BG-2021-157 – 2ND RESPONSE TO REVIEWERS - 2 September 2022

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 Dr. Chmura for her review of this manuscript. Dr. Chmura outlined 5 major revisions for us to address. Below we detail our responses to her comments. In our response we:

1. *Attempt to find terminology that both accurately reflects definitions of "bay" and "estuary" while also being suitable to the reviewer,*
2. *Provide a detailed description (with photographs) as to why the reviewer's choice of coring device is not suitable for our study area,*
3. *Refute the reviewer's claim that our compacted cores are unsuitable for estimations of 210Pb accumulation rates, describe the flaws in the Smeaton et al. (2020) arguments, and demonstrate via a modeling exercise how accumulation rates can be properly estimated in terms of cumulative mass instead of depth (which is what we have done).*
4. *Explain why there is no processed-based reason why the empirically derived %C-%LOI relationship needs to have a zero intercept, and provide multiple examples from the literature where similar, regional relationships with non-zero intercepts have been used.*
5. *Replace the term 'humus' with 'soil,' which is further described in table and figure notes*
6. *Replace the term 'mesotidal' with 'brackish' to circumvent the discussion around tidal height and duration and focus on future work needed to assess the impact of methane emissions from brackish marshes on the total carbon budget.*

We thank the editor for the opportunity to elucidate these points and hope that these responses are sufficient for publication.

- *Original reviewer comments are* ***bold grey****; our original responses are italicized grey.*
- *New reviewer comments are* ***bold red****; our response to new comments is italicized red.*
- *We provide the location of changes using the line numbers from the 'track changes' version of our resubmitted manuscript, with change and associated line numbers highlighted in yellow.*

Sincerely,
The authors

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.

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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)."

REVIEWER COMMENT ON AUTHOR STATEMENT AND REVISED TEXT
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".

Author Response:
We appreciate this clarification, but unfortunately the request from the reviewer remains unclear. We examined the Kelley et al. (1995) paper, but it is neither a review paper nor a methodology paper: its purpose was to discuss the specific evolution of back barrier marshes along the Webhannet and Little Rivers of Wells, Maine USA. The paper itself does not describe a list of geomorphological terminology applicable to the characteristics of the salt marshes examined on the west coast of Vancouver Island. In our previous revision, we therefore attempted to add a comparison with the Van Ardenne et al. (2018) paper because of its focus on geomorphological classification and its relationship with carbon dynamics. We are happy to remove this comparison.

We are also puzzled by the reviewer's definition of an estuary versus a bay. Essentially, an estuary is a partially enclosed coastal body of brackish water with one or more rivers or streams flowing into it, and with a free connection to the open sea (Pritchard, 1967). In contrast, a bay is simply a depression marked by a penetration whereby land-locked waters are contained by the proportion of the width of its mouth (United Nations Convention on the Law of the Sea). Therefore, an estuary can exist within in a bay, but a bay is not necessarily an estuary.

With this said, we have changed the section to state (line 103):
"These sites are typical of salt marshes along Canada's Pacific coast because they include small marshes along protected shorelines and bays as well as larger estuarine marshes near creeks and rivers."

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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).

REVIEWER COMMENT ON AUTHOR STATEMENT AND REVISED TEXT

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

Author Response
The reviewer brings up two points here: (1) the effect of coring device on compaction, and (2) the potential impact of compaction on calculated accumulation rates. We address these two points separately below.

POINT 1. We appreciate that the reviewer has decades of experience coring marsh sediments, but so do some of the authors of this manuscript. We absolutely agree that there are issues with percussion coring, but we also are well aware of the issues with gouge corers and Russian corers. These corers are not suitable for our study site as we found the compression and disturbance from a Gouge corer and Russian corer to be unacceptable. We have included some photographs (Figures 2.1-2.3) showing the compaction (~50% caused by a gouge corer, and although hard to see, the inability to rotate the Russian corer in the mix of sand and sticky sediment at the sites. Note as for using a shovel, we have stated that this was not permissible in a National Park Reserve.

Fig 2.1. Gouge corer in Grice Bay marsh sediments.

Figure 2.2. Note compression of sediments with the gouge core. Black line is depth of core. Note sediment is compresses over 50%.

Figure 2.3. We were unable to recover a sediment core with the Russian Corer due to disturbance on resistance of sediment.

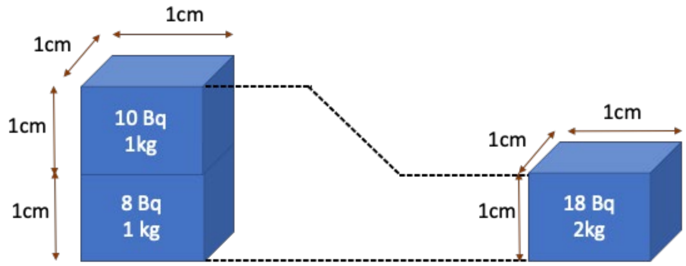
Note that, according to Frew (2014), Gouge augers are easily transportable tools that permit relatively quick survey of subsurface sediments in terrestrial environments. Sampling is particularly rudimentary and involves thrusting a semi cylindrical chamber into deposits and twisting the device using a handle at the surface to capture the sample. Consecutive drives are enabled by the addition of extension rods. The retrieved sample is subject to significant disturbance as the open chamber is prone to resampling of material from depths above those required, especially where sands underlie the softer organic material above. Additionally, more-consolidated material can force its way upwards over less consolidated horizons within the chamber. For these reasons, it is not recommended that the gouge auger be used to retrieve samples for analysis. (Frew, 2014).

As for Russian corers, De Vleeschouwer et al. (2010) state "The Russian corer is not designed to cut the living plant mat cleanly and will strongly compress the core." This is particularly problematic in sandy/gravelly sediments where researchers have identified: "The Russian corer is used to core terrestrial and wetland soft sediments; clay, gyttja, or peat, but cannot be used to core in sand or other coarse-grained sediments." https://corerepository.ldeo.columbia.edu/content/types-samples.

As such, we have left the text on lines 153-158 unchanged.

POINT 2. Regarding the use of compressed cores to determine carbon accumulation rates: as we mention in our manuscript, compaction is not a problem when calculating stocks to the base of the marsh deposit. However, measurements of the stocks at a fixed depth in compacted cores lead to a misleading impression of salt marshes' organic carbon sequestration efficiency due to varying densities and accumulation rates. We added a short description about the advantages of using stocks accumulated at a common age-horizon instead. Contrary to what the reviewer states in her comments, compaction does not affect the calculation of the ^{210}Pb-derived accumulation rates when they are estimated in terms of cumulative mass ($g\ cm^{-2}$) and not depth (cm). In the text below, we discuss why the paper Smeaton et al. (Geoderma 2020) cited by the reviewer is fundamentally wrong when claiming that ^{210}Pb dating models do not work in compacted cores. We also simulate compaction in an undisturbed core to evaluate the potential differences in the ^{210}Pb derived chronology.

Smeaton et al. (2020) claim that compaction affects the calculation of mass accumulation rate (MAR) ($g\ cm^{-2}\ yr^{-1}$) when applying the conventional ^{210}Pb dating models. To prove this, the authors combine a published ^{210}Pb profile from a saltmarsh (Barlow et al., 2014) with density data from two new cores collected using gouge and hammer cores. The densities of these new cores are higher than the original core due to compaction during sampling. Although the objective of the paper is to evaluate the applicability of the ^{210}Pb dating models in compacted cores, the paper's methodology is fundamentally flawed because compaction does not only affect the density of the material but also the distribution of a certain element (^{210}Pb in this case) along the profile. For that reason, density and porosity profiles and ^{210}Pb concentrations (Bq/kg) must be corrected to model the effects of compaction on the ^{210}Pb-derived chronologies correctly. To highlight that systematic error bias occurs in both density and ^{210}Pb concentrations, we provide a hypothetical example of the compaction of two sections or slices of a saltmarsh core (Figure 2.4). In Figure 2.4, two consecutive core sections (1 cm thick) have been compacted, resulting in one single slice (1 cm) (we keep the slicing at 1 cm thick as is usually done in these types of studies). While the concentration of ^{210}Pb in intact sections are 10 and 8 Bq/kg, respectively, the resulting section corresponds to 9 Bq/kg (18 Bq of ^{210}Pb distributed in 2 kg of mass), which shows that the vertical distribution of ^{210}Pb along the core changes under compaction. Thus, (Smeaton et al., 2020) should have also corrected the ^{210}Pb concentration profile due to compaction before applying the ^{210}Pb dating model.



$$C_1 = \frac{10\,Bq}{1\,kg} = 10\,Bq/kg \qquad I_1 = 10\,Bq/cm2 \qquad C_1' = \frac{18\,Bq}{2\,kg} = 9\,Bq/kg$$

$$C_2 = \frac{8\,Bq}{1\,kg} = 8\,Bq/kg \qquad I_2 = 8\,Bq/cm2 \qquad I' = \frac{10 + 8\,Bq}{1\,cm^2} = 18\,Bq/m2$$

Figure 2.4. *Effect of compaction on ^{210}Pb concentration (Bq/kg) and inventory (Bq/m^2).*

Another piece of evidence showing that (Smeaton et al., 2020) applied the ^{210}Pb dating models erroneously is the extremely old ages found in the compacted cores (Figure 4 in paper). Due to the half-life of ^{210}Pb (22.3 years) and large uncertainties usually found in the older layers due to the low concentration of ^{210}Pb, ^{210}Pb dating models usually provide accurate chronologies for the past 100-150 years. Thus, the 200-250 years found in (Smeaton et al., 2020) seem to suggest some errors when the ^{210}Pb dating models were applied. These extremely old ages can be ascribed to using higher density values for the gouge and hammer cores while keeping invariant the ^{210}Pb profile concentrations. Besides providing inconsistent marsh ages, increasing density values without changing ^{210}Pb concentrations increases the amount of ^{210}Pb accumulated in the core (Inventory, Bq/m^2). This is a big mistake, as the flux of ^{210}Pb (flux of ^{210}Pb (Bq/m2/yr= ln(2)/22.3 years·Inventory (Bq/m2)) in a given area is constant. Thus, all cores must have the same ^{210}Pb inventory. We tried to find the raw data that Smeaton et al (2020) used to estimate the different age-depth models, but neither the ^{210}Pb concentration profiles nor the density profiles are provided in the paper.

To further prove that the chronology derived from the CFCS model (model that we used in our manuscript) is not affected by compaction when depth is represented as cumulative mass (g cm^{-2}) and not in cm, we have simulated compaction on an initial undisturbed tidal marsh sediment and evaluated potential deviations in mass accumulation rates (MAR, g cm-^{2} yr^{-1}) and chronology. We used the ideal excess ^{210}Pb profile of seagrass sediment provided in (Arias-Ortiz et al., 2018). We chose the seagrass profile and not the tidal marsh provided in the review because the length of the tidal marsh was higher than 1 m, which made the calculations more difficult to follow. The ideal ^{210}Pb profile was modelled considering the following:

1) *A constant flux of excess ^{210}Pb of 120 Bq m^{-2} yr^{-1}.*
2) *A mass accumulation rate of 0.2 g cm^{-2} yr^{-1}.*
3) *And a dry bulk density (DBD) of 0.1.03 g cm^{-3}.*

We assumed that the ideal core was subjected to a 50% compaction during sampling, meaning that its original length (30 cm) was reduced to its half (15 cm)(see Appendix for detailed calculations). We assumed that the compaction occurred homogenously along the whole profile and combined *two consecutive marsh layers in one. After this, we recalculated the cumulative mass (g cm^{-2}), excess ^{210}Pb inventory (Bq m^{-2}) and ^{210}Pb concentration (Bq kg^{-1}) per each layer. Then, the CFCS was applied in both cores (uncompacted and compacted) using cumulative mass (g cm^{-2}) instead of depth (cm) (Figure 2.5). Results showed that the CFCS model provided similar chronologies for both the ideal and the compacted core (Figure 2.6), which confirms that compaction does not affect the derived MAR when those are obtained using the cumulative mass profile (Figure 2.4 and 2.5, Table 1**). Differences in MAR between ideal and compacted cores were only 0.18%.*

With this, we have proven that compaction does not affect MAR and marsh ages when the ^{210}Pb models are applied using cumulative mass instead of depth. As we used this methodology to estimate our CAR and stocks, we can confirm that our results are valid.

We also provide two papers (Gifford & Roderick, 2003; Wendt & Hauser, 2013) where the use of a single equivalent soil mass layer from the surface, or the use of cumulative mass coordinated, is described and used to facilitate organic carbon quantification in soil organic layers.

In our current revision, we have addressed this point in ln 161-163, where we have (a) indicated that our 210Pb-derived accumulation rates are calculated using cumulative mass, (b) have provided a reference to Gifford and Roderick (2003), and (c) have made clearer (as per the reviewer's request) that we have followed a method previously used in Gailis et al. (2021):

"Furthermore, when we have estimated ^{210}Pb-derived accumulation rates (Figure 6), we have done so in terms of cumulative mass (g cm^{-2}) instead of depth (e.g. Gifford and Roderick, 2003). When we do need to account for compaction (e.g. Figure 3), we use a compaction factor as described in Gailis et al. (2021), estimated for each core by dividing the length of core penetration by the length of core recovered (Table A1)."

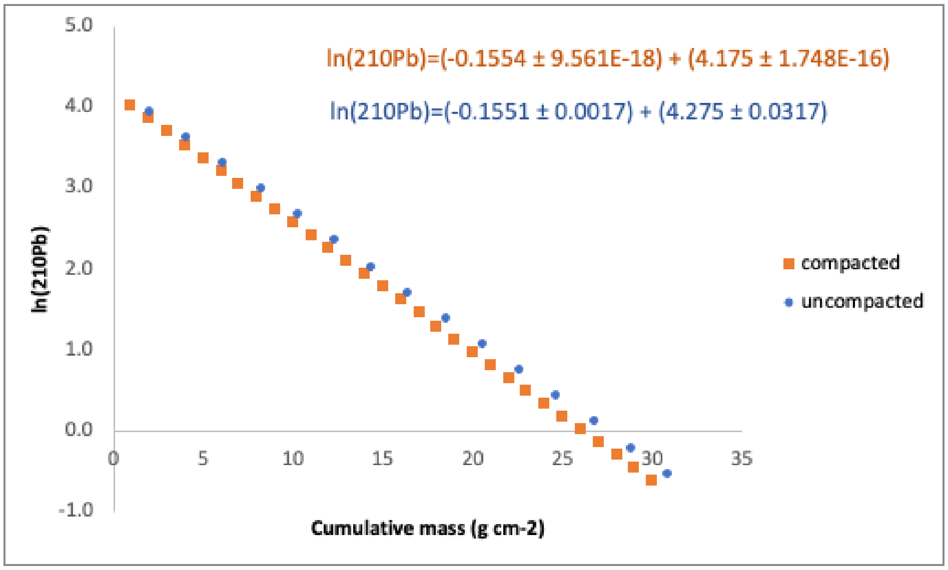


Figure 2.5. *Ideal and compacted ^{210}Pb concentration profile.*

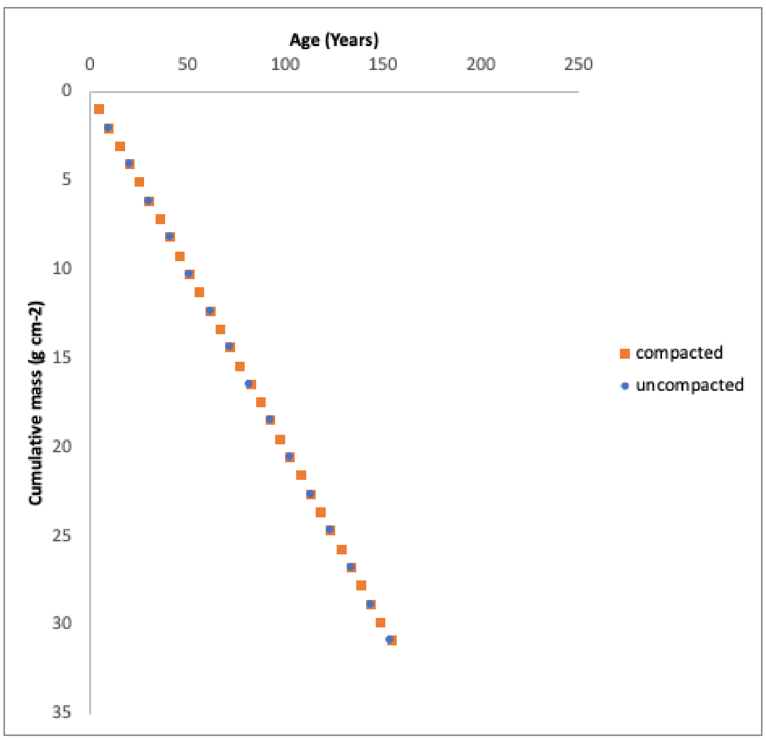


Figure 2.6. *Age-depth (expressed as cumulative mass) model obtained after applying the CF:CS model for an ideal (uncompacted) and compacted core.*

Table 1. *Comparison of MAR between the ideal and compacted ^{210}Pb profile.*

Core	*Total depth (cm)*	*Cumulative mass ($g\ cm^{-2}$)*	*MAR ($g\ cm^{-2}\ yr^{-1}$)*
Ideal	*30*	*30.9*	*0.2000*
Compacted	*15*	*30.9*	*0.2004*

*****Please note: We will gladly make the excel file containing these calculations available to the editor upon request***

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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.

We understood the original purpose of the reviewer comment, which requested that we recalculate the regression relationship between %LOI and %C so that it is forced through zero and then, accordingly, revise all estimates (and figures) of %C, Carbon stocks, soil carbon densities, and carbon accumulation rates, etc. However, we disagree with the original premise of this comment, i.e. that the relationship between %LOI and %C must have an intercept of zero.

Several previous studies have indicated that LOI has the potential to overestimate soil carbon because the ignition process drives off both organic matter as well as water bound in any clay minerals that are present in the sample (e.g. Howard, 1966; Howard and Howard, 1990; Santisteban et al. 2004). Howard (1966) originally showed that the intercept for zero %C was actually 2% LOI in the soils they examined. Santisteban et al. (2004) produced an intercept of -1.83 ($\%OC = 0.634\ LOI_{550}\ (\%) – 1.83$) and showed that the intercept depended on the type of soils compared. Poppe and Rybczyk (2021) used a polynomial relationship to account for the lower %C values at small values of %LOI and still produced an intercept of –0.4496 ($\%OC = 0.0035\ \%LOI^2 + 0.4135\%LOI – 0.4496$). In their revision of estimates of global carbon stocks, Ouyang and Lee (2020) also produced an empirical relationship with a non-zero intercept for salt marshes ($\%OC = 0.52(\%LOI) – 1.17$).

In summary, these studies indicate that there is no process-based reason for forcing this regression equation through zero, and in fact, the nature of the loss-on-ignition method suggests that we are more likely to expect there to be some small value of %LOI when %C reaches zero. As a result, forcing the intercept through zero could artificially inflate the estimates of %OC at low values of %OC. Most of these publications also suggest that soil-specific, empirical equations are the best approach for determining %OC from %LOI. As such, we have kept the original equation and left the text as written.

Finally, we note that, ultimately, the choice of these two equations makes very little difference to our final estimation of carbon accumulation rates. Below we compare the average carbon accumulation rates (+/- SE) for the six cores on which we conducted 210Pb measurements and therefore also estimated carbon accumulation rates. We show that forcing the regression equation through zero actually makes only a minimal difference to the final estimates of CAR, as differences are within the estimated standard error for each core (Figure 3.1).

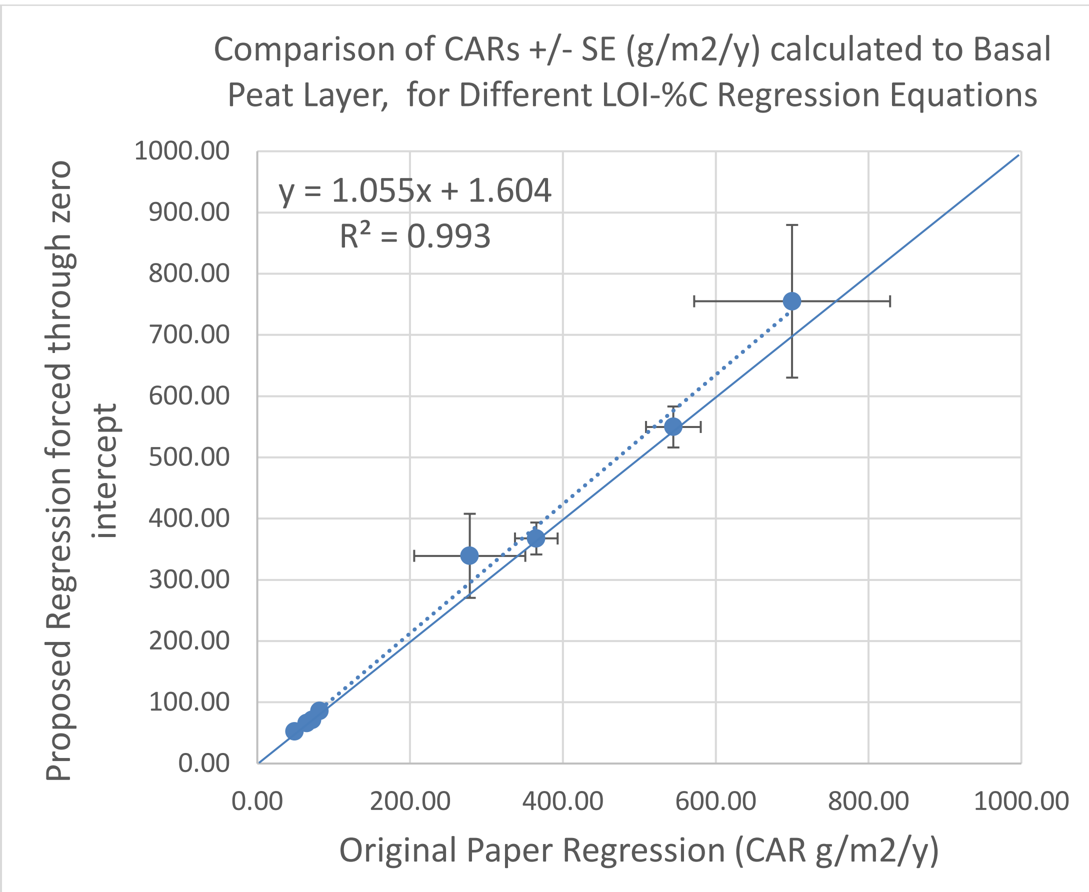

Figure 3.1: Comparison of carbon accumulation rates estimated to basal peat layer using original and proposed (zero-intercept) regression equations, demonstrating that both options are within the estimated standard error for each core. Dotted blue line shows the regression line between these two calculations, and the solid blue line represents a 1:1 line. Regression through zero intercept was: %C = 0.411%LOI (R^2 = 0.98). Original regression as stated in paper: %C = 0.44 %LOI – 1.9 (R^2 = 0.96).*

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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.

We used the term "soil" to describe the surface organic layer that is distinct from the underlying peat layer. This is defined in Table A2 and in Figure A1.

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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.).

REVIEWER COMMENT ON AUTHOR STATEMENT AND REVISED TEXT
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!

Thank you for this clarification. The goal of this text is to point out that some of these marshes are BRACKISH, at least seasonally, and that brackish marshes can emit methane and therefore affect the overall *carbon sequestration potential. We did not intend to delve into a specific discussion of role of tidal range / duration on methane emissions. Rather, we wish to indicate that future work on understanding the carbon/greenhouse gas dynamics requires further investigation in these systems. The references provided – along with the original Poffenbarger et al. (2011) paper - support this point. We have therefore changed the work "mesotidal" to "brackish" in this sentence (LN 579-581).*

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