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.

General comments on review of revised manuscript:

Authors' responses do not seem to reflect an understanding of the reviewer comments regarding geomorphology, proper coring methods and acceptable compaction levels, statistical analyses and hydroperiods relative to tidal ranges (thus methane emissions). Unfortunately, the responses and text revisions are not appropriate and I cannot recommend accepting the manuscript in its present form. There are 5 major "revisions" or "responses" that are unacceptable. Below I repeat my original comment, the authors' response and my new comments on the authors' responses and text revisions, the latter identified by ALL CAPS.

1. Original Reviewer Comment

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.

A direct comparison with the geomorphic contexts in van Ardenne et al. 2018 is somewhat challenging because the terrain around our study area does not involve 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 recent work by van Ardenne et al. (2021) has examined this question – albeit in fresher 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.

We have added a comment about geomorphology here (ln 106-108):

"These sites are typical of salt marshes along Canada's Pacific coast because they include small, pocket marshes encompassing an enclosed, semi-circular area of coastline as well as larger, estuarine marshes. Unlike geomorphological settings in Atlantic Canada (e.g. van Ardenne et al. 2018), we do not see extensive formation of spits and lagoons; many of our sites were in enclosed bays but were not cut off by spits. All sites were somewhat close to fluvial sources of varying size. Surface water salinity in the surrounding waters ranged from 5.9 at KCS to 24 in Grice Bay, and 29 at Roberts Point six km south of CRF (Postlethwaite et al. 2018)."

<u>REVIEWER COMMENT ON AUTHOR STATEMENT AND REVISED TEXT</u>

NEW TEXT ON LINE 106-108 NEEDS CORRECTION. REVIEWER COMMENTS DID NOT REQUEST A DIRECT COMPARISON WITH VAN ARDENNE ET AL. BUT AS AN EXAMPLE OF HOW TO PUT THE BC SITE IN THE CONTEXT OF THE GEOMORPHOLOGICAL CLASSIFICATION OF KELLEY ET AL. THUS, THE COMMENTS

ABOUT SPITS, ETC. ARE INAPPROPRIATE AND SHOULD BE DELETED. PROPER GEOMORPHOLOGICAL TERMINOLOGY IS REQUIRED HERE, RATHER THAN TERMS NOW ADDED SUCH AS "SEMI-CIRCULAR AREA OF COASTLINE" AND "ESTUARINE MARSHES". I SUSPECT THAT THE SEMI-CIRCULAR AREAS OF COASTLINES ARE BAYS - WHICH FALL UNDER THE DEFINITION OF "ESTUARY".

2. Reviewer initial comment

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 have added an explanation in the Methods section to explain why we sampled with a percussion corer, in which we quantify the effects of compaction on our sediment cores. We also provide a brief explanation for how we have accounted for compaction (ln 157-168):

"*Use of the percussion corer resulted in sediment compaction during sample collection, which averaged about 20% across all cores (range 0-55%) (Table A1). Nevertheless, we opted to use a percussion corer instead of a gouge corer because the percussion corer had a closed chamber with internal PVC sleeves. Our experience with this sedimentary has demonstrated that a gouge corer would have been susceptible to disturbance and sediment mixing due to the nature of the open chamber of the corer. Because the nature of the marsh sediments, we also did not use a Russian corer because compaction would have been similar to what we experienced with the percussion corer, and we did not want to introduce increased contamination through the pivoting nature of the sampling chamber with the Russian corer. Digging pits with a shovel was not an option as this study took place in a national park and biosphere reserve. We note below that correction for compaction was not necessary for estimation of C stocks because the C stocks were estimated directly from sediment cores and not from the overall depth of marsh soils (thus all carbon in the peat layer, regardless of compression, is included in the calculation).*

<u>REVIEWER COMMENT ON AUTHOR STATEMENT AND REVISED TEXT</u>

THE NEXT TEXT ON LN 157-168 (WHICH DOES NOT SEEM TO BE RECOGNIZED AS AN ADDITION IN TRACK CHANGES) IS INAPPROPRIATE AND MUST BE DELETED AS IT WOULD BE EXTREMELY MISLEADING TO ANY READERS WITHOUT THEIR OWN CORING EXPERIENCE. FIRST, A RUSSIAN CORER DOES NOT COMPACT SEDIMENT AS IT CUTS IT FROM THE SIDE AND DOES NOT RESULT IN CONTAMINATION ACROSS DEPTHS! HOWEVER, IT IS IMPRACTICAL IN SOME WETLANDS THAT HAVE DENSE MINERAL SOIL. THOSE OF US WITH DECADES OF EXPERIENCE CORING A RANGE OF MARSH

SEDIMENT TYPES KNOW THAT ANY DISTURBANCE AND SEDIMENT MIXING USING A GOUGE CORER WOULD BE MINIMAL. (PERHAPS THE AUTHORS HAVE NEVER USED A GOUGE CORER?) THIS REVIEWER HAS USED A GOUGE CORER IN BC SALT MARSHES AND FOUND THAT IT CAN BE VERY EFFECTIVE, PARTICULARLY IN THE SHALLOW DEPOSITS FOUND IN THIS STUDY. TO SAMPLE WITH A SHOVEL (SPADE IS BEST) REQUIRES A *HOLE*, *NOT A PIT* AND ONCE THE BLOCK OF SEDIMENT IS CORED THROUGH, THEN THE SURROUNDING MATERIAL CAN EASILY BE PLACED BACK IN THE HOLE, SOMETHING THAT THIS REVIEWER HAS FOUND TO BE A SUCCESSUL APPROACH.

NOTE THAT THESE SAME SUGGESTIONS FOR USE OF CORERS FOR QUESTIONS OF CARBON ACCUMULATION ARE FOUND IN

*1) COASTAL BLUE CARBON: METHODS FOR ASSESSING CARBON STOCKS AND EMISSIONS FACTORS PUBLISHED BY THE BLUE CARBON INITIATIVE AND FREELY AVAILABLE ONLINE (*http://thebluecarboninitiative.org/manual/)

AND BY

2) SMEATON C, BARLOW NLM, AUSTIN WEN. 2020. CORING AND COMPACTION: BEST PRACTICE IN BLUE CARBON STOCK AND BURIAL ESTIMATIONS. *GEODERMA* 364

SMEATON ET AL NOTE: "A COMPARISON OF GOUGE AND HAMMER CORING TECHNIQUES IN INTERTIDAL WETLAND SOILS HIGHLIGHTS A SIGNIFICANT EFFECT OF SOIL COMPACTION OF UP TO 28% ASSOCIATED WITH THE WIDELY APPLIED HAMMER CORING METHOD EMPLOYED IN BLUE CARBON RESEARCH. ……. WE SHOW THAT HAMMER CORING IS UNSUITABLE FOR THE CALCULATION OF OC STOCKS AND SHOULD BE AVOIDED IN FAVOUR OF RUSSIAN OR GOUGE CORES. COMPACTION CHANGES BOTH SOIL DRY BULK DENSITY AND POROSITY AND WE SHOW THAT RESULTANT RADIOMETRIC CHRONOLOGIES ARE COMPROMISED, ALMOST DOUBLING MASS ACCUMULATION RATES. WHILE WE SHOW THAT THE OC (%) CONTENT OF THESE SEDIMENTS IS LARGELY UNCHANGED BY CORING METHOD, THE IMPLICATION FOR OC BURIAL RATES ARE PROFOUND BECAUSE OF THE SIGNIFICANT EFFECT OF HAMMER CORING ON THE CALCULATION OF SOIL MASS ACCUMLATION RATES."

THUS, AUTHORS MUST NOT REPORT ACCUMULATION RATES IN THOSE CORES THAT SUFFERED EXCESSIVE COMPACTION – THOSE COMPACTED GREATER THAN 20% MUST NOT BE USED FOR ACCUMULATION RATES.

AUTHOR RESPONSE CONTINUES

However, when we do need to account for compaction (e.g. Figures 2-3), we use a compaction factor (Howard et al. 2014; Gailis et al. 2021) estimated for each core by dividing the length of core penetration by the length of core recovered (Table A1)."

REVIEWER COMMENT ON AUTHOR STATEMENT AND REVISED TEXT

THE CITED PAPER BY HOWARD ET AL 2014 MAKES NO MENTION OF COMPACTION AND GAILIS ET AL IS NEITHER A METHODS NOR A REVIEW PAPER, THUS NEITHER CITATION IS AN APPROPRIATE SUPPORTING REFERENCE

3. Reviewer initial comment

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 using the following equation, setting any negative %C value resulting from the use of a negative intercept equal to zero

REVIEWER COMMENT ON AUTHOR STATEMENT AND REVISED TEXT

AUTHORS DO NOT SEEM TO UNDERSTAND THE COMMENT – FORCING THE LINEAR REGRESSION THROUGH ZERO DOES NOT MEAN SIMPLY DROPPING THE INTERCEPT AFTER OBTAINING THE REGRESSION MODEL. WHEN RUNNING THE REGRESSION ONE SIMPLY CHOOSES NO INTERCEPT FOR THE MODEL, THUS A NEW REGRESSION EQUATION IS REQUIRED. DOING THIS WOULD SIMPLY MEAN THAT 0% LOI IS EQUIVALENT TO 0% C. AUTHORS HAVE NO REASON TO CONCLUDE THAT FORCING THE REGRESSION THROUGH ZERO WOULD OVERESTIMATE THE %C.

4. Reviewer initial comment

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." However, we have kept use of the term "humus" as term that has been used as a descriptor in other salt marsh publications (e.g. Goni and Thomas, 2000; Santin et al. 2008)

REVIEWER COMMENT ON AUTHOR STATEMENT AND REVISED TEXT

AUTHORS ACTUALLY DELETED HUMUS FROM THE TEXT, BUT NOT THE TABLES. AUTHORS WILL NEED TO FIND AN ALTERNATIVE TERM FOR HUMUS – ONE THAT IS WIDELY USED BY THOSE WORKING WITH SALT MARSH SOILS, NEITHER PAPER CITED SUPPORTS THEIR USE OF HUMUS. SANTIN ET AL NEVER USE THE TERM HUMUS – THEIR STUDY IS ABOUT HUMIC ACIDS. GONI AND THOMAS SIMPLY USED THE TERM HUMUS TO IDENTIFY A PARTICULAR SIZE FRACTION OF ORGANIC MATTER, NOT AN ENTIRE PORTION OF THE SOIL.

5. **Reviewer initial comment**

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 on LN 608: (e.g. Abdul-Aziz et al. 2018; Huang et al. 2019; Huertas et al. 2019; Li et al. 2021; Wei et al. 2020.).

<u>REVIEWER COMMENT ON AUTHOR STATEMENT AND REVISED TEXT</u>

THE TEXT RELATED TO METHANE SHOULD BE DELTED ENTIRELY.

AUTHORS WRITE "THE MESOTIDAL NATURE OF SOME OF THESE MARSH LOCATIONS COULD MEAN THAT SOME OF THESE MARSHES EMIT SUBSTANTIAL CONTRIBUTIONS OF METHANE, WHICH MAY COUNTER THEIR EFFECTS AS C SINKS (E.G. ABDUL-AZIZ ET AL. 2018; HUANG ET AL. 2019; HUERTAS ET AL. 2019; LI ET AL. 2021; WEI ET AL. 2020)." THESE PAPERS ARE ABOUT DURATION OF FLOODING – GREATER TIDAL RANGES MEAN LOWER HYDROPERIODS, NOT LONGER. THUS ONE WOULD EXPECT LESS METHANE PRODUCTION AND CERTAINLY OXIDATION OF THE METHANE THAT IS PRODUCED. NOTE THAT CHMURA FOUND NEGLIBLE METHANE EMISSIONS IN A MACROTIDAL MARSH.

NOTE SOME OF THE REFERENCES ARE IDENTIFIED BY URLS ONLY ACCESSIBLE TO THE SIMONE FRASER UNIVERSITY SYSTEM!