

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 #1

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The article submitted by Chastain and al aims to estimate carbon stocks and accumulation rates in salt marshes of the Pacific coast of Canada. The argument is the capacity of tidal salt marshes to sequester C. However there are still a limited number of marshes for which carbon accumulation have been estimated. In this work, the authors present an new investigation of salt marshes of the Pacific Canadian coast. The paper address a relevant scientific questions within the scope of BG and present new data. But I have a major problem with the estimate of the mass accumulation rate. There is no details on ^{210}Pb data and not enough on the dating method. Indeed to estimate CAR, it is necessary to estimate carbon but also sediment accumulation rates. It is classical to use ^{210}Pb for dating sediments over the last decades. Measurements by gamma spectrometry permit to determine also ^{226}Ra , the supported ^{210}Pb , and

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^{137}Cs , an independent time marker. Here the authors use another technique, alpha spectrometry. The problem of this method is that it measures ^{210}Pb only. But the ^{210}Pb -dating method is based on the decay of the excess Pb, i.e. the fraction of ^{210}Pb not supported by its radioactive parent (^{226}Ra) in sediment. This implies the authors made assumptions to estimate this supported fraction, this information is not given in the article. How do the authors determine this supported value? Did they use the same value for the 4 cores. What is the error associated with the assumption? In addition, $^{210}\text{Pb}/^{210}\text{Pb}_{\text{ex}}$ are not presented which is a critical aspect as these data define the SAR. The method used to estimate SAR/MAR is also not enough detailed. From the CSR (constant rate of supply) model based on the inventories, it is possible to calculate directly age of each layer, and then to estimate SAR and MAR, such values would have been interesting to discuss also (temporal trends, potential change in accretion regarding sea level rise). I do not understand why the authors speak two times about ^{137}Cs , there is useless. In fact in such environments, where accretions could have change, ^{210}Pb IN EXCESS is indeed appropriate, ^{137}Cs is interesting only to validate the chronology.

The second problem is the sampling. The authors explain "Within each marsh, sediment cores were extracted along linear transects perpendicular to the low tide shoreline following the methodology of Howard et al. (2014). Coring spots were approximately evenly spaced along the transect" (between nine and 24 meters apart) from land to sea and spanned the low and high marsh zones". On the other hand, the authors determine SAR only on 4 cores sampled in different systems. Do they assume there is no change in sediment according to the position along the transects? What about the morphology along these transects? Regarding the purpose of the article, I would have expected to have a higher number of cores on which ^{210}Pb was determined in order to obtain more reliable SAR and then CAR. Whereas ^{210}Pb is already mentioned in the abstract, there is no data of this radionuclides nor figures presenting profiles with depth. Considering the objectives of the article, that imply to know rather precisely SAR/MAR in order to calculate CAR, the number of dated cores is also too weak to be

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representative of the different systems. I do not recommend publication of this work.

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