Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-268-AC3, 2017
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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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Received and published: 18 October 2017

REVIEWER #3

This paper is a substantial and interesting addition to the literature and I think that it could be publishable for Biogeosciences Discussions pending some revisions. The study reports correlations between geomorphic variation in variables such as tidal elevation, tidal frame, suspended sediment, salinity, etc. with important biotic variables affecting carbon sequestration (above and below ground biomass, root addition, root-shoot ratio, and recalcitrant/labile carbon fraction), and observations of Carbon Accumulation Rate, measured by radiocesium date and previously published by Unger et al.

and Boyd et al.. The observations that complex correlations between root production and drainage, and sediment trapping having multiple positive interactions with carbon burial, are compelling conclusions as they point to the complex and dynamic nature of tidal wetland systems more generally. Predicting behavior outside of well-studied plots and sites is a large concern of the community and this paper speaks to many difficulties in those efforts. I think what set's this paper apart from much of the literature is how well monitored all of the sites are. All locations have measured elevation, inundation, and soil properties. This should be commended and in many ways is close to an ideal salt marsh carbon dynamics study design. I have thee major critiques of the paper, somewhat overlapping. 1. There is not enough available methods data for the calculation of tidal datums from the water loggers. I found some of the inclusion of comparing NAVD88 elevation, MHW and MLW hard to follow, especially when these were used as proxies for multiple hydrologic properties. I was a bit taken aback at how much variation there were in datums that are located fairly close together. Could this be because of the short 1.5-year time period? 'No data' values deflating MLW datums? Etc? Is there really that much local variation in datums? I would like to see more information before making a judgment there.

Yes, the reviewer is correct, more explanation was needed to discuss the datums. This was similar to a comment made by Reviewer #1. 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. We included this information in the Methods and Results section.

2. This leads me to my second critique. A lot of the correlation analysis could be paired down. First because of what I discussed in one, maybe some of these measures are redundant or could be reduced to more directly causal variables.

For example, by converting site elevations (NAVD88) into dimensionless elevation z = (Elevation - MTL) / (MHHW-MTL) [Swanson et al., 2012], or focusing on flooding depth and # of floods a year (maybe converted to volume of water / unit time) since those are probably a clearer functional driver for organic and inorganic sedimentation. Maybe there's a better metric for drainage, such an average time between inundation events. The number of correlations discussed makes the text âĬĘAËĞT especially figure 7 âĬĘAËĞT a bit cluttered and hard to decipher on a quick read through (although there are benefits to being thorough). Statistically, the number of correlations presented is problematic. Which leads me to point 3.

Yes, indeed. As the reviewer suggested, we revised the text and removed the multivariate correlation analyses from the results section. Relevant information was summarized in the data analysis section. Table 2 showing the correlation analysis results was moved to a Supplementary Table.

3. There are many correlations presented, but the threshold for significance (0.05) is not adjusted for multiple testing. The more correlations your run, the higher chance of getting false positives. Researchers often address this by using the Bonferroni correction, or some related correction (there are some arguments that the Bonferroni is too restrictive). The fact that many of the regressions presented are barely significant at the 0.05 level and many would no longer be significant after correcting for multiple testing. Maybe a combination of reducing the number of variables tested to a few functionally important variables and adjusting for multiple testing could allow the authors to focus more on the very clearly significant driving geomorphic variables.

The purpose of the correlation analysis was to identify environmental parameters for which other parameters co-varied. Almost all significant relationships had p-values of < 0.01. As a result, we only used a sub-set of environmental parameters in subsequent analyses. Almost all of these had correlations or regressions where the p-values were very low, and only 2 or fewer independent variables influenced response variables. Based on this, we don't feel that a Bonferonni correction is really necessary. However, we rans some exploratory analyses with Bonferonni corrected data, and found similar results to what is presented.

Line comments: 63: This is an excellent point that does not get enough attention in the literature.

Excellent! This sentence is now L57 – 59 in the revised version.

64: Is there any literature you could cite in the hazards literature or other ecosystem-climate change dynamics that discuss complexities in projecting system resilience?

While this is an excellent discussion point, it may be a little outside of the scope of this paper. Particularly, now that we have made changes to improve the clarity and focus.

132: What is the time frame of RSLR? Same as cores, or total length of the gauging period?

The time frame of RSLR was the same as the cores/ We added a few words to clarify. L 148-151 in the revision "Accretion rate in Barnegat Bay marshes (0.28 $\pm$ 0.06 cm/yr) over the last 50 – 100 years was less than the rate of relative sea-level rise over approximately the same time period (0.41 cm/yr; NOAA, Tides and Currents; in Boyd et al., 2017). In Delaware Bay, salt marsh accretion rate (0.70 $\pm$ 0.26 cm/yr) exceeded the rate of local relative sea-level rise over the same time period (0.34 cm/yr, NOAA, Tides and Currents)."

277: If dimensionless elevation may be a better fit than using MHW and MLW. Alternatively inundation time, the number of inundation events or cumulative annual mass of water seem like they would be much better variables to use as there is process-knowledge involved.

The reason why we chose to use MHW and MLW because these variables represent the magnitude of surface flooding at high tide and the magnitude of drainage at low tide. We anticipated that these would be important biologically. However, this was not very well clarified in the original submission, and therefore we added this information explicitly to the data analysis section.

427: Is sediment trapping by biomass part of this positive feedback?

Yes, an excellent point. In the revised manuscript, we highlight the importance of aboveground biomass and its relationship to labile C accumulation rate. While there are several mechanisms that can explain this relationship, sediment and allochthonous labile C trapping is one.

Tab 1 - What is MHW MWL and MLW relative to NAVD88? Station datum?

We added this information to the table and all relevant figures; it is relative to the marsh surface.

Tab 3 - Far left column a bit hard to read. Maybe fix in formatting.

We fixed the formatting of the table, as suggested by the reviewer.

Fig. 1 - Howe a basemap with better definition. Maybe one that emphasizes the differences between uplands systems and wetlands. I would delete the service layer credits and put it into the figure caption. The map looks low resolution.

We have replaced Figure 1 with a new better resolution map of the study locations.

Citations: Swanson, Kathleen M., et al. "Wetland accretion rate model of ecosystem resilience (WARMER) and its application to habitat sustainability for endangered species in the San Francisco Estuary." Estuaries and Coasts 37.2 (2014): 476-492. Abdi, H. (2007). Bonferroni and Šidák corrections for multiple comparisons. Encyclopedia of measurement and statistics, 3, 103-107.

We thank the Reviewer for the citations.

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