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Interactive comment on "Carbon Stocks and Accumulation Rates in Salt Marshes of the Pacific Coast of Canada" *by* Stephen G. Chastain et al.

Anonymous Referee #2

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General comments: Assessing the organic carbon stocks and accumulation rates of vegetated coastal ecosystems is a topic of interest in recent years, increasing number of papers aim at quantifying the potential of these ecosystems to mitigate CO2 emissions through their management in an approach described as Blue Carbon. However, limitations such as the scarcity of estimates of organic carbon accumulation rates, uncertainties in the area covered by these ecosystems or the large areas still unsampled are precluding their inclusion into existing carbon mitigation strategies. The article by Chastain et al. aims to address a data gap in saltmarsh ecosystems by providing new estimates of organic carbon stocks and accumulation rates in saltmarshes of Clayoquot Sound, in the Pacific Coast of Canada, where no data are available. The scope of the paper is valid- It is important to increase the number of available data on C stocks

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and, in particular, on carbon accumulation rates in these ecosystems to enlarge provincial, national, and global databases to finally develop policy priorities for conservation. While the data compilation on organic carbon stocks presented in this manuscript is commendable, insufficient data on carbon accumulation rates (CAR) is provided for the purpose of estimating carbon accumulation rates in the Pacific Coast of Canada. I found major flaws in information regarding the estimation of sediment accumulation rates (SAR), hence CAR. Also, there are a series of miss-points in the methodology used for the estimation of C stocks and accumulation rates (detailed in the comments below) that the authors should take into consideration to achieve the publication of this work.

Specific comments on the estimation of carbon accumulation rates (CAR):

I have major concerns about how carbon accumulation rates (CAR) are estimated. First, authors only estimate CAR in a total of five cores collected at 4 marshes, although they sampled a total of 34 sediment cores for C stock determination. The authors do not explain why only these cores were dated, or whether other cores were also analyzed by 210Pb but could not be dated. Mixing, erosion or changes in sedimentation are common processes in coastal sediments, and could lead to the alteration of sediment records, hence 210Pb concentration profiles (Ruiz-Fernández and Hillaire-Marcel, 2009). However, altered 210Pb profiles, although not datable, are results themselves.

The authors do not report data on total or excess 210Pb specific activities and no explanations are given regarding the determination of supported 210Pb, which might vary between marshes but also along the depth of their sediment profiles, especially if soils consist of three marked layers, topsoil, peat, and sand/clay (section 3.1, line 16-17).

The application of the CRS dating model to estimate SAR is unclear and some arguments should be provided regarding the election and application of this model. To

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apply this model certain assumptions must be met, for instance, this model is based in excess 210Pb inventories, which implies that the excess 210Pb horizon should have been reached in all dated cores. Without 210Pb data, this is impossible to evaluate. In addition, the CRS model provides estimates of SAR at each sediment layer rather than average sedimentation rates for the last century. In the main text Chastain et al. report average SARs at each core but do not explain how this average is estimated or if they have normalized SARs to a certain age-depth.

A results section showing 210Pb concentration profiles, 210Pb inventories and estimated fluxes should be included in the paper, this is important to evaluate whether the dating model applied is valid and to discuss the uncertainties associated to the estimation of ages and SAR. In the current version of the manuscript the authors include a section comparing 210Pb and 137Cs dating, which I believe is unnecessary; the authors did not analyze 137Cs in their cores and 137Cs is most commonly used to validate 210Pb chronologies. There are many aspects that can bias SAR and CAR high, for instance the presence of sediment mixing in 210Pb concentration profiles. My recommendation to the authors is to look critically at their 210Pb data and discuss the uncertainties related to their age-depth models, SAR and CAR estimates.

Second, to estimate CAR authors use sediment accumulation rates (SAR) which they multiply by the soil carbon density (SCD). While they acknowledge that sediment compaction occurred during coring and so they correct SAR for potential compaction, they do not correct SCD for such. This might lead to an overestimation of CAR. The authors estimate SCD multiplying the percent carbon content (%C) by the soil dry bulk density (DBD). While the rationale behind this is correct, soil DBD should be corrected for core compaction prior to the estimation of SCD. The mass contained in one cc volume of soil after coring occupies a greater volume in the field (before compaction occurs). Related to core compaction, I disagree with the statement in equation 8 used to estimate the uncompacted depth of a given subsample. Let's assume the recovered core length is 50 cm and the core penetration is 100 cm. This would result in a correction factor of

0.5 following equation 7. Then, if the correction factor is applied in equation 8 and is multiplied by the subsample depth (i.e., 1 cm-thick slice) this do not result in an uncompacted depth, please revise. In addition, compaction would unlikely have been linear throughout the soil column due to the presence of different soil layers (topsoil, peat, and sand/clay), which may show different degrees of compaction. For this reason, any variable that is sensitive to soil compaction such as DBD, SCD and SAR should not be used for the determination of CAR or C stocks. Variables such as the mass depth (m) (or mass per unit of area; g cm-2) and mass accumulation rate (MAR; g cm-2 yr-1) are not affected by soil compaction, then should be used instead of DBD and SAR to avoid the propagation of errors in the determination of CAR or C stocks (see below). My recommendation to the authors is to recalculate CAR as:

CAR (g C m-2 yr-1) = MAR (g cm-2 yr-1) x %C

Where the % C is not the average percentage of C along the sediment column but the fraction of the accumulated mass of C (gC cm-2/ g soil cm-2), estimated from the sum of the sediment layers accumulated over a period t = 100 yr, which should be approximately where the excess 210Pb horizon is reached.

To finish with concerns about CAR estimates, I think differentiation between low and high marsh CAR is not possible with only one CAR estimate for a low marsh. The authors indeed acknowledge this at the end of the manuscript in section 4.4, line 3-4. I believe this should be said upfront. Accordingly, comparisons of Clayoquot Sound CAR with other salt marshes should be based only on high marsh CAR estimates reported at the other study sites. Final recommendation to the authors would be to avoid estimating total CAR for a marsh with only a dated core as the high marsh core CAR times the total marsh area (this is represented as a crosshatched column in figure 4). The latter is probably unlikely according to the results presented: lower C stocks in low marsh cores and low CAR in the single low marsh core.

Specific comments on the estimation of C stocks:

Similarly, estimation of C stocks should be done using soil mass depth (g cm-2) rather than SCD multiplied by the thickness of soil slices, i.e., 1 cm, which is affected by soil compaction. My recommendation to the authors is to recalculate C stocks using the soil mass per unit area (m) rather than the sum of all sections DBD x %C x 1 cm. The soil mass per unit area at each layer is not affected by compaction or by inaccurate slicing. It is estimated by dividing the dry sample mass by the area sampled by the core tube, which is the cross-sectional area of its inner diameter (D), π(D/2)ˆ2:

Cstock_core=$\sum(DW/(\pi$rˆ2)) x %C

The second problem here is the computation of overall averages when the averaged values are computed over a number of estimates that are different at each site or when the area each marsh represents is not the same. - The mean C stock at a marsh (C stockmarsh) should be calculated as the weighted average of the mean Cstockscore estimated in the low marsh area and the mean of those estimated in the high marsh area, being the weights, the area made by low and high marsh at each individual marsh. - Then, the average C stock of low marshes at Clayoquot Sound (Cstock-LowCS) also should be a weighted average, with weights being the low marsh area of each individual marsh. Same for CstockHighCS.

The authors use the depth of refusal (DoR) as a measure of the maximum depth of organic accumulation. C stocks are then estimated down to this depth (average 27.6 cm) and compared with those of global estimates (which some are estimated down to 1 m and others extrapolated to the same depth, 1 m). The authors conclude the C stocks at Clayoquot Sound are lower than those globally, but this is not a fair comparison. DoR is relative to the equipment being used and to the type of soils, therefore I feel that any comparisons made without standardizing all sites to a certain depth/mass depth (preferably) can be misleading. Rather than extrapolating their measurements to 1 m, the authors could normalize global estimates to 30 cm or perhaps to a certain mass depth, which would be the most consistent to establish comparisons (see Wendt and Hauser, 2013). As well, authors could discuss differences on C stocks based on %C, DBD and CAR rates found globally and at other regions such as eastern Canada, the Pacific Coast of the United States and Mexico.

Authors should take action on the points listed above and revisit their calculations to provide more consistent estimates of C stocks and CAR. As well, they should discuss their results, perhaps, with more emphasis on C stocks and intra marsh variability (for which they have a good dataset), while presenting CAR results in a more local scale, avoiding upscaling to the Pacific coast of Canada. Instead, I encourage the authors to discuss temporal trends in C accumulation at the dated marshes if 210Pb profiles allow so.

Minor comments:

- Introduction; line 2: "Coastal, vegetated ecosystems, such as eelgrass meadows, mangroves. . ." why only eelgrass? Haven't the other seagrass species been recognized for their ability to store carbon?

-Section 2.2 line 15: please, add in the text the percentage area of ditches and channels rather than directing the reader to section 4.4.2 which does not exist in the present version of this manuscript.

- In section 2.2 there are some aspects the authors should mention such as the length of the PVC tubes and the technique to slice sediment cores. Whether the cores were cut lengthwise or were sliced using a sediment extruder, are important details since compaction may become greater using the latter.

- Figure 2: In the present version of the manuscript authors evaluate C stocks in high and low marsh sites, while the distance to shore is not really a variable they evaluate. Because of that, my recommendation would be to represent percent C profiles using either two colours/shapes for low and high marsh.

-Figure 3 is repetitive if figure 2 is already presented, in addition figure 3 may be misleading as SCD has not been corrected for compaction.

- Section 3.2 Carbon storage and marsh area: DoR measurements at each core/site are key to understand differences in C stocks within sites, the authors should provide this information either in table 1 or Table A1. Then, since C stocks and DoR are highly connected, my suggestion to the authors is to merge section 3.2 and 3.4.

- Appendix C: please, provide uncertainties associated to SAR estimates.

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