RESPONSE TO REVIEWER COMMENTS: 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 both reviewers for their helpful and detailed reviews of this manuscript. Below we detail how we have addressed each review within our revised manuscript. Comments from reviewers are in bold; our responses are italicized. We provide the location of changes using the line numbers from the 'track changes' version of our resubmitted manuscript, with line numbers highlighted in yellow.

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.

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)."

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.

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.

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).*

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.

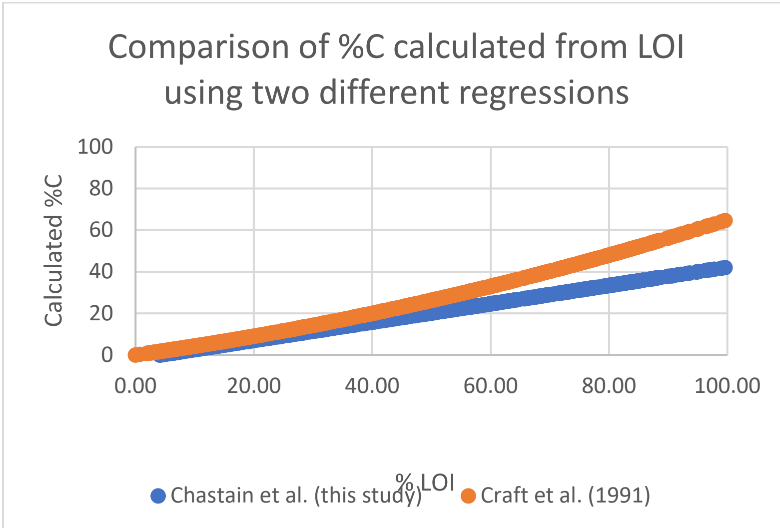

Figure R1.1 (new Fig A3 in appendix). Comparison of %OC calculation using the empirical regression determined for our study area (blue) and the model of Craft et al. (1991) (orange).

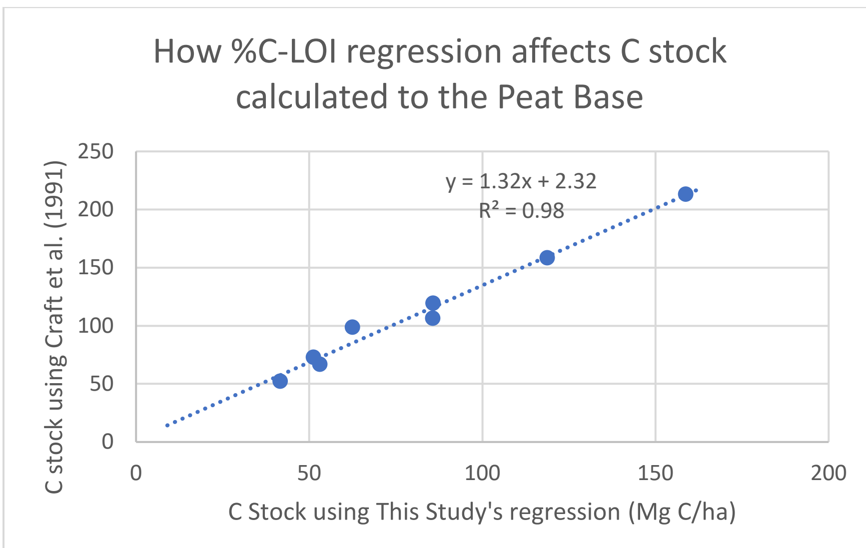

Figure R1.2 (new Fig A4 in appendix). Comparison of C stocks (Mg C/ha) estimated using the Craft et al. (1991) equation versus C stocks estimated using this study's empirical relationship for southern BC, for 8 cores from our study region. Comparison suggests that using the Craft et al. (1991) relationship would produce C stocks that are ~32% greater than our estimates. This difference, while substantial, would not account for global C stocks that are 3 times larger than those found in Clayoquot Sound salt marshes.

To address this concern, we have added the following text to the Discussion (ln 391-402): "The C stocks in Clayoquot Sound marshes are less than one-third of the globally averaged estimate of 250 Mg C ha^{-1} for salt marsh C stocks in the upper 50 cm (Chmura et al. 2003; Pendleton et al. 2012). This is true whether we consider the base of peat, 20-cm, or 30-yr *estimates (Table 1). One possible contributor to these discrepancies could be the use of different formulas relating %C to LOI. Chmura et al. (2003) utilized the formula of Craft et al. (1991) to convert LOI measurements to %C. In contrast, we used an empirical relationship based on measurements collected from Clayoquot Sound marsh samples. Comparison of these two regression equations suggest that estimates of %C are very similar for values of %C equal to or less than 30%, but the %C estimates diverge for percentages above 30% (Fig. A3), with calculated differences in %C exceeding 20% at %LOIs above 80%. However, we note that the interquartile (Q1:Q3) range of our %C values fall between 4.4 and 28.8%, suggesting that most of our samples have %C values of less than 30%, where the two equations produce similar results. To test the potential impact of the different equations, we compared C stocks (estimated to peat base) that were calculated using the two different %C-LOI relationships for the eight cores that were ^{210}Pb dated (Fig. A4). Using the Craft et al. (1991) regression inflates our C stock values by about 30%, but this increase is not sufficiently large to account for the full difference between our C stocks and the global values in Chmura et al. (2003), which are three times greater than our estimated C 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.

We agree that these caveats should be added to the paper and have updated this discussion based on more recent research on the impact of climate change and wildfires on the permanence of carbon storage (see Section 4.4)

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. To address this point we have incorporated 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) prior to our main comparison with the compilation in the Discussion (ln 437-441):*

"One effort has been made to assemble a global compilation and synthesis of CARs within salt marsh ecosystems (Ouyang and Lee, 2014). Here we compare our results with this compilation with the caveats that (a) Ouyang and Lee (2014) relied heavily on ^{137}Cs dating or marker horizon methods for their estimates, and (b) appears to have some instances of double-counting of sites (e.g., averages of 3 sites were included as a 4th site) and minor issues with attribution of some geographic locations. That said, we note that…"

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).*

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)."

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 (LN 193).

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)

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.).

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 have changed this term to "upland 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.
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*.
changed.

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

Line 185 - Khrishnaswamy should be spelled Krishnaswamy
changed.

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.
"probably" removed.

Line 440 - Ryczik should be spelled Rybczyk.

changed.

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

changed.

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REVIEWER 2.

We thank Reviewer 2 for the helpful comments provided on this manuscript. Below we have put Reviewer comments in bold text and our responses in italicized text.

The paper reports data on 'blue' carbon stocks and 'blue' carbon accumulation rates from seven salt marshes at the west coast of Vancouver Island, BC, Canada. These seven salt marshes cover a total area of 47 ha. The authors differentiated between high and low marsh through identification of indicator plant species. They took in total 34 cores of the organic (peat) layer down to the underlying sand or gravel bed. In 10 cases the cores did not penetrate into the sand, clay or gravel layer. Eight cores (four from high and four from low marsh) were dated using 210Pb. The authors found an average total C stock to the base of the peat layer of 67 ± 9 Mg C ha-1 (mean ± SE) for all cores, which was less than one third of the globally averaged estimate of 250 Mg C ha-1 for salt marsh C stocks. In contrast, the average base of peat carbon accumulation rate (CAR) was 184 ± 50 g C m-2 yr-1, and in the high marsh even on average 303 ± 45 g C m-2 yr-1, which was about five times higher than in the low marsh areas. It has to be noted, though, that these CARs were based on four dated columns only per low and high marsh. In the discussion part, the authors put their findings into perspective of data from other salt marsh ecosystems along the Pacific and Atlantic coast of North America, and claim that they have addressed the knowledge gap regarding the carbon accumulation potential of these ecosystems. Finally, the compare the carbon accumulation of their salt marsh system with that of Canadian boreal forest and conclude that the carbon accrual rates are much higher in the salt marsh, but acknowledge that the salt marshes cover approximately only 0.016-0.1% of the area covered by boreal forests in Canada and that their absolute magnitude of carbon accumulation is only minor.

While the work appears to have been conducted in a scientifically sound way, and also the data have been well evaluated and compared with the literature, the representativeness of the studied system remains vague. The authors have taken 34 peat deposit cores in a range of about 25 km in an area that is subject to negative relative sea level rise due to uplift of this part of the coast. They attribute the below average carbon stocks of their salt marshes, amongst other reasons, to this relative sea level drop. But they do not detail to which extent this is representative or not for the vast Pacific coastline of North America, not to speak of the Atlantic coast. Also the effect of tidal range (the be more precise, its local differences) on the partitioning of marshes in 'high' and 'low' marshes, which obviously has a large effect on carbon accumulation rates and total C stocks, has not been addressed. That means, the authors did not put their 'mesotidal' system into perspective of other parts of the NE Pacific coast.

They only wrote "We expect that these mesotidal estuarine marshes, often constrained in size by surrounding topography, are typical of the marshes found on the Pacific coast of

British Columbia" (L510-511). How is the situation in systems with higher or lower tidal range than the one encountered here? In a nutshell, the authors should address the open question regarding the representativeness of their system in more detail.

Regarding the first point, the reviewer is correct that, within the financial constraints of the project, we have sampled 34 peat deposits from a marsh area of 47 ha within the Clayoquot Sound UNESCO Biosphere Reserve and Pacific Rim National Park Reserve and have been able to provide ^{210}Pb dating for 8 (4 high marsh, 4 low marsh) cores. While this sampling may sound limited, it provides a substantial expansion over what has been sampled previously and adds the first ^{210}Pb dated materials for the area. While some studies have provided a larger number of sampled and dated cores (e.g. Callaway et al. 2012; Suir et al. 2019; Brown, 2021), the assessment of carbon accumulation rates in many other marsh blue carbon studies are often completely lacking in ^{210}Pb-dated quantification, particularly of high and low marsh environments separately (see e.g. supplemental information from Ouyang and Lee 2014 compilation). Therefore, this work represents a step forward in quantifying salt marsh processes, particularly on a previously under-sampled coastline.

That said, we appreciate the Reviewer's concern that we clarify our assumptions regarding our study's representativeness for the Pacific Coast of Canada and have addressed this concern in the following ways:

(1) In our discussion of the impacts of sea level rise on vertical accretion rates and carbon accumulation rates (Ln 471-473), we provide a better estimate of sea level change along the Pacific Coast of Canada, which averages -0.76 ±1.32 mm/y (James et al. 2014).
(2) Regarding the second point, we have included in the methods that we were not able to measure the precise tidal range at each location and note that the average tidal range in nearby Tofino is ~2.7 m. (Ln 104-105). (We do note, however, that recent global syntheses (Wang et al. 2020) have suggested that tidal range variables were not a significant driver of CAR in tidal wetlands.)
(3) Additionally, within this system, the partitioning between high and low marsh zones (defined previously by tidal range) appears very closely linked with associated vegetation (which is seen in our canonical correspondence analysis). We have noted this in Ln 113-114. We have also added the following sentence to the end of the paragraph describing our marsh zonation methodology (Ln 126-130):

> *"This tight coupling between vegetation type and marsh zone has also been observed in other studies on the west coast of Vancouver Island (Deur et al. 2000) and studies in nearby Boundary Bay (Gailis et al. 2021). Vegetation associations related to salinity and inundation are well documented and commonly used in salt marsh delineation (MacKenzie and Moran. 2004)."*

(4) Importantly, we do not make any claim that these sites are at all representative of the Atlantic coast, and in fact, intended to make the point that the Pacific Coast environment

> *is substantially different from the geomorphological and depositional environments along the passive margin of the Atlantic coast, and that formal estimates of C sequestration require better documentation of C stocks, accumulation rates, and areas. We have (hopefully) addressed this concern in our rephrasing of our estimate of pan-Canadian carbon accumulation rates (Section 4.4, Ln 550-594), where we have (a) recalculated the Canada-wide carbon accumulation using an Atlantic Canada estimate and the global average CAR estimate from the IPCC, and (b) explicitly stated that a pan-Canadian CAR estimate (and associated uncertainty) should be a subject of future research.*

L136-137: How long were the cores stored under refrigeration?

Cores were collected in June and September, 2016. While in the field, cores were stored in portable coolers with ice packs. Long-term storage of cores was at Parks Canada laboratory in Vancouver, BC at 4C. This information has been added to the Methods (LN 155-156)

L207: How representative were these eight dated cores for your whole system (four for the high marsh, and four for the low marsh)?

As described above, we used the species composition of marsh vegetation communities to determine low and high marsh designations, by examining vegetation found within 50x50cm quadrats. We used these vegetation communities to establish representativeness of the high and low marsh sites that we sampled for ^{210}Pb analysis. We have clarified this point in the methods (ln 205-206)

L 485-488: Here you write "Our Clayoquot Sound data represent only a small area of a single region of the west coast, but if we assume our CAR estimate of 184 ± 50 g C m-2 yr-1 from Clayoquot Sound approximates the average for all tidal salt marshes in Canada,…". But the question is whether this generalization of your findings is justified. And if so, on what basis / with what assumptions?

We agree completely with the reviewer that our calculation is a vast oversimplification (although not unlike oversimplifications that have recently been published in other blue carbon literature). Our purpose in this text was not to provide a definitive estimate of blue carbon accumulation rates in Canadian salt marshes, but to provide a "back-of-the-envelope" comparison of the scale of accumulation relative to other ecosystems. We have revised this section to make the speculative nature of our calculation clearer, and to emphasize what work needs to be done to make this estimate more robust. (see revised Section 4.4).

(We note, however, that the CAR used in our calculation does fall within the range of expected values for worldwide and NW Atlantic CAR estimates of Ouyang and Lee (2014). However, in our revision we expand this analysis to encompass a wider range of possible CARs.)

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