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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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Dear Editor,

We very much appreciate the constructive comments and suggestions from the three reviewers. We have revised the manuscript to address all of the reviewer comments, which has resulted in a much improved paper overall. Below please find the Reviewer Comments and our responses below each comment . We have also added the revised manuscript as a Supplementary PDF.

Best regards,

Tracy

REVIEWER # 1 This manuscript covers a lot of topics and presents some interesting data, but it is difficult to follow and could be more effective in highlighting the key findings. Three hypotheses are presented but are poorly linked to data collection and analysis (they seem like after the fact general points rather than truly testable hypotheses). From the manuscript, the hypotheses are: (1) environmental parameters are highly correlated across marshes; however, hydrology is the most important predictor of belowground productivity, decay rates, and above- and belowground biomass; (2) short-term (< 2 yr) surface accretion rates are influenced by a combination of aboveground vegetation structure, belowground productivity, decay and mineral sedimentation rate; and (3) longer-term (âĹij50 years) accretion and soil C accumulation are more strongly related to belowground biomass in organogenic marshes in a coastal lagoon than in more minerogenic marshes of a coastal plain estuary, where the potential for allocthonous C contributions are greater. When I first read these, I wondered how would these be tested? For #1: how can you determine that hydrology is the most important predictor (in part, there are many components of "hydrology", how do you determine relatively importance, and in comparison to what other factors?). For #2: this seems very open ended rather than a testable hypothesis: accretion rates are influenced by a combination of factors? And for #3, the authors come to this conclusion in the discussion, but no mention is made in the analyses of how these comparisons would be made (what is the data/statistical support for this). The manuscript would be much more effective and focused, if clear, testable hypotheses were presented. The data collection and analyses should clearly identify how these hypotheses are to be tested. This would give some structure to the results rather than the wide ranging review of results that currently are difficult to link to specific questions/hypotheses.

We revised the text of the manuscript to improve the overall readability and provide a clear focus for the study. The reviewer is correct, the hypotheses included in the original manuscript were post-hoc and, therefore, poorly constructed. We refocused our manuscript around our a-priori (pre-study) hypotheses, which are directly related to the data analyses and results. The revised hypotheses are outlined in L 102 – 119 of the revised manuscript. Specifically, we hypothesized that rates of S. alterniflora belowground productivity were greater in marshes of the coastal plain estuary than in the marshes of the coastal lagoon, where a higher water table, higher salinity, and lower rates of sediment deposition were predicted to limit root and rhizome growth. We predicted that patterns of belowground productivity and turnover would mirror those of longer-term total and labile organic carbon accumulation rates across marshes and estuaries. Our hypothesis would be supported if environmental conditions that promoted C accumulation such as high rates of mineral sedimentation and, potentially, high tidal range and low salinity also promote high belowground biomass production. Further, we examined the role of belowground decay in explaining spatial patterns of C accumulation. We hypothesized that the amount of organic material remaining following 20 months of belowground decomposition would be greater in marshes with higher C accumulation rates. For this, the conditions that promote high rates of C accumulation may also promote the preservation of C particularly in the upper soil column where much of the decay of labile organic matter occurs (Hackney and de la Cruz 1980; Hackney 1987; Morris and Bowden 1986). Ultimately, the net amount of belowground biomass (C fractions greater than $\sim$1 mm in size) was predicted to be directly and positively related to the density of C in the soil profile and C accumulation rate. Similarly, above-and belowground biomass was predicted to be positively related to soil C accumulation. Finally, because plant productivity and decay processes as well as overall plant structure (e.g., height, stem density, biomass) have been shown to be tightly regulated by abiotic factors, we examined the influence of local environmental conditions (i.e., water level, salinity, soil nutrient status, and sediment deposition rates) on S. alterniflora growth, decay and biomass across marshes and estuaries.

There are some arguments that are presented that are difficult to untangle: for example, mineral matter drives productivity. If this is the case, what is the expected “response” that supports this and what “response” would not support this (how would the

measured parameters of total biomass, ingrowth, mineral matter accumulation, etc., vary if this is true and what if it is not true – or is the key in the the relationships of different parameters)? As above a clear articulation of expectations (hypotheses) is essential but lacking. Without these, it’s an interesting story but not so clear what is actually being supported from these findings/data. Part of my confusion in interpreting the results is that this is a relatively complex set of experiments with many different factors and response parameters. In terms of factors, there are two locations, with multiple sites within each location – and many factors vary both across locations as well as within locations: tidal range, sediment inputs, salinity, inundation, etc

By revising the hypotheses, the somewhat complex study design and findings are clarified. We have also revised much of the text to provide justification for our hypotheses and context for our findings. The specific argument highlighted above, “mineral matter drives productivity” was removed in the revision of the manuscript.

Plus there are many different response parameters, some closely related, some not (ingrowth, biomass, decomposition, accretion, C accumulation, mineral accumulation, etc ...). It might be very useful to put together a summary table that links the various components of this research to the hypotheses/research questions of interest (factors, locations/sites, responses, expectations). Or at a minimum, to clearly identify in the methods what these links are: to test the hypothesis #2, we compared xxxx across sites (or across inundation conditions within sites), using xxxx analysis...

We revised the text throughout the manuscript to simplify the hypotheses and how they relate to the response variables. For example, the multivariate correlation analyses were removed from the results section and relevant information was summarized in the data analysis section. Table 2 showing the correlation analysis results was moved to a Supplementary Table.

Overall, I found the writing difficult to follow. Many of the paragraphs are very long and cover a mix of topics. I’d suggest focused paragraphs with very clear topic sentences so that the logic of each section is clear and easier to follow than the current paragraphs that ramble over a mix of topics. In addition, there are some grammatical mistakes, dropped words, etc. that make the manuscript difficult to understand (e.g.,l.66: should be wide range OF geomorphic settings) check throughout for grammar (many compound sentences missing commas (l.212-213), etc ...). Also some sentences are overly complex and difficult to follow (for example, the last sentence of the abstract): "These findings indicate that mineral sedimentation is of utmost importance for promoting belowground biomass and soil C accumulation in sediment-limited systems while in minerogenic systems, belowground biomass may not scale with C accumulation and accretion, which may be influenced more by smaller submillimetre-sized C particles." (secondarily, I don't think submillimeter particles are brought up again in the manuscript, so why are they in the abstract?)

The text was revised throughout to improve readability and grammar. Strong topic sentences were added to the beginning of paragraphs and the amount of rambling was minimized. The sentences highlighted by the reviewer above, were ultimately removed from the revised manuscript.

It was not entirely clear what was previously collected background information, and what was new data for this study. For example, you refer to published rates of accretion from Boyd et al. 2017: are the accretion rates here the same data or different?

We added text to both the Introduction (L 78 – L 99) and Methods (L 155 – L 157) to clarify that we used 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 discussion of elevation is not so clear. Be more specific. I'm assuming that it is relative elevation that is critical (where within the tidal frame the marsh surface is found). For example on Figure 4, is this elevation relative MLW (see other point below about MLW)? And are positive elevations above or below MLW? I would put lower

elevations on the left side of the x-axis (not sure if this is the case as presented). It also looks like much of this relationship is driven by the two points with zero biomass. How does this affect your interpretation: is it just a threshold relationship or is it really a linear relationship? Also, for figure 5, organic matter inventory: the one outlier seems to be driving this relationship. Does this affect your interpretation?

We have clarified our use of elevation and water level data throughout the methods and results. While we collected elevation data (relative to North American Vertical Datum 1988), what we used in our analyses were water level data, which were calculated relative to the marsh surface. This, as the reviewer indicated, is what is critical for driving plant and soil processes. For Figure 4 (Figure 3 in revised manuscript), we revised the axis to read MLW depth relative to marsh surface (cm). While it appears that the relationship was driven by two points, when those points were removed, the linear relationship remained significant with only a slight reduction in the R-square value. We included this information in the Results section, as well as a sentence on this relationship really being more of a threshold relationship, as the reviewer aptly pointed out.

It's surprising that you've found a strong fit between mineral accumulation and accretion rather than organic matter and accretion. Most others have found differently (e.g., Turner et al. 2000). How can you explain this difference?

This is a good point, which we subsequently worked into the discussion. Actually, Table 2 in Turner et al. 2000 illustrates regional differences in the contribution of mineral sediment to accretion. Along the U.S. Atlantic coastal plain, accretion rates were directly related to both mineral sedimentation and organic matter accumulation rates for most marshes (all but 1 study), as well as across all Atlantic coastal marshes combined. Our previous studies have shown a similar trend (Unger et al. 2016; Boyd et al. 2017). Conversely, in U.S. Gulf coast marshes, accretion rates were related to organic matter accumulation rate only. Turner et al. 2000, hypothesized that at high rates of mineral sedimentation, the relationship between accretion and mineral sedimentation becomes variable associated with a threshold of organic production at high rates of mineral sedimentation. I might suggest that these linear averaged accumulation rates, don't account for major declines in mineral sediment over time, and thus with a high historic sediment input and a much lower recent sediment supply to many Gulf Coast marshes, the relationship between accretion and sediment input becomes variable. In addition, accretion rates also respond to changes in relative sea-level and have done so in Gulf Coast marshes mostly by organic matter accumulation. These marshes are experiencing subsidence and deterioration due to the lack of sufficient mineral sediments to support plant growth and biomass. Our study illustrates this point, and suggests that allochthonous C burial and C preservation may also be significant at high rates of sedimentation, provided that marshes have a relatively continuous supply of sediment over time.

For Figure 3: how can the decay rates and the % mass remaining not be indirectly related: How can CC have the highest decay rate, but have more mass remaining than 3 of the other sites? These should be strongly related.

We have included a Supplementary Figure (A) to explain this. Marshes in both estuaries had similar amounts of organic matter remaining, yet Barnegat Bay had a steeper decline in organic matter over time. However, the reviewer's comment highlighted the need for some additional explanation. Litterbags in Barnegat Bay were placed in the marsh slightly later in the year than those in Delaware Bay due to logistical delays following Hurricane Sandy. As a result, asymptotic decay rates were greater in Barnegat Bay, however, this was likely associated with slightly warmer temperatures following deployment as compared to Delaware Bay. We have added this information to the Methods (L 203 – L 206) and removed the decay rate calculation and results, and instead just used % mass remaining following 20 months as the response variable.

Many of the figures present multiple panels, and it is not clear, what is essential to get out of a figure: seems more like a fishing expedition in presenting a wide range of results rather than targeting specific questions/hypotheses.

Each figure in the revised version of the manuscript is directly tied to hypotheses stated in the Introduction, data analyses in the Methods section, and findings in the Results section.

Details: You refer to cores of 6 cm diam. in line 144, but then 15 cm cores in l. 160. Were two different sets of cores taken? This needs to be clarified.

The cores were 15.5 cm in diameter. The size was corrected in the revision.

Be consistent in how you refer to sites: sometimes in the coastal plain site, sometime it's a minerogenic site.

We revised the manuscript for consistency when referring to estuaries and marshes. We refer to coastal plain and coastal lagoon in the Introduction and Discussion. We refer to the specific estuaries (i.e., Delaware Bay and Barnegat Bay) in the Methods and Results sections.

Paragraph starting at l.276 (and paragraph above): this all seems very exploratory, with little focus: you looked at a wide range of variables for patterns, went with MHW and MLW. As above, link the approach to the hypotheses (and move the methods to the methods section and out of the results).

This section was removed in the revision. The correlation analysis was performed so as to select environmental variables that were somewhat independent, not strongly co-varying with others, so as to limit our interpretation. We moved any relevant information to the Data Analysis section.

Also, it was not clear to me how MHW and MLW represent the range of factors (were these absolute elevations of MHW or MLW) – as you can see, I did not follow this section of the ms. very well (it was not clear to me, but maybe it is clear to others). Similarly at l.288: how does "MLW influence ingrowth rates"? MLW is a characteristic of a particular site, but how does it influence growth across a marsh?

This point is now clarified in the revised manuscript. MHW and MLW were average high and low water depths relative to the marsh surface calculated from two years of continuous water level data. Root ingrowth rates were higher in marsh locations where the average low water depth was lower than in areas where the average low water depth was high (i.e., greater root growth with greater drainage during low tide).

Lead with the key issues in presenting the data for each section. For example, for aboveground vegetation structure (paragraph starting at l. 316): clearly stem density is important, but why include the CV here: what is the significance of this? As above, I got lost in the details of the data that were presented, and did not see the key issues from the results.

We highlighted key issues and findings relevant to the hypotheses to the beginning of each result section.

Figure 1: provide some context. Not all readers know where Delaware and New Jersey are.

We added context to the legend.

Other figures: As above, be consistent in mentioning features of sites so people will remember lagoon vs. coastal, minerogenic.... For example for Figure 3, group sites as you do for Figure 2 (or color bars or use hatching so that the two groups are obvious).

As the reviewer suggested, we have revised figures to be consistent using Barnegat Bay and Delaware Bay for designation of data points.

Figure 3: the dark bars on the bottom panel, make it very difficult to see the symbols for organic matter accumulation rates.

This figure was deleted in the revision of the paper.

Similar to the broader point about figures with multiple panels, some multi-panel figures are not organized intuitively (at least not for this reader). For example, for Figure 7: what is mineral sedimentation the x-axis on the top two LEFT panels and the bot-

tom two RIGHT panels? The wide mix of combinations, makes if very difficult to see patterns and follow the logic of the data presentation.

We revised Figure 7 (Figure 4 in revised manuscript) such that all x-axis mineral sedimentation rates are on the left panel and all x-axis MHW depth are on the left panel, as the reviewer suggested.

References: Some of them are out of order: See Cahoon at l. 618 and again at l. 639. In addition, some journal titles are abbreviated, some are spelled out in full (l.619 &625).

We have edited the references, as suggested by the reviewer.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2017-268/bg-2017-268-AC1-supplement.pdf

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-268, 2017.

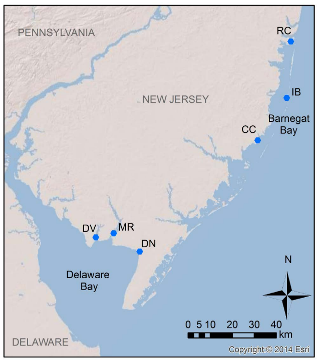

Figure 1: Study locations in Barnegat Bay and Delaware Bay along the mid-Atlantic coast, U.S.

Fig. 1.

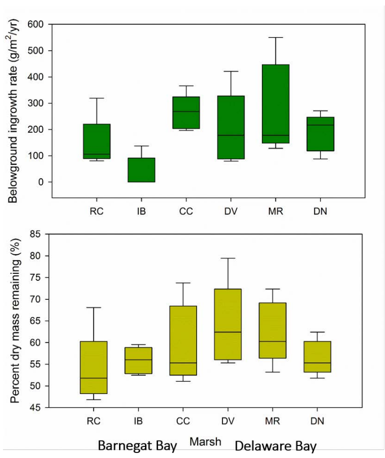

Figure 2: Belowground ingrowth rate (upper) and percent mass remaining in belowground litterbags (lower) over a 20 month period in salt marshes of two mid-Atlantic estuaries (n = 5, ± standard error).

Fig. 2.

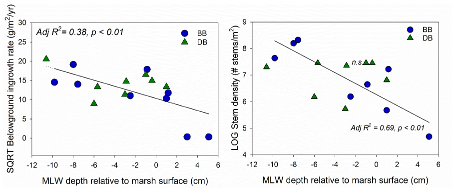

Figure 3: Relationships between belowground ingrowth and mean low water (MLW) relative to the marsh surface (left) and stem density and MLW (right). Blue dots and green triangles refer to data collected in Barnegat Bay (BB) and Delaware Bay (DB), respectively.

Fig. 3.

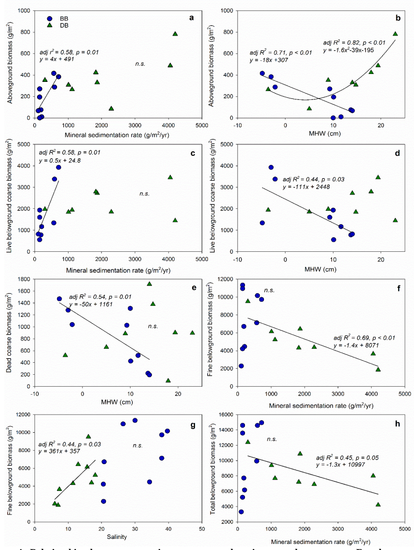

Figure 4: Relationships between vegetation structure and environmental parameters. For these analyses, belowground biomass to 50-cm depth was used. Mineral sedimentation rates were calculated using ^{137}Cs-dating, and therefore, are average rates over the last 50 years (Boyd et al. 2017). Blue dots and green triangles refer to data collected in Barnegat Bay (BB) and Delaware Bay (DB), respectively, which were analysed separately. Both significant and non-significant (n.s.) relationships are shown.

Fig. 4.

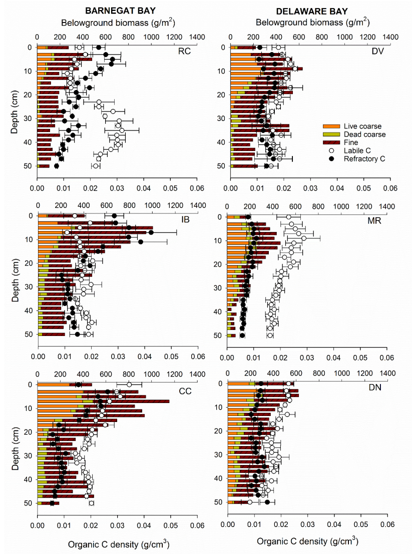

Figure 5: Belowground biomass and labile and refractory organic C density profiles in marshes of two Mid-Atlantic estuaries. Biomass data are means (standard errors not shown). C density data previously reported in Unger et al., 2016.

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Fig. 5.

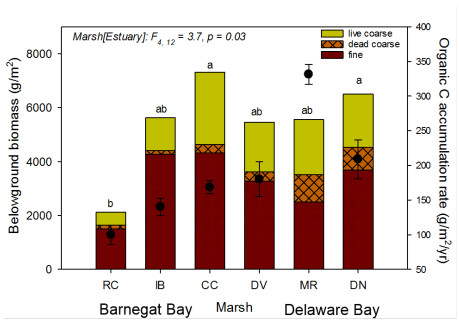

Figure 6: Belowground biomass (scaled on left-axis) and organic C accumulation rate (scaled on right axis; previously reported in Unger et al., 2016) in marshes of two Mid-Atlantic estuaries. Belowground biomass and C accumulation are relative to the ^{137}Cs peak depth. Statistics for total belowground are shown and letters represent differences ($p < 0.05$). Standard errors are shown in Table 2.

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Fig. 6.

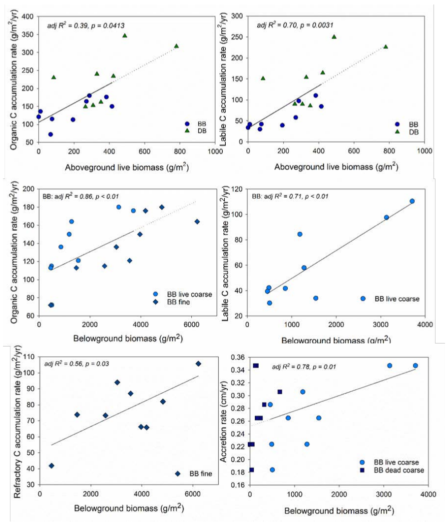

Figure 7: Relationship between *Spartina alterniflora* biomass and organic C (total, labile and refractory) accumulation and accretion rate in marshes of Barnegat Bay (BB) and Delaware Bay (DB). Only significant ($p < 0.05$) relationships are shown.

Fig. 7.