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Interactive comment on "Seasonal Carbonate Chemistry Variability in Marine Surface Waters of the Pacific Northwest" *by* Andrea J. Fassbender et al.

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Review of: Seasonal Carbonate Chemist ry Variability in Marine Surface Waters of the Pacific Northwest Fassbender et al.

General comments:

On the whole Fassbender et al. have compiled a prodigious amount of inorganic carbon data collected by different platforms in a dynamic under-sampled coastal zone. They have also made the effort to make each data-set predict the full carbonate system (using an approximation based on Salinity - to empirically determine total alkalnity.

Overall I think that this manuscript and dataset should be published and laud the authors on their hard work putting these varied data together. Where the effort could be improved I give some suggestions below.

Main points:

(1) Although this journal's aims are to produce data sets, I think that the paper would benefit from a stronger focus on the story that the data tell and mechanisms. Mechanisms are especially important to understand for readers who don't have an oceanographic background and OA is garnering concern from many interdisciplinary and political camps. The region is highly complex so responding to this comment is a tall order. (The specific comments below contain some suggestions in the region that this reviewer is familiar with, but the authors should not limit themselves to these comments.)

(2) Since the focus on combining data into one high quality data set is so important I suggest that they include data that were collected by Canadian researchers in their study region (in addition to US collection in Canadian waters), even if these data are not as readily available, something this reviewer feels badly about. (more below)

(3) Perhaps the most important general comment concerns uncertainty. The authors have considered uncertainty, but not as fully as they could. For e.g. uncertainty in discrete samples may be (is likely) underestimated, especially nearer shore - in highly stratified waters where there may be terrigineous inputs of organic matter that are assumed neglible in the open ocean assumptions. (see specific comments below) There is also considerable uncertainty in the determination of the CO2 parameters depending on which of the carbonate pairs are used,and in particular in the reaction constants. James Orr et al. have done some comprehensive work on this problem recently.

Specific comments:

This set of suggestions is up to the authors - how they handle them should not preculde publication: As much of 20In Canada this region is not termed the 'Pacific Northwest'. Suggest: –"Pacific Northwest" —> "U.S. Pacific Northwest" – "U.S. West Coast waters" —> "U. S. West Coast and adjacent waters" or "the northern region of the California Current System".

Abstract - Suggest add more information about the results and a more concise treatment of what was done to obtain them.

Data - The authors have compiled a truly impressive amount of data.

There are more discrete and underway data available in the region and those that I know about are at:

https://www.waterproperties.ca/data/

(I anticipate that at least some would already be posted on OCADS but perhaps not at the time that the paper was prepared) This site (hosted by Fisheries and Oceans Canada) is less useable than OCADS and would contribute relatively few data relative to those contained in the full study, but still worthy of inclusion especially because they give context in the northern area of the study zone shown in Figure 1 of this submission and some of these data go back to 1973 (underway surface) and some (discrete) to 1998.

Some publications that I am aware of that contain these data include:

(a) Ianson et al. 2003 The inorganic carbon system in the coastal upwelling region west of Vancouver Island, Canada DSRI (discrete DIC and TA from the summer of 1998 (Enso!))

(b) Wong et al. 2010 Carbon dioxide in surface seawater of the eastern North Pacific Ocean (Line P), 1973–2005 DSRI (covers region in northernmost transect and Juan de Fuca Strait in this study - surface only - like SOCCATS only S and pCO2 so would require use of TA-S relationship)

(c) Ianson et al. 2016 Vulnerability of a semienclosed estuarine sea to ocean acidification in contrast with hypoxia GRL (covers the Juan de Fuca strait - discrete DIC, TA (etc) data from 2003-2012)

Lines 160-170 - Its unlikely that the true uncertainty is ts low as +/-2 umol/kg for either DIC or TA but esp. for TA and especially as one gets closer to the coast. I list one example, but it is certainly not the only one and probably not the best. E.g. Ianson et al. 2016 took >12% replicates and compared. These data show two populations in their sample area (which overlaps with this study) and they calculate an uncertainty based on replication in the field. See supp https://agupubs.onlinelibrary.wiley.com/action/downloadSupplement?doi=10.1002%2F2016(The authors don't likely have access to replication necessary, but I think that thier text should reflect the larger potential uncertainty in these open ocean methods (see above). Useful ref: Hunt et al. 2011; Contribution of non-carbonate anions to total alkalinity and overestimation of pCO_2 in New England and New Brunswick rivers

Section 4.1

This reader suggests that the best reference describing the circulation in the JdF is Thomson et al. 2007 JGR Estuarine versus transient flow regimes in Juan de Fuca Strait. (although certainly not nec. to support their simple mixing statement)

Perhaps more important to carbon dynamics in the tidal mixing zones (Juan de Fuca, Haro, Admiralty etc.) is the fact that at least within the former of these two Straits nutrients are never limiting to phytoplankton growth - see:
Mackas, D.L., Harrison, P.J., 1997. Nitrogenous nutrient sources and sinks in the Juan de Fuca Strait/Strait of Georgia/Puget Sound estuarine system: assessing the potential for eutrophication. Estuarine, Coastal and Shelf Science. 44, 1–21. doi:10.1006/ecss.1996.0110
and also (when its available),
Krogh et al. (2018) Risks of hypoxia and acidification in the high energy coastal envi-

ronment near Victoria, Canada's untreated municipal sewage outfalls (accepted May, 2018- Marine Pollution Bulletin)

In addition, as a function of the circulation and this tidal mixing - the seasonal cycle of surface pH is opposite in Juan de Fuca and Haro Strait when compared with the Strait of Georgia (highest pH in winter in JdF due to outer coast downwelling/upwelling circulation on the outer coast. The author's data may not show this feature but data of Ianson et al. 2016 do.

The Columbia and Fraser River appear to have different chemistry. As a result of this difference, and their unique circulation/stratification the respective impact of these rivers is radically different. The authors miss this aspect in their text. For river chemistry see https://doi.org/10.5194/bg-2017-349 - one figure has both rivers in DIC-TA space. For Fraser river physical control of biology and as a result surface chemistry see
(a) https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015JC011118
(b) https://www.sciencedirect.com/science/article/pii/S027277140900002X?via

lines 425-427 - The Juan de Fuca (or Tully) eddy yields enhanced upwelling by bathymetry - a complexity that may be worth mentioning in terms of mechanisms that provide more consistent upwelling and upwelling that raises deeper isopycnals to the surface. some possible refs:
Freeland and Denman 1982: A topographically controlled upwelling center off southern Vancouver Island. JMR
Waterhouse et al. 2009 Upwelling flow dynamics in long canyons at low Rossby number. JGR
Hickey and Banas 2008: Why is the northern end of the California current system so productive? Oceanography.

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