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Interactive comment on "Biophysical controls of marsh soil shear strength along an estuarine salinity gradient" *by* Megan N. Gillen et al.

Anonymous Referee #1

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This paper investigates the influences of salinity and belowground biomass on soil shear strength in coastal wetlands of the Chesapeake Bay area. It is well-organized and clearly written. The data used and methods of analysis are, in themselves, sound. However, I would like to suggest a few changes that would improve its clarity, and also point out what I consider to be weaknesses in their overall conclusion that, broadly speaking, tidal freshwater wetland soils have reduced soil shear strengths compared to their salt marsh counterparts.

1) I may have missed it earlier in the document, but I could not find in the text how the five sites were classified as either "fresh", "brackish", or "saline" until the caption for Figure 6. Prior to that figure, numerous references are made regarding these marsh types, but it is difficult to follow along with the data presented without knowing which

sites were classified as which marsh types. Please specify this classification earlier in the paper (ideally in the methods/study site section).

2) The bulk density values reported often seem quite high (many exceed the density of granite, commonly taken as 2.65 g/cm3). Are the bulk density values reported in the manuscript "wet" or "dry"? If they include water weight ("wet"), that would explain the high values.

3) The elevations measured at the Pamunkey site were much lower than those measured at the other sites. The authors explain this difference as possibly arising from the Pamunkey elevations being surveyed early spring prior to vegetation growth, while elevations at the other sites were measured well into the growing season. I suppose that makes sense to some degree, but the difference approaches 50 cm (through examination of Figure 1). That seems like a lot of elevation capital to attribute to a seasonal biomass cycle, and whether or not this site is considerably lower than the other site has fairly strong implications for interpretation of some of the differences seen in the data. It would be helpful if the authors could explain how before/during growing season differences could explain this much elevation difference in the marsh soil surface, which is presumably what was surveyed, and specifically what processes are at play here.

4) Following up on comment (3) above, numerous studies exist that illustrate the importance of inundation in controlling belowground biomass. Many show hump-shaped responses, where high sites and low sites exhibit reduced biomass/productivity, and optimum production occurs at intermediate elevations. Others show monotonic production/biomass decreases with decreased elevation (or increased flooding). There is no mention of those dynamics to speak of in this manuscript. The authors should interpret their findings to some degree in the context of those studies. This is particularly the case given that their sites consistently show higher elevations at the "interior" locations than at the "edge" locations. Those differences are not trivial – mean interior elevations exceed their edge counter parts by 11cm, 14cm, 15cm, 11cm and 20cm for Pamunkey, Sweet Hall, Taskinas, Catlett, and Goodwin locations, respectively. I'm unfamiliar with

how those elevation differences would translate into differences in hydroperiod in the Chesapeake Bay region, but in the microtidal, low gradient wetlands on the northern Gulf of Mexico coast, those elevation differences would easily translate into differences in flood duration that would exceed 30 % (e.g., an interior marsh at 0.25 m NAVD that was flooded at 40% of the time would be flooded around 70% or more of the time if its elevation were reduced by 0.15 m).

5) In Lines 151-153, enhanced nutrient loading at the edge sites relative to the interior sites is suggested as a possible explanation for why shear strength at the edge sites may be lower. However, according to figure 1, this distance is only on the order of 10 m. It does not seem reasonable to expect a meaningful reduction in nutrient concentrations over this length scale, but perhaps the authors can demonstrate otherwise with citations to support this claim.

6) The authors cite the Howes et al. (2010) paper that concludes that "salt marshes are more resistant to later edge erosion than freshwater marshes" (Lines 158-160, this manuscript). The Howes et al. (2010) paper identified Spartina patens as the dominant vegetation present in the low shear strength region of Breton Sound basin, Louisiana, that was so badly decimated by the shearing forces of Hurricane Katrina. They pointed out that although this species "has extensive rooting but of smaller diameter [than S. alterniflora]. The plant is less tolerant to anoxic soil conditions, which likely limits the rood network to shallower depths" and use this logic to conclude explain why S. patens regions were sheared while S. alterniflora regions were largely intact after the storm. However, in lines 180-185 of this manuscript, the authors cite the presence of S. patens (and the co-occurring S. alterniflora) as the reason the salt marshes of the York River estuary exhibit the relatively high shear stresses, owing to their high productivity and their creation of dense networks of belowground biomass. It is difficult to square these – Howes et al. (2010) on the one hand indicating that it is the limited development of the S. patens root network that contributes to a low shear stress, and this manuscript on the other hand citing the dense root network of S. patens in promoting high shear

strength. It is important to consider, when citing Howes et al. (2010), that their "low salinity", "low shear strength" zones were vegetated almost exclusively by S. patens. In the present study, S. patens is identified as being in the high shear, high salinity zone.

7) Given the rather large elevation gradients across each of the transects (see comment 3 above), it seems reasonable that vegetation species composition varies markedly across the transects (particularly between the "edge" and "interior" zones. Given that a paper cited frequently in this manuscript (Howes et al. 2010) attribute variations in shear strength to taxa-specific morphological differences in root structure, if transitions in species composition existed across the elevation gradients in the transects, within-transect species composition could be responsible for some of the patterns observed in the manuscript. Can you speak differences in species composition not only between the transects, but within each transect as well?

8) Similar to what was done for figure 6, could the regression in Figure 5 be separated by marsh type? For example, when breaking this regression down by site (see attached figure), a different picture emerges – biomass vs. shear strength is significant for Taskinas (the brackish site) and Goodwin (one of the saline sites), but insignificant for the remaining three sites. Beyond simple reductions in sample sizes and degrees of freedom, are there other explanations for why this relation may be significant at some of the sites (or marsh types) but insignificant at other sites?

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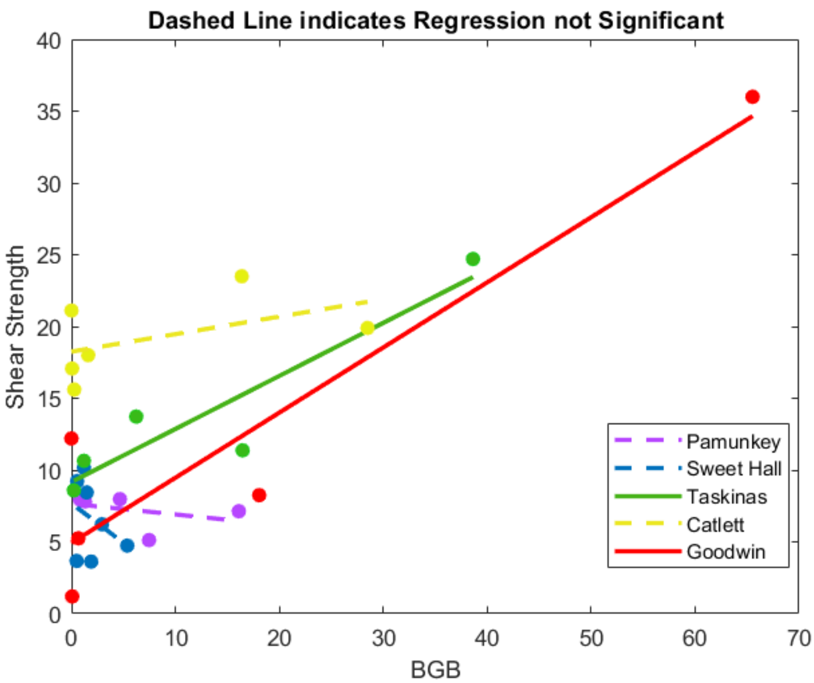

Fig. 1. Figure 5, BGB vs Shear Strength, by site