sensitive to the temperature change than Ω , temperature might plays a role in the coupling/decoupling of pCO₂, pH and Ω . In Fig. 3, have the authors tried that if the use of temperature-normalized pCO₂ (4.23% per degree, Takahashi et al. 1993) and temperature-normalized Ω would improve the fitting?

6. p14901, lines 16-21: Please discuss briefly what causes the undersaturated pCO₂ in the glacial melt and why it differs from that of sea ice melt (oversaturated pCO₂)?

III. Technical corrections

p14888, lines 15 “across the sound” : is it only in the western sound (as shown in Fig. 7)?

p14891, lines 9-15: Just for your reference, another possible reason for the mismatch between the direct observed pCO₂ and calculated pCO₂ could be the existent of organic alkalinity (Cai et al. 1998; Hernandez-Ayon et al. 2007; Muller and Bleie 2008; Kim and Lee 2009).

p14891, lines 15-18 “It follows . . .”: Please rewrite this sentence. According to the authors’ review on the work of Sejr et al., [2011], the “direct glacial discharge less buffered by high concentrations of reactive particles derived from river sediments” would result in lower TA and higher pCO₂ in the glacial melt when compared to those in the

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proglacial rivers, but not necessary “severe CaCO₃ corrosivity in addition to undersaturated pCO₂”.

p14896, lines 21: 0.06, should this be 0.60?

p14900, lines 9-11 “The cumulative pulse . . . the wholesale reduction in CaCO₃ saturations in PWS”: do you mean in the surface layer in PWS? The seasonal changes in surface and sub-surface are different according to the discussion below.

p14900, lines 19-21: “the lesser increase in the dynamic range of DIC relative to TA” can result from either “a process is adding DIC with little effect on TA” or additional TA removal with little effect on DIC (also see the comment 4).

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