Review

Chastain, S. G., Kohfeld, K. E., Pellatt, M. G., Olid, C., and Gailis, M.: Quantification of Blue Carbon in Salt Marshes of the Pacific Coast of Canada, Biogeosciences Discuss. [preprint], https://doi.org/10.5194/bg-2021-157, in review, 2021.

This comprehensive study fills a major gap in our knowledge of tidal marsh accretion and blue carbon as there is a lack of data on tidal marshes of the Northern NE Pacific coast. A major contribution is not only the geographic aspect, but also observations of C accumulation rates under regressive sea levels and the evaluation of low versus high marsh. The thorough, detailed explanation of all calculations makes the methodology clear and *most* of the results (see comments on compaction) justifiable. The approach to comparing 30-yr C stocks is novel and perhaps should be adapted as a standard for future studies of blue carbon stocks where dating models are available.

This work on the British Columbia coast could even further advance blue carbon science by providing details on the geomorphic context of each marsh. There is nascent research showing that the C stock of marshes is related to their geomorphic context (see van Ardenne, Jolicouer, Bérubé, Burdick, Chmura. 2018. The Importance of Geomorphic Context for Estimating the Carbon Stock of Salt Marshes. *Geoderma* 330:264-275). It would be useful to know if it plays a role in these British Columbia marshes, e.g., behind spits, on lagoons, fluvial marshes (as per Kelley JT, Gehrels WR, Belknap, DF, 1995. Late Holocene relative sea—level rise and the geological development of Tidal Marshes at Wells, Maine, U.S.A. *J. Coast. Res*. 11, 136–153.) or at least be available for future meta-analyses.

On line 359 – Authors state that C stocks per ha are less than 1/3 that of global estimates, undoubtedly due to the shallow marsh deposits that are less than the 50 cm depth used by Chmura et al. (2003). The estimate of Chmura et al. (2003) also utilized a formula published by Craft et al. (Craft CB, Seneca ED, Broome SW. 1991. Loss on ignition and Kjeldahl digestion for estimating organic carbon and total nitrogen in estuarine marsh soils: Calibration with dry combustion. *Estuaries* 14:175– 179.) to convert LOI to %OC and the authors used their own conversion, which results in lower values than what would be produced using Craft's. Would the stock still be <1/3 if authors had used the conversion of Craft et al? It would not be a terribly difficult exercise and would help to stimulate a re-evaluation of global carbon stocks.

The comparison of C accumulation rates in tidal marshes of Canada's Pacific coast to that of boreal forests is interesting and one cannot argue with the point that the considerably greater area of boreal forest makes them (presently) a greater C sink, despite the slow rates of C storage in the latter ecosystem. However, authors should recognize that with climate change the increased prevalence of forest fires would result in episodic losses of the carbon. If fire frequencies are too high, then there may not be time for succession to proceed to the needle leaf forest, shifting the landscape to a semi-permanent deciduous forest, with reduced carbon storage potential (see Melvin et al. 2015 *Ecosystems* 18:1472-1488). As sea level rise is not a threat to the Canadian Pacific salt marshes they are likely to continue to function as efficient C sinks despite global warming, and policy makers should be alerted to this fact.

Authors compare their results to averages reported in the review by Ouyang and Lee (2014). As this review has a number of errors with respect to double-counting records (e.g., averages of 3 sites were included as a 4th site) and attribution of geographic locations, its reports should be used with caution.

Some cores had high levels of **compaction**, due to use of percussion corers. (This type of coring should be the last choice when working in wetland soils as there are other devices that can be used which produce negligible or no compaction. For instance, authors do not mention trying a narrow diameter Dutch gouge corer, which often saves the day – or simply shoveling out a block and coring through the excavated material.) Although the compaction not a problem when calculating stocks to the base of the marsh deposit, it can affect bulk densities, thus carbon densities and the calculation of accumulation rates (one of the dated cores had 41% compaction). At line 200, the text states, "Here we estimated the accumulated C to the corrected (uncompacted) depth of 20 cm". Use of lead-210 inventories and 30 yr stocks help to address the complication of compaction, but authors should note how compaction was corrected for and how bulk densities were adjusted – this is very important and should be in the methodology. I assume that there was a threshold for compaction level beyond which cores were not used for calculation of bulk or carbon density and certainly lead-210.

Shouldn't the regression for the relationship of %LOI and %C be forced through zero? With a negative intercept a sample with no organic matter, thus 0% LOI would have a negative amount of carbon – an impossibility.

Clarification of and distinction amongst the terms "topsoil", "humus" and "peat" is needed. What is "topsoil" in a marsh? This term is not commonly used for wetland soils. The manuscript states see "Supplemental Information", but there is no explanation there. Also, the term "humus" is seldom used in wetland soils. Presumably it plant litter that is gradually broken down with depth? A bit of explanation would be helpful, even if just in a footnote to the Appendix table.

Line 518- Why would tidal amplitude be a driver of methane emissions? The paper cited on this line (Poffenbarger et al. 2011) reports that salinity, as a proxy for marine sulfates, is an important correlate.

The text and Figure B1 include "backshore" vegetation, a term not commonly seen in salt marsh ecology – it would be good to cite a paper that describes what this designates, beyond "less salt tolerant" vegetation. On Line 585 is the phrasing "freshwater-dominated backshore or salt-tolerant meadow" intended to indicate that these two are synonymous? I note that *Plantago maritima* is included in the "circle" of backshore vegetation, yet the text (line 114) includes it in high marsh. The distribution of *Plantago maritima* on the east and west Atlantic coasts does not suggest it has a low salt tolerance, so it might be advisable to adjust the bounds of the circle.

Technical Editing

Authors appropriately compare data to IPCC estimates. It would be preferable to cite the source chapter in the IPCC document: Kennedy HA, Alongi DM, Karim A, Chen G, Chmura GL, Crooks S, Kairo JG, Liao B, Lin G. 2013. Chapter 4 Coastal Wetlands In: *Supplement to the 2006 IPCC Guidelines on National Greenhouse Gas Inventories: Wetlands*.

Line 115 - Note that there has been a botanical revision of *Glaux maritima* to *Lysimachia maritima.*

Line 185 - Khrishnaswamy should be spelled Krishnaswamy

Line 356 – This statement could be more direct and not couched as "probably". If there is little difference in C density, then it is obvious that the shallower the soil/sediment/peat, the less carbon stock in that location.

Line 440 - Ryczik should be spelled Rybczyk.

Line 583 – why not replace “from close to” with “near”?