Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2020-58-RC2, 2020

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

Anonymous Referee #2

Received and published: 23 November 2020

General Comments: This reviewer believes that the data presented in the manuscript is of value and should be publishable. However, there are numerous problems (see below) that first need to be addressed.

Specific Comments: 1. Line 11: "erodibility" was not directly measured in this study. Insert "potential" between before "marsh erodibility". 2. Line 12: Insert "likely" before driven. 3. Line 13: "rooting structure" was not measured in this study. Hence, the need for comment 4. Lines 15-16: The last sentence in the abstract is misleading and not supported by the data presented in the manuscript. Based on the data presented in the manuscript, one can only state that the freshwater marshes investigated had weaker soils than the salt marshes investigated. Extrapolating to salt marshes and fresh marshes universally goes far beyond this specific study, and, based on Table 3

in Sasser et al. (2018), is an unsupported generalization. 5. Line 65: the statement is made that there "were consistent elevations across all study sites" and Fig. 1b is referenced. However, when this reviewer looks at Fig. 1b, one sees elevations that differ both among the marshes and within a marsh. Hence, this statement appears incorrect and may need qualification. 6. Lines 65-68: The reviewer suggests rewriting these lines as follows: We collected samples from two zones within each marsh: (1) the tidal channel marsh edge located between the tidal channel and any levee (1 m from edge) and (2) the interior marsh located at a measured distance of 10-12 m away from the edge site (Fig. 1b). 7. Lines 78-79: Provide information on the spatial distribution of the 10 replicated profiles. Were they within a 1-meter radius, for example, or where they distributed over a 10 m distance parallel to the shoreline, for example? 8. Lines 83-84: It is stated that "All sites were located at similar elevations ...." . , However, Fig. 1b shows that, for example, the Catlett study site had dissimilar elevations within the edge zone. Maybe what would help is to provide the range in elevations for the specific zones in each marsh where shear strength was measured. A table might also work. The bottom line is that the authors do not adequately convince this reader that elevations were "similar". 9. For both above- and belowground sampling, the spatial distribution of the three replicates should be provided. Were they within a meter of each other or 10 meters of each other? 10. Generally, soil shear strength is very much dependent on the plant species that occur where measurements are taken. Knowing the dominant community type does not provide enough information. Hence, within the Methods or Results, the authors should specify the specific plant species present where the shear strength vanes were inserted. In the salt marsh, of course, it is likely to be Spartina alterniflora, but in the brackish marsh, it could be any number of species (e.g., S. patens, S. cynosuroides, or Distichlis, etc.), and in the fresher marshes it could be a mixture of species or one dominant like Peltandra. This information is needed to better interpret the shear strength results. 11. Lines 57-101: This reviewer is surprised that soil textural components were not measured. Are sand, silt, and clay data available for these study sites? 12. A Statistical Analysis sub-section is absent from the Methods section. The authors should describe the experimental design of their study and the statistical methods used to test significance. In this regard, this reviewer finds it difficult to understand why the authors did not use an Analysis of Variance approach to identify significant differences among study sites and between zones, as well as their interaction, which they graphically present in Figure 4 and discuss in lines 119-123. See below for more on this. 13. Lines 119-123: Figure 4, which is discussed in lines 119-123, may be the most important results of the study. Yet, it received no statistical analysis. Although one might argue that the study is pseudoreplicated, given that the York River salinity gradient was not replicated i.e., two or more rivers), one can still do a 2-way factorial ANOVA with study site (the five marshes) and marsh zone (edge and interior) as the main treatments. The marsh zones should be nested within study sites. The authors could even include, if they desired, depth as a repeated measure (repeated in space). The caveat is that inferences would only be applicable to the marshes studied because there was no true replication of the salinity gradient. So, extrapolation of conclusions to other marshes would have to be done cautiously. This is the approach this reviewer would take. Of course, one might posit that the main effects and interactions are obvious in Figure 4 and no stats are needed. Although this reviewer agrees that the effects are obvious, it's always better to have a rigorous statistical analysis supporting what appears intuitive. In summary, my recommendation is that an ANOVA of the data be performed and included in a revised manuscript. 14. Lines 119 -123: Comments about treatments being equal or different are not supported by statistical analyses, as discussed in comment 9. 15. Line 125: Insert "R2 = 0.58" before "p=1.086e-5". 16. Lines 127-129: Organic content was correlated with shear strength, yet organic matter data were not presented. The organic matter data should be in a graph or table. The probability of the relationship was close enough to 0.05 to be of interest. If the authors think the relationship is unimportant based on a probability of 0.059, they should state it was not significant. 17. Line 128: Instead of using the phrase "marginally significant", This reviewer suggests you simply state "…significant at p = 0.059". 18. Line 129: Water content is mentioned but no

data presented. Either delete water content in this line or say more about the relationship between it and organic content. 19. The authors statistically exlain variation in soil shear strength with salinity, aboveground biomass, belowground biomass, and bulk density. R2 values and regression graphs are provided for each of the predictor variables. This reviewer suggests the authors perform a stepwise multiple regression, similar to that done by Ford et al. (2016), to try to tease out the relative importance of each of these. Granted that some of the predictors might not be completely independent of each other, but this approach, especially when one varies the sequence by which the predictors enter the regression model, might help to better explain the important drivers of shear strength differences. 20. Lines 134-135: Sasser et al. 2018 also found that soil shear strength positively correlates with belowground biomass. Therefore, the present work confirms, rather than "extends", the concept. 21. Lines 138-139: This study did not directly measure "marsh erodibility", and hence the statement that "our results demonstrate that soil properties such as bulk density are also important drivers of marsh erodibility", although likely true, is not empirically demonstrated by this research. The sentence should be revised to reflect this. 22. Lines 139-140: This is an important finding. 23. Line 141: ANOVA would confirm this. 24. Lines 153-154: It is not clear to this reviewer if belowground biomass drives soil shear strength variability in all marsh types. Figure 5 shows that four data points drive the significant relationship between shear strength and belowground biomass. Are these data points from a single marsh type or study site? The relationship between belowground biomass and soil shear strength needs to be fleshed out more than just how it varies with marsh zone; marsh type should also be addressed in Figure 5 by using different shaped symbols. 25. Lines 154-155: It is stated "….and soil properties influence marsh edge shear strength (Fig. 6) for brackish and salt marshes". Figure 6 presents shear strength as a function of bulk density. It is clear that for salt marshes, bulk density is an important factor determining soil shear strength (p=0.002). However, the relationship is not significant for brackish marshes (p=0.318). Hence, the statement in lines 154-155 as written is incorrect. 26. Lines 160-162: The authors state that this study found that salt marshes soils are generally stronger than fresh marsh soils. Howes et al (2010) concluded similarly. However, this reviewer believes we need to be cautious is making these broad statements about the importance of marsh type because soil strength may depend on the specific plant species that dominate the marsh, as well as the specific soil type in that marsh. For example, will a freshwater Peltandra marsh have the same soil strength as a freshwater Panicum marsh, and will they similarly differ from a Spartina alterniflora salt marsh? This reviewer submits the answer is likely – no, based on the work of Sasser et al. (2018), which showed large differences in shear strength both between marsh types and within marsh types. In addition, a freshwater marsh with a sapric organic soil is likely to have a different soil shear strength than a freshwater marsh with a mineral (entisol) soil, regardless of the dominant species. This reviewer suggests that the authors expand this section to include a discussion of these nuanced, but important, concepts. In summary, the differences seen in soil shear strength between salt and freshwater marshes are likely due to differences in dominant species, soil type, belowground biomass and structure, and hydrogeomorhic setting, all of which affect each other. 27. Lines 169-174: There is no discussion of the brackish marsh dominant and its root structure and anatomy. This should be added. 28. Lines 185-190: The important factor associated with biodiversity may not be biodiversity, per se, but rather the specific species contributing to the biodiversity. A monospecific stand of Spartina alterniflora may generate a greater soil shear strength than a diverse community of freshwater dicots with shallow and low-density roots. This concept may be similar to the importance of plant diversity to primary productivity and stability. It's not greater biodiversity that's important, but rather the species composition (or functional guilds) comprising the plant community. The authors should emphasize the potential importance of species composition. 29. Lines 196-197: This reviewer believes this statement is too simplistic. This reviewer agrees that all things being equal, saltwater intrusion alone would create stronger soils by promoting Spartina alterniflora and increasing bulk density via enhanced sedimentation. However, saltwater intrusion will be accompanied by increased water levels, assuming that sea-level rise is the driver of

the saltwater intrusion. Hence, coastal salt marshes will experience longer periods of inundation and higher water levels. Prolonged inundation will decrease root productivity and live belowground standing stock, resulting in a reduction in soil shear strength. Hence, the statement – "Although these changes will have a variety of ecological and geomorphic consequences, our work suggests that saltwater intrusion may be accompanied by stronger salt marsh soils that are less easily eroded." does not tell the whole story. 30. Figure 5. This reviewer suggests that the symbol style should differ by marsh type (salt, brackish, fresh) or by study site so that the reader can visualize the importance of marsh type in determining the relationship between belowground biomass and soil shear strength.

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