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Interactive comment on "Plant genotype determines biomass response to flooding frequency in tidal wetlands" *by* Svenja Reents et al.

Anonymous Referee #2

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Considering the heightened vulnerability of tidal marshes to SLR, an increasing number of studies are examining flooding and other climate change impacts to marsh plant growth and viability and their feedbacks to marsh elevation and resilience. As the authors note, most of these do not consider responses of different genotypes of the same species, but rather responses at the species level or among species. Thus, this experiment, which investigated biomass responses of different plant genotypes to increasing flooding frequency, fills an important gap. While the overall conclusion that the low-marsh genotype is better adapted to flooding than the high-marsh genotype is an intuitive one, this paper provides some direct evidence of biomass responses and suggests that formation of longer rhizomes by the low-marsh genotype serves as a

flooding escape strategy. The paper is generally well-written and presents vegetative response data clearly and succinctly. However, there are several areas in need of attention, as detailed below.

Broader context: Situating this work within the context of other studies examining population-level or genotypic differences in species' responses to flooding/elevation, salinity, nutrient enrichment or other global environmental changes would be helpful and would allow a more robust discussion of the potential implications of genotype-specific differences for ecosystem function and resilience (e.g., Lessmann et al. 1999; Proffitt et al. 2003, 2005; Mozdzer and Megonigal 2012).

Materials and methods: The paper is significantly lacking in important information on the experimental set-up and methodologies, on everything from plant collection, marsh organ construction and maintenance, and the specific measurements (as noted by section below).

Section 2.1: How were the plants collected from the field? Were they intact sods of soil and vegetation? Were they rinsed of site soils before planting? How were they planted and grown in the trays (under what hydro-edaphic conditions, temperature, light availability, density, etc.)? How was plant size determined and standardized across treatments for use in the study (or randomized if standardization not possible)? Although there were some measures of change to account for potential initial differences, additional discussion of how plant size varied (or not) and what efforts were made to control for these differences is warranted; otherwise, subsequent biomass results could be skewed based on differences in initial weights of plants used in the study. What are the soils like at the field site and were they sieved to remove belowground biomass before being used in the pots?

Section 2.2: How were the mesocosms constructed and how did this affect the way in which water filled and drained the pots (were there holes in the bottom so that they filled and drained from below)? How were marsh organs oriented to control for shading or other effects? Were you limited to 3 flooding levels due to tidal tank size? What was the height difference among steps in the marsh organs and by how much was the marsh surface flooded for each of the treatments? What was the flooding range relative to the mesocosm position; were all pots fully drained at "low tide" or not? How do the flooding treatments compare to the elevations and flooding ranges in the field? Did the flooding treatments encompass the current marsh elevation/flooding gradient or was the study designed to simulate increased flooding as expected with SLR? How did the salinity regime compare to those at the field site? What it the typical growing season for these plants (is 12-weeks a reasonable study length given this marsh's latitude)?

Section 2.3: Were there any hydro-edaphic variables measured? These could confirm treatment effects and help explain observed differences among flooding treatments. Was there any evidence that the plants were nutrient-limited? Did they become "root-bound" over the course of the study?

Results & Discussion: One of the main points made is that flooding leads to shifts in biomass allocation from below- to aboveground for the low-marsh, but not the high-marsh, genotype, but the data presented do not explicitly demonstrate shifts in allocation along the flooding gradient. Why not calculate the root:shoot for both genotypes to test this explicitly?

In the introduction, the authors note different mechanisms of plant-mediated feedbacks to elevation – sediment trapping aboveground and contributions to soil volume belowground. Some discussion of this in light of the results would strengthen the paper. For instance, what are the implications of declining aboveground biomass (for both genotypes) with increased flooding for marsh resilience to SLR? How reliant on sediment accretion are these marshes, and to what extent would reduced sediment trapping capacity be expected to reduce resilience? What about the relative importance of reduced belowground inputs to soil volume in these marshes?

Regarding the conclusion that there is potential for the low-marsh genotype to invade

lower elevations, it would also be worth discussing its adaptability to SLR and its potential to displace the high-marsh genotype as water levels rise. Given that, what are the implications for marsh resilience?

Some additional technical corrections are provided below:

Line 19: "with "increasing flooding frequency."

Lines 37-38: "and often depends on"

Line 52: "if SLR-induced shifts . . . composition also are"

Line 122: introduce LM and HM abbreviations earlier

Line 126: "remained constant"

Line 165: "parameters with increasing flooding frequency."

Lines 172, 175: italicize scientific names

Line 182: "responded similarly and decreased with increasing flooding frequency"

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