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Biogeosciences Discussions

Interactive comment on "Geomorphic influences on the contribution of vegetation to soil C accumulation and accretion in *Spartina alterniflora* marshes" *by* Tracy Elsey-Quirk and Viktoria Unger

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REVIEWER #2 The authors conducted a study to investigate the impact of environmental conditions across marshes on biomass, belowground production, sediment accretion, organic/mineral accumulation. The scientific questions addressed by the ms fall within the scope of BG. The authors examined different belowground processes, and related them to each other and biogeochemical processes. The study will present some interesting results for the studies of saltmarsh sediment acrretion and carbon sequestration after careful revision. General comments This study used many data from paralleled studies, such as Unger et al., (2016) and Boyd et al., 2017. To avoid

confusion, you need to clearly show which data come from paralleled studies.

As described above, we have extensively revised this manuscript. We clarified the inclusion of C accumulation rates published in Unger et al. 2016 and Cs-137 based accretion rates published in Boyd et al. 2017 for our examination of how vegetation dynamics relate to soil C dynamics. The text was added to the Introduction (L 78 – L 99) and Methods (L 155 – L 157).

Data analyses need to be checked and refined. Tidal range and mean water level are calculated from mean low water and mean high water, organic/mineral accumulation rate is calculated from sediment accretion rate. You cannot do correlation or regression analysis between the variables and those variables they are calculated from.

We conducted a correlation analysis to examine which variables (mean low water and/or mean high water) was driving variability in tidal range. While statistically, tidal range was calculated from the difference of MHW and MLW, our analysis revealed that spatial variation in tidal range was driven by differences in average high water not average low water. This illustrates how marsh interiors do not drain much at low tide and any difference in tidal range across marshes is due to high tide levels. We maintain that although this is not a main focus of the paper, it is still important.

Surface accretion and accumulation rates were removed in the revision.

The significant difference should be labelled alphabet-sequentially.

I am not exactly sure what this comment is referring to.

Specific comments

Abstract Line 7: add of after rates.

This line of the abstract was changed in the revision.

Line 14: add permil after 7-40.

Some journals show salinity is unitless; I will defer to the Editor for recommendation.

Introduction Line 29-31: you need to add references to support your statement, such as Ouyang et al. (2017).

This specific line was modified in the revised paper to: "Plant biomass, especially belowground biomass, is considered to be a primary contributor to soil organic matter and carbon (C) sequestration in marshes (DeLaune et al. 1983; Nyman et al. 2006)." References were added, as the reviewer suggested.

Line 59-61: I suggest you add some references here, such as Haslett et al. (2003).

We appreciate the reference recommendation and added it to the sentence, which is now L 49 – 51 in the revised manuscript.

Line 66: add of after range

This sentence was removed in the revision.

Line 99-100: The allochthonous source of labile C may also include C input from riverine sources where marshes are near rivers or delta. see Craft (2007)

This specific sentence was removed in the revision, however, two sentences in the revised manuscript, L 64 – 67, "Higher tidal range, greater supply of mineral nutrients and sediments, and lower salinities are conditions that are all predicted to enhance both plant productivity and soil C accumulation (Mendelssohn and Kuhn 2003; Craft 2007; Kirwan and Guntespergen 2010)." And L 72 – 74, "In contrast, marshes in geomorphic settings with high rates of mineral sedimentation such as those near river deltas may have greater magnitudes of both allocthonous C deposition and autochthonous plant C inputs (e.g., Craft et al. 2007)." include the Craft et al. 2007 citation. Again, we appreciate the reference recommendation.

2.Methods

Line 162: remove the after each.

The correction was made as the reviewer suggested.

Line 184: what's the diameter of coarse roots and rhizomes used in your study?

We did not measure the diameter of coarse roots and rhizomes.

Line 196-204: from your results, I understand you quantified belowground biomass to both 50cm depth and the maximum of Cs-137 profile. Please clarify this point clearly here.

As the reviewer suggested, we added the methods for calculating belowground biomass to both the 137Cs-peak depth and 50-cm depth.

Line 214: Some mineral material may be lost during from high temperatures of LOI analysis. Have you done acid treatment to remove inorganic carbon before LOI analysis?

These data were removed in from the revised manuscript. As the reviewers generally alluded to, there was a lot of data and analysis presented in the original manuscript, and we decided to simplify the scope and just concentrate on longer-term accretion and accumulation rates.

Line 207: Please specify the month of start and end periods. Line 208-10: The justification of longer periods for accretion estimation may also lie in the fact that organic matter accretion lags behind belowground ingrowth as it takes some time for the newly grown roots to decompose.

Yes, the reviewer points out an additional reason for removing these data and comparisons.

Line 225-7: Have you conducted the homogeneity test before ANOVA or MANOVA?

Yes, we used the Levene Test. We added this information to L 235 – 237 of the revised manuscript: " We tested for homogeneity of variances using the Levene Test on transformed data. The only violation of the equal variance assumption was for the

95% rooting depth, which, following log-transformation, failed the Levene test between estuaries, but not among marshes.”

3 Results

Table 1: add the statistical method you used in comparison of the variables.

As the reviewer suggested, we added the statistical test to the legend of Table 1.

Please check the label of ’Mean high water’; you have ab, bc, d, cd, a, ab but it is weird that there is no c. Some other variables also have the same problem, such as ’tidal range’ and ’long-term mineral sedimentation rate’ . Normally, the labels should be a, ab, b, bc, c......

We double checked the statistical output, and the letter designations in Table 1 correctly reflect the output of the Post-hoc Tukey Test. I think there is just a lot of overlap in error between sites, which is also a function of the nested design. I have attached some output from our test of tide range differences between marshes nested in estuaries:

You also need to check flooding events and duration of floods. For example, IB has 24 flooding events but 324h (per month or year?) of flooding time while MR has 455 flooding events but 7h of flooding time.

These data are correct. IB has had 24 flooding events and the duration of each flood averaged 324 hrs. By also comparing this with the % time flooded, it is clear that IB is almost continuously flooded so the # of flooding events is low, yet the flood duration is high. MR, on the other hand, is flooded frequently but has an average flood duration of 7 hours for each flooding event.

Table 2: I suggest you remove tidal range and MWL in the correlation analysis, or you keep them and remove MHW and MLW, and modify your results in ’3.1’. Tidal range is the difference between MHW and MLW, while MWL is the mean of MHW and MLW. You cannot correlate MHW or MLW with tidal range and MWL just like you will not correlate the area of a circle with the diameter (A=d2/4) since this is common sense.

Results section 3.1 was revised and the correlation analysis was removed from the Results section in the revision. Please see response to the same comment above.

Figure 3: why you do not show organic matter accumulation rate for IB?

Figure 3 was removed in the revision.

Line 294-6: you analyzed decay constant (Figure 3 and 4) rather than decay rates, and need to keep consistency in context.

Decay constant and decay rates were removed in the revision.

Line 309: were related or were not related? The sentence means they are related since you used ’neither’ and ’nor’.

This sentence was removed in the revision.

Line 298-9: the last sentence is unnecessary if these variables are excluded in the stepwise regression analysis.

This sentence was removed in the revision.

Figure 5 caption: the dependent variables in your regression analysis should not be organic/mineral accumulation rates, of which the unit is g/(m2.yr). The accumulation rates in Table 1 are the correct term. You need to revise ’3.2.3’ accordingly.

Figure 5 was removed in the revision.

Table 4: why don’t you use the data from all the sites to conduct the analysis of labile/refractory C densityâĹijbelowground biomass?

As the reviewer suggested, we added an analysis using data from all of the sites, as well as site-specific data. Combined this illustrates important biomass-C density relationship across marshes, as well as, how coastal lagoon marshes have a strong refractory C-biomass relationship, but coastal plain estuary marshes do not.

Figure 7: significant outliers are found in the relationship live belowground coarse

biomassâĹijMHW.

Indeed, live belowground biomass was much more strongly related to mineral sedimentation rate across marshes, which is also shown in Figure 7 (Figure 4 in revised manuscript). We added a comment about the variability in the relationship between MHW and biomass in the results section, which is reflected in the r-square value (0.44) in Figure 7 (Figure 4 in revised manuscript).

Why do you say mineral sedimentation rates correspond to average rates over the last 50 years? The time dated using Cs-137 relates to nuclear events (e.g.1963). Since sediment accretion rates vary from site to site and even position to position within the same site, the dating time at 50cm depth may not all be 50 years. This was a misstatement, and was removed in the revision.

Figure 8: a typo in the caption. It should be Table 3 instead of Table 2.

Yes, this was changed as the reviewer suggested and in the revised version, the legend refers correctly to Table 2.

Figure 9: No relationships between belowground biomass and (organic, refractory, labile) C accumulation/accretion are shown for Delaware Bay. Are all the relationships insignificant? Have you considered to examine the relationship between C accumulation rate (as a whole, rather than organic, refractory, labile) and belowground biomass?

Yes, the goal was originally to examine C-biomass relationships across estuaries. Yet, there were no significant relationships across estuaries. The only significant relationships were found in Barnegat Bay. We analyzed relationships for total organic (labile + refractory), labile, and refractory C and all biomass fractions.

Discussion

Line 402: you need to be specific about decay. It is decay constant.

This was changed in the revision, as explained above. The response variable is now

percentage of dry mass remaining.

Line 403: add mineral before sedimentation rate.

This sentence was removed in the revision.

Line 404: add coarse before belowground biomass.

This sentence was removed in the revision.

Line 405: replace little with insignificant as you can not consider the insignificant relationship in the linear regression analysis as little influence. Maybe environmental parameters co-vary with other factors, and explain some variance in multiple regression analyses.

This sentence was removed in the revision of the manuscript.

Line 420: I only found you examined the relationship between biomass and MHW. Where do you analyze the influence of elevation on biomass?

We did not analyze the relationship between biomass and elevation. We added text to the revised manuscript to explain why. In the Results section (L259 – L 266) and in the Discussion, we describe how hydrology was uncoupled to elevation across marshes due to factors such as poor drainage through mosquito ditches in IB, which created high water levels throughout the study, despite moderate elevations. Because plant and soil C processes respond more directly to hydrology than elevation relative to a datum, we used hydrologic parameters instead of elevation.

Line 432-3: tidal range is not comparable based on your results. One is labelled bc and the other is ab.

The statistical results indicate that there is not a significant difference in tidal range between RC and DN.

Line 446: the explained variance is 58% rather than 62%.

This sentence was changed in the revision.

Line 454: it is decay constants rather than decay rates which you did not estimate in your results. You need to modify other parts of the ms accordingly.

This was modified in the revision.

Line 484-7: There' are no direct linkage between CO_2 emissions and decay rates although decay contribute to CO_2 emissions, since other sources also contribute to CO_2 emissions such as crab burrows.

As the reviewer suggested, this sentence was removed in the revision.

Line 538-9: the factors relate to surface accretion are organic matter inventory and mineral sediment inventory.

This sentence was removed in the revision.

Line 551: Some sites have fine biomass lower than MR site such as RC.

This sentence was removed in the revision.

Line 553-4: you only show the influence of belowground biomass on specific components of C accumulation rates (organic, refractory, labile), and your discussion here and hereafter should be more specific.

We added some clarification throughout the manuscript that total organic C = labile C + refractory C. Thus, we examined relationships between belowground biomass and labile, refractory and total organic C accumulation rates as a whole.

References Boyd, B., Sommerfield, C.K., and Elsey-Quirk, T.: Hydrogeomorphic influences on salt marsh sediment accumulation 610 and accretion in two estuaries of the U.S. Mid-Atlantic coast. Mar. Geol., 383, 132-145, 2017. Unger, V., Elsey-Quirk, T., Sommerfield, C. and Velinsky, D. J.: Stability of organic C accumulating in Spartina 805 alterniflora-dominated marshes of the mid-Atlantic U.S.A. Estuar. Coastal Shelf

Sci. 182: 179-189, 2016. Ouyang, X., Lee, S. Y., Connolly, R.M. (2017) The role of root decomposition in global mangrove and saltmarsh carbon budgets. Earth-Science Reviews.166: 53-63. Haslett, S. K., Cundy, A. B., Davies, C. F. C., Powell, E. S., & Croudace, I. W. (2003). Salt marsh sedimentation over the past c. 120 years along the west Cotentin coast of Normandy (France): relationship to sea-level rise and sediment supply. Journal of coastal research, 609-620. Craft, Christopher. "Freshwater input structures soil properties, vertical accretion, and nutrient accumulation of Georgia and US tidal marshes." Limnology and oceanography 52.3 (2007): 1220-1230.

The format an inclusion of references was corrected in the revision.

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