Röhr et al. rebuttal letter Oct 9 2016

Dear Editor, We have now addressed the minor comments by the referee and hope now that the ms can be published. Looking forward to your respons,

Sincerely,

Emilia Röhr

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

The authors have addressed many of the issues raised during the first reviews, but a number of issues remain problematic :

1. The fact that samples were not acidified prior to analysis remains not well discussed. In the original version, the authors stated that "The inorganic carbon content in our samples was low (0.003 to 0.3 %DW, n= 10 per region) and therefore carbonates were not removed from the sediment samples prior to the analysis". Given that %OC varies from 0.1 to 5.78 % (average values) for the different sites, this implies that inorganic carbon can contribute at least 5.2 % of the total carbon (assuming the highest %IC was found in the sample with the highest %OC), but it could be more if %IC and %OC are not related. Hence, I feel the data should be presented so that readers can make their own judgement on how problematic this is. Assuming the carbonate fraction has a d13C of around 0 per mil and the organic fraction somewhere between -15 and -20 per mil, a 5% contribution of carbonate to in the measured sample can correspond to a bias of about 1 per mil, which is large. This should at least be acknowledged and discussed.

Answer: We understand the referees concern. The %IC varied between 0.3-5.6% (average 3.3%) and was on average less than 0.76‰ (range 0.16-1.17‰) which is less or similar to the natural variance of the sediment isotopic signal. As we have addressed in our previous reply, it is not trivial to acidify the samples, in particular in low organic content samples (such as those sampled from Finland) where acidification itself can cause similar analytical errors than the procedure would be able to eliminate (Schlacher and Connolly 2014). To further clarify this issue, we have now added a sentence to the ms (l. 129-132) in which we have acknowledged this possible bias.

2. Line 334 and further in the revised version: this still does not make sense. The total organic C stock is measured and should be derived from your bulk density and %OC data, the OC accumulation rates are irrelevant here. The same applies to the section on line 223 and further in the revised version (see also my original comments). The same goes for their reply to my original comment (12) that you use a rate to calculate the OC stock.

Answer: The Corg stocks are calculated according to Lavery et al. (2013) by multiplying the OC (OC mg/gDW) measured from different sections of the sediment core with the sediment dry bulk density (g/cm^3), the product of this calculation gives units in OC mgC/cm^3. The product is further multiplied with the depth of the measured sediment section, and all the products from the

different sections of the sediment core were summed (depth integrated Corg stocks). The product of this summing was further multiplied with 10 in order to get the units into C g DW/ m^2. We have now added a section to the ms where this procedure is explained in more detail (l. 213-216).

From line 342 → our goal is to estimate how much carbon has been lost from the Danish eelgrass meadows due to reduced eelgrass area in the last 100 years. For that calculation OC accumulation rate is actually relevant. In order to address the lost carbon stocks, we need to have a number for both annual sediment accumulation rate (we have used 3 different rates, minimum, medium and maximum, obtained from literature) as well as an estimate of rate on how long it has taken to accumulate the average Corg stock of the area in the top 25 cm of sediment (eg. depth integrated Corg stocks in top 25 cm in Finland: 627 C gDW/m^2) with the sedimentation rate of 2.02 mm/y (the medium rate), the product of this calculation is referred to as annual areal carbon accumulation (t C ha^{-1} y^{-1}) and the procedure is explained in detail in the ms in lines 231-237.

3. My original comment concerning the relationship observed between %OC and bulk density; the authors mention they don't understand this comment and mention their data show exactly this. Yes, they do, but the point is that this is not a causal relationship but a logical inverse relationship reflecting different contributions of organic matter (low density) and the mineral fraction (which has a high density). Thus, statements that %OC is determined partially by density are not appropriate.

Answer: We do understand the referees concern on this subject, however, we do not find such a section in our manuscript where our text would imply that % OC would be partially determined by sediment density, but rather that according to the DistLm model sediment dry density was an important predictor variable for variation in the Corg stocks. We understand that high organic content sediments naturally have lower densities and thus, the results from the DistLm model can be logically explained by the inverse relationship of sediment dry density and the mineral fraction.

4. Earlier comment regarding extremely low OC contents (<0.001 mg cm-3), I still do not see these low numbers on Figure 3. These are incredibly low.

Answer: As stated in our previous reply, the number <0.001 stands for those sites that had replicates in which the OC was below detection limit of the elemental analyzer (%OC < 0.001% DW). Therefore, no number could be plotted in the figure for those replicates and hence, cannot be seen in Fig. 3. We have added a sentence to the figure legend stating that numbers below detection limit are not shown in the Figure. The values plotted in the Figure are averages of C (mg C cm-3) from each site presented with standard error of mean.

Minor comments:

5. Table 1: use "." as the decimal separator throughout, not comma's.
Answer: Corrected.

6. Table 1: use 1 decimal only to report d13C data given the analytical uncertainty.
Answer: Corrected.