RESPONSE TO RC1 COMMENT ON: 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.

We thank Reviewer 1 for this helpful and detailed review of this manuscript. Below we outline our responses red text.

REVIEWER 1:
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.

This is an interesting suggestion by the Reviewer 1. A direct comparison with the geomorphic contexts in van Ardenne et al. 2018 is somewhat challenging. The terrain around our study area does not really lead to the formation of spits and lagoons. Many of our sites were enclosed bays but they were not really cut off by spits. All locations were somewhat close to fluvial sources of varying size. Thus, applying the exact categories of van Ardenne et al. (2018) could be somewhat contrived here.

We do note that very recent work by van Ardenne, Hughes, and Chmura (JGR-Biogeosciences, 2021) has examined this question – albeit in fresher salt marsh systems - on the central BC coast. They argue that relating carbon density and marsh depth to geomorphology is difficult on a geomorphologically dynamic coastline as is found in our study area. We suggest that this might be an interesting topic to revisit in future.

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

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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.

Thank you for this interesting suggestion as a possible cause for the differences between the global and Pacific coast C stock estimates. The two equations mentioned here for calculating %OC from LOI are as follows:

Craft et al. (1991) polynomial regression: %OC = 0.40 [LOI] + 0.0025 $[LOI]^2$
Chastain et al. (this study) linear regression: $\%OC = 0.44(\%LOI) - 1.80$

We examined the effect of using the Craft et al. (1991) regression by calculating the differences in %OC that would result from using Craft et al.'s equation (see Figure R1.1.). The calculated %OC values are fairly similar for low values of %LOI (<~30%), but the %OC values diverge for %LOI values above this point, with calculated differences in %OC exceeding 20% at %LOIs above 80%. However, we note that the interquartile (Q1:Q3) range of our %C values fall between 4.39 to 28.84%, suggesting that most of our samples have %C values of less than 30% where the equations produce similar results. This seems to imply that the differences may not be too large.

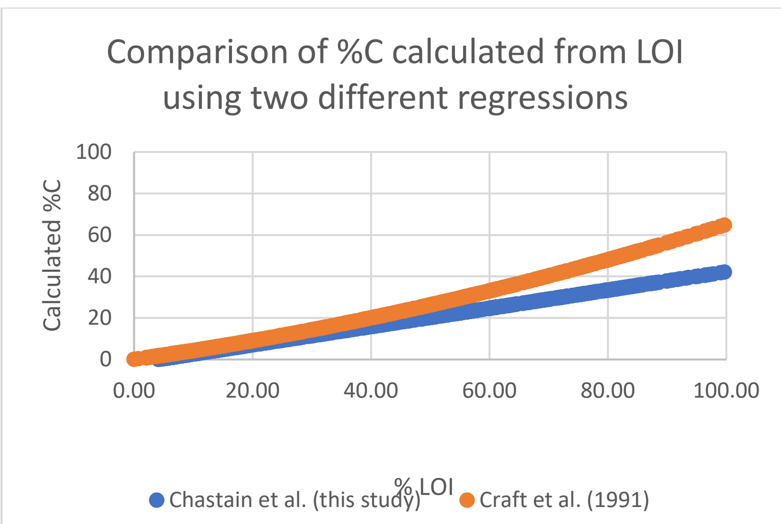

Figure R1.1. Comparison of %OC calculation using the empirical regression determined for our study area (blue) and the model of Craft et al. (1991) (orange).

However, to test the potential impact of the different equations, we conducted a quick comparison of C stocks (estimated to peat base) generated using the two different %C-LOI relationships, just for the 8 cores that were ^{210}Pb dated (see Figure R1.2). Using the Craft et al. (1991) regression inflates our C-stock values by about 30%, but this is not sufficiently large to account for the full difference between our C stocks and the global values in Chmura et al. (2003) (where global average estimate is 3 times greater than ours).

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We prefer to use our equation because it is site specific but plan to note the potential effect of these different equations in our text.

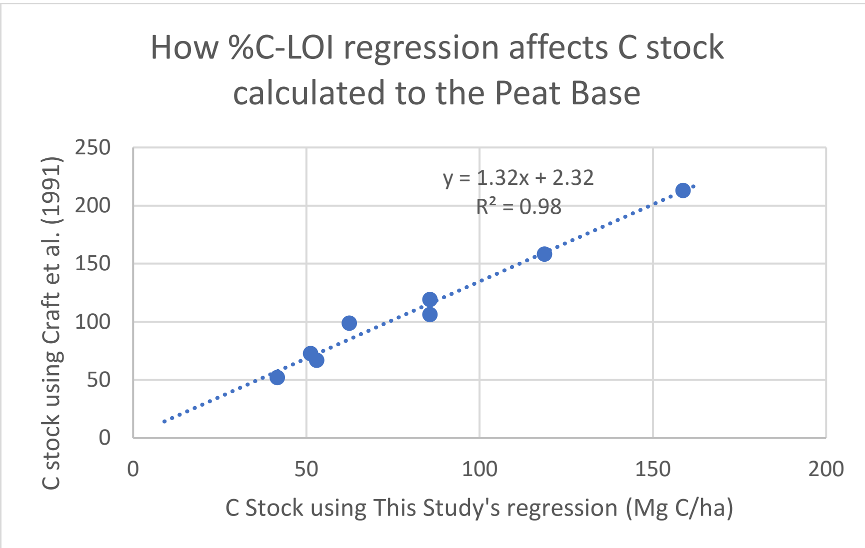

Figure R1.2. *Comparison of C stocks (Mg C/ha) estimated using the Craft et al. (1991) equation versus C stocks estimated using this study's empirical relationship for southern BC, for 8 cores from our study region. Comparison suggests that using the Craft et al. (1991) relationship would produce C stocks that are ~32% greater than our estimates. This difference, while substantial, would not account for the 3-fold difference between global C stocks and those found in Clayoquot Sound salt marshes.*

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.

We agree that these caveats should be added to the paper and plan to update this discussion based on more recent research on the impact of wildfires on the permanence of carbon storage. We thank the reviewer for the reference provided as a starting point. We will incorporate additional studies and information as needed (e.g. Kurz et al. 2013; Natural Resources Canada 2020, Zhao et al. 2021).

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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.

We agree that the Ouyang and Lee (2014) has incorporated some errors. Our comparison with the Ouyang and Lee (2014) paper was basically intended to point out the absence of dating for those records, which we discuss on lines 575-589. We recognize, however, that we first mention the Ouyang and Lee (2014) comparison very early in the paper, prior to our discussion of these complications (Line 63-79).

To address this point we intend to incorporate additional caveats about Ouyang and Lee (2014) paper (i.e., potential issues with double-counting in addition to the reliance on ^{137}Cs dates already mentioned), in the introduction when we first address the topic.

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.

We used the percussion corer as it was a closed chamber with internal PVC sleeves. This was chosen over a gouge corer because they are susceptible to disturbance and sediment mixing due to the nature of the open chamber of the corer. We did not use a Russian corer as compaction would be similar because the nature of the marsh sediments and we did not want to introduce increased contamination by the pivoting nature of the sampling chamber. Digging pits with a shovel was not an option as this study took place in a national park and biosphere reserve.

To clarify this point we will add a short explanation in the methodology to explain why we sampled with a percussion corer, and the potential uncertainties or errors associated with the corer.

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We will also review our description of the methodology to clarify when and where we corrected for compaction and how.

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.

Thank you for pointing this out. The relationship between %C and %LOI suggests that we measure zero %C in samples where LOI is not completely zero (below approximately 10% LOI). Although negative values of %C are obviously not possible, forcing the equation through zero would overestimate %C in these low LOI samples. Therefore, all calculations producing a negative value for %C were adjusted to zero %C. This occurred in 41 of 835 samples measured. Our methods have been clarified to reflect this change.

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.

We take this point and have changed the term "topsoil" (which was used to describe the fibrous organic material within and below the root zone) as "peat." We will also modify the term "humus" and add a more detailed explanation 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.

We appreciate this comment and we can replace the Poffenbarger et al. (2011) publication in this context, as there are several better citations that have measured changes in methane emissions associated with tidal activity and sea level rise (e.g. Abdul-Aziz et al. 2018; Emery et al. 2021; Huang et al. 2019; Huertas et al. 2019; Li et al. 2021; Wei et al. 2020.).

We will add 1-2 sentences to this discussion to reflect the results of these works and relate them to how the measurement of CH_4 emissions will be important for assessing the overall blue C potential of the Clayoquot Sound marshes studied here.

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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.

We have taken this term from the following resource: Green Shores | Resources https://stewardshipcentrebc.ca/green-shores-home/gs-resources/glossary/

Here “Backshore” is defined as “The upper zone of a beach (or land above the OHWM) beyond the reach of normal waves and tides, landward of the beach face. The backshore is subject to periodic flooding by storms and extreme tides, and is often the site of dunes and back-barrier wetlands”

We can change this term to “upland vegetation” if the reviewer is more comfortable with that.

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.

Point taken and we have adjusted 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*.

Will do.

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

Will change

Line 185 - Khrishnaswamy should be spelled Krishnaswamy

Will change

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.

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Will change

Line 440 - Ryczik should be spelled Rybczyk.

Will change

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

Will change

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