Response to reviewer 1

The authors present a paper describing variation in sedimentation and organic carbon accumulation between levee and basin position and along a gradient of degradation in a tidal marsh of the Chesapeake Bay ecosystem. The authors collected data from eight study sites representing levee or basin geomorphic positions and in different plant communities from the least degraded to most degraded portion of the marsh with degradation being caused by sea level rise and eventual reclamation of these wetlands by the bay. Degradation is determined by the ratio of vegetated to non-vegetated area. The authors conclude there are substantial differences in sediment deposition and carbon accumulation between levee and basin position and more modest differences along the degradation gradient. Overall, the paper is well written, clear, and easy to follow although I have some concerns I would like to see addressed.

We would like to thank the reviewer for the very thorough review of our paper. We have tried to implement your comments to the best of our abilities. In particular we (1) replaced the initial statistics with linear mixed effects models, as suggested, and (2) gave more detail on the sampling methods, such as how sites were selected, how sediment samples were cored and how replicate samples were defined. Many other smaller text changes were included in response to your feedback as detailed below.

The original feedback is denoted in black with numbered headers (1.1 for the first comment, R1.1 for the response on the first comment), followed by our response in blue. The textual changes made in the manuscript are indicated below the response (new pieces of text in blue and removed text in red).

1.1 The authors should emphasize what is novel about the work they have conducted. Much of the paper summarizes basic physical geography of tidal wetlands. That levee positions receive more sediment is not novel – in fact, its why they are levees. Similarly, that they receive more OCAR input is simply because they receive more sediment. Plant community differences similarly are not novel – the zonation of tidal wetland communities associated with deposition and salinity are well understood. What seems most interesting is the degradation aspect of the study and how these systems change with sea-level rise. I think this theme could better come forward in the paper overall.

R1.1 *This is an interesting remark. While we do agree that the geomorphic differences between levees and basins are well known and described in literature, the difference in organic carbon accumulation rate (OCAR) between these locations is very poorly described and explained in literature. In particular, we identify that much stronger compaction of the sediment bed on levees versus basins (identified from much higher sediment dry bulk density, and explained in the discussion as a likely result of better sediment pore water drainage and pore collapse on levees) creates more accommodation space for sediment and organic carbon accumulation, as such contributing to the 4-fold higher OCAR on levees versus basins. This finding is particularly novel. Further, we agree with the reviewer that the degradation aspect of the study is novel.*

Therefore we aimed to emphasise both the degradation aspect and the levee-basin aspect of the study, which are both novel. We have altered the text in the introduction to highlight this more:

Line 110-113: "While the geomorphic differences between levees and basins are well known, it remains understudied to what extent the rate of organic carbon accumulation differs between both, and which processes contribute to this difference. ~~It can be expected that these differences between basins and levees can also lead to differences in OCAR~~. ~~However~~Moreover, there are currently no studies that have investigated the dynamics of OCAR along levee-basin gradients in marsh zones with a different degree of marsh degradation in response to sea level rise, which hampers our ability to predict the long-term stability of carbon in these systems as they progressively degrade in response to sea level rise."

1.2 I am somewhat confused by the design of the sampling. As written in the text, the sampling appears quite limited. Table 1 and the text around 145-155 suggests 8 sampling sites and 4 soil cores per site. This would make n=32. However, the figures showing data points such as Figs. 2 and 6 suggest many, many more data points. The sampling needs to be clarified. Moreover, please describe how the sampling within a site is independent. Were samples collected along multiple transects? Minimum distance between soil cores? Overall how the soils were collected needs to be better described.

R1.2 *We have changed the explanation on the sampling design to make it clear.*

Line 164-174: "The selection of study sites resulted in eight sampling locations (Table 1), two in the most degraded zone and 3 in the intermediately and least degraded zone. At each sampling location, four replicate soil cores were sampled approximately one meter apart. Three replicates were used for organic carbon analysis (see 2.3.1 and 2.3.4) and one was used for radiometric dating to determine the sediment accretion rate (see 2.3.2). As a result, of the total of 32 cores, 8 were used for radiometric dating and the remaining 24 for organic carbon analysis. Every core was between 25 and 50 cm long and was sliced in increments of about 1 cm. For the organic carbon analysis every other depth interval was used, leading to 12 to 25 data points for each core and a total number of 329 data points. ~~This resulted in a total of 8 sampling locations (Table 1). At each sampling location four replicate cores were taken, of which one was used for radiometric dating (refer to section 2.3.2) and three were used for bulk density and organic carbon analysis.~~"

In the initial figure 2 and figure 6 we show all measured depth intervals of every sampled core, which indeed results in more datapoints than sampled cores. Based on this comment and that of reviewer 2, we have changed our figures to show both the average value and standard deviation of each core:

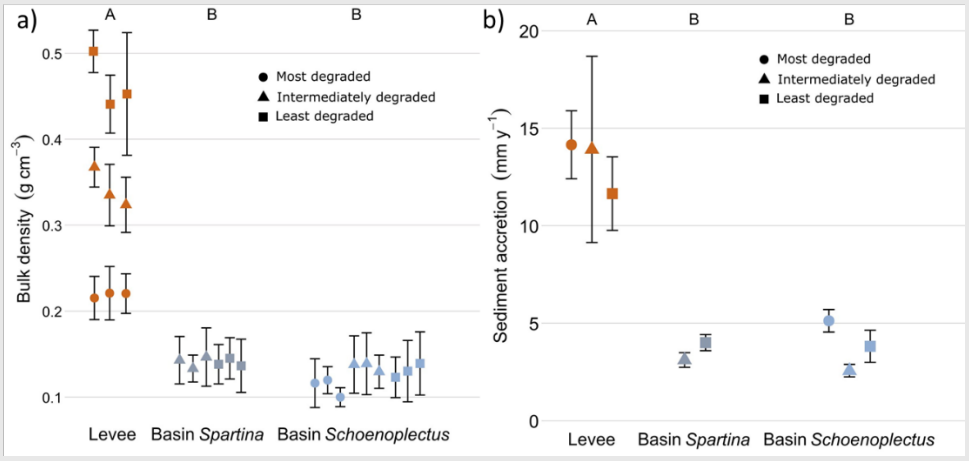

Figure 1: Dry bulk density (left) and sediment accretion rates (right) determined with radiometric dating along the levee basin gradient. The coloured points indicate the average value for all depth measurements of each core and the error bars show the standard deviation for all depth measurements of each core. The different shapes indicate data from the least degraded (square), intermediately degraded (triangle) and most degraded (circle) zone. The letters above indicate the significance of the differences between levee, basin *Spartina* and basin *Schoenoplectus*, where observations with the same letters are not significantly different from each other (derived from ANOVA for the sediment accretion and from linear mixed models for bulk density).

and

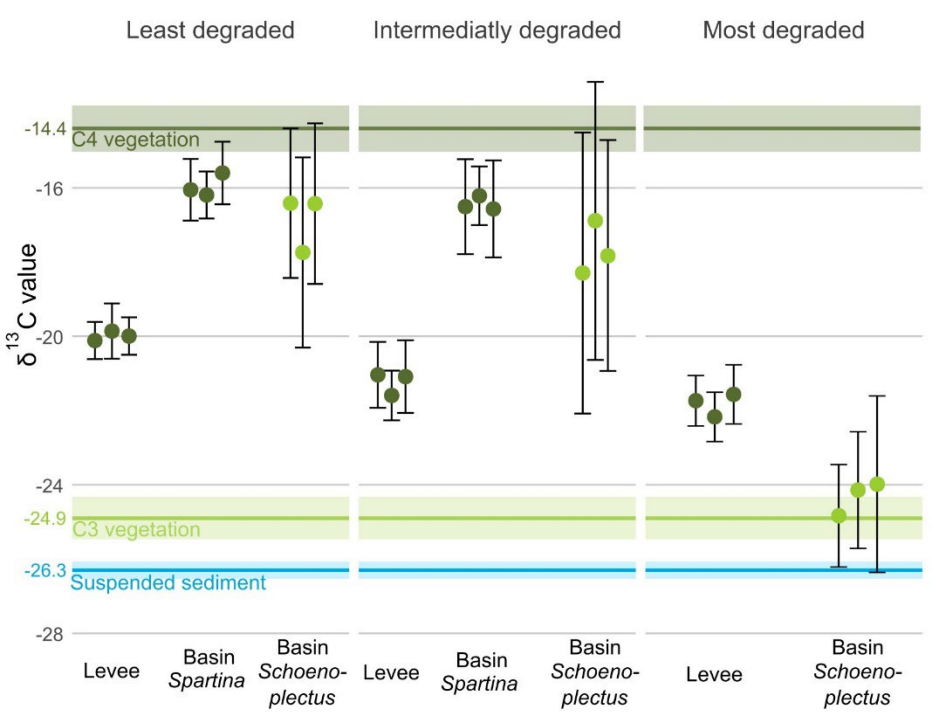

Figure 6: δ13C values along the degradation and levee-basin gradient. The coloured points indicate the average value for all depth measurements of each core and the error bars show the standard deviation for all depth measurements of each core. The colours of the ~~boxplots~~ points correspond to the photosynthetic pathway of the dominant vegetation (light green for C3, dark green for C4). The horizontal coloured lines correspond with the δ13C values of C3 vegetation (light green), C4 vegetation (dark

green) and suspended sediment (blue). The lighter-coloured area around the lines correspond to the 95% confidence interval of the δ13C values.

1.3 Similarly, I am concerned about the sampling of the gradient in degradation. The gradient is described/quantified as the ratio of vegetated to non-vegetated surface and while this ratio is reported in table 1, the sampling as I understand it is somewhat misleading since only the vegetated portions of the marsh were sampled, regardless of gradient position. Clearly the vegetated and non-vegetated portions of the marsh would experience differences in OCAR input so the decision to only sample vegetated – i.e., least degraded regardless of the degradation gradient needs to be justified and the implication of this choice clearly described.

R1.3 *We did in fact try to sample the unvegetated shallow ponds within the marsh as well, but this turned out to be unsuccessful. The sediment at the bottom of the unvegetated ponds was very loose, unconsolidated, fluid mud, making it impossible to sample solid sediment cores using the same method as for the cores sampled from the vegetated marsh portions (using coring tubes), where the sediment was consolidated and bound together by roots. However, the remaining vegetated marsh portions are also very different along this gradient of UVVR. In the most degraded zone the vegetated marsh portions have sediment beds that are much less strong (clearly noticeable as our feet could sink up to several dm into the sediment bed during the field work) as compared to the least degraded zone (where we did not sink much into the sediment bed). We added an explanation in the methods section to explain why only the vegetated parts were sampled.*

Line 154-15: **"**Degraded zones consist of a mosaic of vegetated marsh portions and large pools of open water, the latter having sediment beds consisting of fluid mud where sampling fixed sediment volumes was not feasible. Therefore, we sampled only vegetated marsh sediment beds in each zone. Within each zone, samples were collected on levee and on vegetated basin locations."

1.4 The description of the statistical analysis is too limited for the statistical procedure to be evaluated. Please expand the analysis section to indicate if fixed or mixed models were used and any random effects, any data transformations, selection of post-hoc tests (the results of which are show in the figures).

R1.4 *You are indeed correct. We changed our statistical analysis to linear mixed models, where core is used as a random effect combined with a Tukey post-hoc test to see the differences between all the sites and locations. We have changed the section on the statistical analysis as follows:*

Line 242-250: "For sediment accretion rates ~~and bulk density,~~ the difference between levee and basin locations with *Schoenoplectus* and *Spartina* was investigated using ANOVA in R version 4.4.1 (R Core Team, 2022). For the organic carbon content, density and accumulation rate, the separate effects of degradation zone and location (basin or levee) were investigated using linear mixed effects models, including core and depth as random factors, using the lme4 package (Bates et al., 2015). Besides the simple

effect of location and degradation zone, we ran an additional model with their interaction effect. To see which locations and zones differed from each other, a Tukey post-hoc test was done using the emmeans package in R (Lenth, 2025). Bulk density was analysed in a similar way, but only looking at the difference between levee and basin locations. ~~, the average value of each core was used to minimize the effect of the depth profiles. The difference between levee and basin locations and zones with a different degree of marsh degradation were also investigated using ANOVA."~~

1.5 I find several inconsistencies in the arguments surrounding the differences between the levee and basin communities. Line 380 suggests that high accretion rates in levees may be due to rapid burial of organic matter and low O2 availability leading to lower decomposition. However, on line 339, there is the suggestion that deep-soil pore water drainage on levees promotes oxygenation and more rapid plant growth. While perhaps these can both be true depending on the precise depth of anoxia, its reads as inconsistent. Similarly, on line 394, the packing of high-density mineral matter on the levees is used as a justification for the greater bulk density of levee soils would further argue against rapid drainage and oxygenation.

R1.5 *Thank you for bringing this to our attention. We understand that the processes we're describing can be seen as inconsistent, but it is indeed the depth of the anoxia that plays a role. Since the accretion rates are high on the marsh levees, we argue that this will result in faster burial of the present carbon to layers below the oxic zone, even though this oxic zone is deeper on the levees compared to the basins. We have highlighted this in the text as follows:*

Line 432-437: "Even though it may be expected that sediment pore water drainage is deeper in levees (Armstrong et al., 1985; Balling & Resh, 1983; Mendelssohn & Seneca, 1980; Ursino et al., 2004; Van Putte et al., 2020), t~~T~~he observed higher accretion rate on the levees results in faster burial of the carbon ~~present in the profile~~, so that it may faster reach layers below the sediment drainage level, where oxygen is less available. This could imply lower rates of decomposition and thus better preservation of the present carbon ~~which may imply less oxygen availability to the carbon and thus lower decomposition~~ (Rietl et al., 2021)."

1.6 I have concerns with the interpretation of the 13C data as presented here. The sediment varies considerably in 13C suggesting different sources of OCAR input as the authors indicate. However, the endpoints of the carbon is somewhat ambiguous. The argument is made that 13C can determine the difference between autochthonous C and allochthonous C. However, autochthonous C can come from two sources – the C4 grass *Spartina* and C3 rush *Schoenoplectus* while allochthonous C is assumed to be C3 (presumably phytoplankton and other algae). Therefore, while seeing a highly C4 signature in sediment is good indication of local C in a *Spartina* zone the opposite is not necessarily true since the deposition could be from OCAR input from outside the wetland as well as OCAR input from remobilized sediment with a local source. Please address this concern in interpreting these data. Figure 6 I think shows the community shift happening with the *Schoenoplectus* OCAR being mostly C4-derived in the least degraded and intermediate sites and mostly C3 derived in the most degraded. Since this is a C3 plant,

the data suggest a recent conversion (and the large error bars support this) in the least and intermediate sites but a long-term history of the C3 rush in the most degraded. Combined with the assertion that basins are sediment starved, the data argue for local carbon inputs dominating the basin system.

R1.6 *This is a very valid suggestion. We have incorporated it in the manuscript.*

Line 419-426: "…This pattern in sediment deposition is confirmed by the $\delta^{13}C$ value of the levee sediments (Fig. 6), where the average value (-21.0‰) indicates a mixture of different sources of carbon, from local C4 vegetation (-14.4‰) and incoming suspended sediment (-26.3‰). The basins under C4 vegetation in the least and intermediately degraded zones, however, have a $\delta^{13}C$ value of (-16.2‰) that is relatively close to that of the vegetation (-14.4‰). Hence for these basin locations we can conclude the sediment organic carbon mainly originates from autochthonous input by the local C4 vegetation. For the basin under C3 vegetation, i.e. in the most degraded zone, we cannot be sure whether the soil organic carbon is mainly from autochthonous origin, since the $\delta^{13}C$ signature of the local C3 vegetation is close to that of allochthonous suspended sediment. …"

As for figure 6 we agree that a vegetation shift may have happened from Spartina dominated to Schoenoplectus dominated, however this discussion is speculative and does not provide an added value to the focus of this paper on carbon accumulation (since we observed no significant differences in sediment properties nor organic carbon content/density/accumulation rate between basin sites with Spartina or Schoenoplectus), so we decided to leave it out of this paper.