

## ***Interactive comment on “Blue carbon stocks in Baltic Sea eelgrass (*Zostera marina*) meadows” by Maria Emilia Röhr et al.***

### **Anonymous Referee #2**

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This study has collected a range of relevant data with which to provide estimates of organic carbon storage in Finland and Denmark. The data set would certainly be a useful addition to the growing literature of carbon storage in seagrass meadows and uniquely provides an indication of the range of carbon stocks observed for a single species of seagrass in different regions. The manuscript would require a major revision before being suitable for publication. The description of methods and presentation of results are confusing in terms of context values and units. Many are small points but collectively it becomes very frustrating to try and discern what has been done. The discussion needs to be kept focussed and relevant.

### Specific comments

Study area: Line 126 the reference of Kristensen and Andersen 1987 seems at odds to

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the preceding sentences as it is a methodological paper that uses different mixtures of sand/eelgrass and shell to examine methods of analysis. A better reference would be Leipe T et al., 2011 Geo-Mar Lett 31, 175-188 who have analysed and obtained low PIC The inorganic content analysis on the samples could then be used to test that this is a common observation. In testing carbonate content, 10 samples were taken from each region, so was this from each station i.e. full coverage of sites? What are the units, wt%C or CaCO<sub>3</sub>? A line stating that that TC was therefore taken to represent OC is needed. Can the study sites be identified that made up the gradients of sheltered to exposed (see that on line 317 sorting is used to identify).

Field sampling: 1 cores taken to 40cm, why were C stocks only estimated to 25cm? Replicate cores taken at 5m for sediment but 15m apart for biomass? Clarify.

Seagrass and sediment variables: In determining the stable isotopic composition of above and below ground biomass and sediments it seems odd to use the term particulate organic carbon, just OC would be a better description. The method of d<sup>13</sup>C analysis is given but an estimation of sample analytical error for biomass and sediment is needed. It is unclear whether all OC data is derived from the mass spectrometric determination of d<sup>13</sup>C. Again error on sample analysis should be provided. It is confusing to know that the sediment has been analysed for OM and OC content and yet the OM content is not discussed only presented in Table 1. OC instead of OM should be presented in table 1. C stocks and accumulation: C stocks “obtained by depth integration of the POC mg C cm<sup>-3</sup> of 0-25cm sediment layers would give units of mg C cm<sup>-2</sup>. which if multiplied by the areal extent of seagrass would give units of mg C, not g Corg m<sup>-2</sup> as reported. Suddenly the notation has changed to Corg, need to be consistent. The areal extent of Danish seagrass is given but not the Finnish (30km<sup>2</sup>?). Economic loss would be better presented as loss in stock (pool) and rate of addition as separate values. Is the economic loss in the last 100 years is based on change areal extent times change in sequestration rate and biomass, but presumably no change in sediment C pool?

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Results: If the C content of the biomass is known then why is the DW data used to determine the Corg pool in the living seagrass? It would be informative to have the dry bulk density data in Table 1 (rather than silt content) as it is not presented elsewhere. Given that OC data for the sediments is available, why are the OM results presented in the text? Especially as the OC stock section now presents the data as g C cm<sup>-3</sup>. Need to rationalise terms as it is confusing to use the same term Corg stocks (g C cm<sup>-3</sup>) in line 324 and (g C m<sup>-2</sup>) in line 329 and table 2. g C cm<sup>-3</sup> (carbon density). The Corg accumulation section includes the sediment trap data. It is presented as gDW, not in OC and the relevance or significance of the measurements are not obvious. They would be better omitted. The areas represented by Finland, Limfjorden and Funen seagrass should be provided. In the isotope section the sediment and source data should be presented before the results of the mixing model. Allochthonous carbon is said to be a major contributor to the sediment (line 373), but no isotope value is given for its source term and it is not included as a component of Fig 5. It is not clear what it represents and is an input of terrestrial organic matter included or are there no significant riverine inputs to the area? Given the range of values for the sources it is difficult to understand how *P. pectinatus* and *R. maritima* can be isotopically distinguished from *Z. marina* in the mixing model for Finland

Discussion: Suggest discussion of sed trap data is removed as it adds little to the discussion. Analysis would be improved if it would highlight the differences in stock/accumulation, sources and particular influences (table 4) between Finland and Denmark given the difference in environmental factors. Overall the discussion could be shortened as there is a lot of extraneous text (more focussed) and consistent. One example is from the C accumulation and stock. Why not just compare with latest and most complete C stock assessment of Fourqurean et al., 2012 that has superseded all the other estimates. Are data for *Z. marina* included in the Fourqurean data set, if so, a comparison with this data would be more relevant. Table 3 C stock 627 g m<sup>-2</sup>, line 520 its 6900 t km<sup>-2</sup>, maths is wrong here? I do not see the relevance of comparing with tropical mangroves and marshes. In the comparison with stocks from Australia and

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Asia this would only be appropriate if the stocks were all calculated in the top 25cms. Why are different units used for comparisons km<sup>2</sup>, m<sup>2</sup> and ha<sup>-1</sup>. The discussion of cost of seagrass loss contains a lot of speculation and should be reduced. How viable is it to compare Danish stocks with those in Sweden and Germany? Line 549 onwards Does this mean that it was assumed that all the carbon was lost from 25cm of sediment in the calculation? What was the basis for this assumption? Why is the loss being discussed now 50 years when it was 100 years in the results.

Further minor comments Line 290 291 state “proportion” but have reported %. When reporting mean values the error associated with the mean should also be reported. Line 291, 292 give mean %OC of biomass (that is used to determine C content ha<sup>-1</sup>) Line 293 change to C units. Line 306 The silt data is presented in Figure 2 and Table 1 (sorting data in Figure 2 but not referenced in the text). Line 326 In Figure 3 the max scale for Finland OC density is 20 and, but text reports value of 22 mg C cm<sup>-3</sup>, which must represent individual sediment values, it would be better if means for the site were reported in the text. Line 333 Finland stock ranged from – but only a single value given. Line 335 The min C stock for Denmark is different in the text to that given in Table 2. Line 356 should be annual areal Corg Line 373 allochthonous Line 383 were not where Line 473 does this data relate to drifting algal mats? Figure 1 Label a,b and c, which sites are shown? Figure 3 axis and legend is wrongly labelled as sediment profiles of particulate organic carbon content. This would have units of wt% or gC g<sup>-1</sup>. Figure 4 the legend “organic carbon stocks (Corg, g C m<sup>-2</sup>) “is confusing. Fig 5 It would be easier to discern the different source if patterns rather than shading were used. Table 2 column 5 should be Mt

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