RC4

Dear editor,

Thank you for inviting me to review this interesting research that aims to understand the relative roles of biotic and abiotic controls on tidal channel network formation in a controlled, scaled, laboratory experimental setting. A total of four experiments were conducted at the state-of-the-art Metronome facility. These experiments included two unvegetated controls and two experiments featuring vegetation with different colonization strategies (patchy = random and hydrochorous = flow-driven). The resulting experimental tidal channel networks were analyzed using various metrics to examine the development of vegetation patterns and channel networks, leading to the conclusion that channel network development is dominated by hydrodynamics near the sea and is more strongly influenced by vegetation moving landward. The paper is well-organized and easy to follow, with excellent figures. The introduction section is particularly commendable for its clarity and conciseness. The results are novel and convincing for the most part (see comment below though), and overall of broad interest to the scientific community.

I can surely recommend publication, although I would first like the authors to make some clarifications and revisions. Below are my detailed comments:

Are the experiments reported here representative of tidal networks cutting through salt marshes or, more broadly, large tidal embayments? It seems they might be more representative of networks developed at the scale of entire tidal embayments (e.g., a back-barrier lagoon). The introduction supports the former, but upon reading the article and examining the figures, the latter seems more accurate.

Reply: The tidal channel networks obtained in our experiments indeed do not fully resemble the more sinuous channels typically found in a salt marsh. We were limited by the dimensions of the tidal flume and the fact that the tilting of the flume led to a tidal flow with a dominant single direction similar to the direction of the tilting. As the reviewer has noticed, this had some consequences on the morphology of the channel systems, making them not perfectly comparable to natural systems. The channel systems we obtained might be more representative of a smaller portion of a salt marsh (e.g., as illustrated in the picture below for a salt marsh in the Scheldt estuary, SW Netherlands (51° 21' 19.25" N, 4°10'11.44" E)). Given the vegetation patch sizes, we do not see the experiments as representative of entire shallow tidal embayments. However, an analysis of the geometric similarities between these larger systems and the salt marsh systems of interest here is something we intend to conduct later.

This figure shows the Drowned land of Saeftinghe, a salt marsh located along the Western Scheldt in SW Netherlands (51° 21' 19.25" N, 4°10'11.44" E)

Similar to experiments by Stefanon et al. (2010, 2012), the width of the tidal inlet is fixed a priori and kept constant throughout the entire experiment. This design has advantages, such as avoiding interference with flume walls and related boundary effects. However, it may prevent the system from fully adjusting morphodynamically throughout the experiment, potentially causing over-deepening at the inlet. Please discuss this point adequately.

Reply: We agree that this is an interesting point. We will discuss it in the revised manuscript as follows. We decided to use fixed barriers to avoid interference with the flume walls and to create a branching network similar to the ones obtained by Kleinhans et al. (2012). We did indeed observe some scouring around the barriers at the inlet, which is not ideal, but this seems not to have prevented the channel systems from fully developing towards a morphodynamic equilibrium throughout the experiments. Instead, we cannot adequately represent littoral processes and expect that the fixation of the inlet allowed for full adjustment of the channel systems inside the tidal basin, while unprotected inlets would continue to erode due to lack of littoral processes.

Considering the above point, since waves were generated at the open sea boundary, why not allow barrier islands and tidal inlets to evolve naturally, adjusting morphodynamically during the experiments? Previous experiments on river deltas with waves and tides have shown that the dynamics of barrier islands and inlets can be replicated even at smaller scales than in the present experiments. See for example:

Baumgardner, S.E. Quantifying Galloway: Fluvial, Tidal and Wave Influence on Experimental and Field Deltas [Ph.D. Thesis], University of Minnesota: 113 p, 2015. https://conservancy.umn.edu/handle/11299/183395

Waves are rarely discussed in the text and are not mentioned in the discussion at all. Given the fixed inlet, it's worth considering the purpose of including waves, especially since they aren't discussed. Please clarify.

Reply: We will discuss this briefly in the manuscript. The monochromatic waves impacted the initial system development and the speed of the system development (i.e., if we had turned the waves off, the systems would have developed slower). The combined action of waves and tidal currents led to the mobilization of sediments and rounding of the ebb-delta. More information on the testing and analysis of the scaling of waves can be found in the supplement of Leuven et al. (2018). However, our wave-related sediment mobility is much lower than in Baumgardner's experiments because we used sand rather than crushed nutshell.

The differences between control experiments and experiments with vegetation appear to be less pronounced than initially hypothesized and traditionally suggested in the literature. I'd like the author to comment on this. My impression (which might be entirely wrong) is that the differences are, in fact, so small that they could be due to the stochastic nature of the experimental results rather than a real genuine effect of vegetation. In other words, these differences might be noise rather than a signal, and averaging results over several repetitions of the same experimental run might amplify the results even further than they already are. (Please note that I AM NOT recommending running the experiment multiple times. I understand the effort required to conduct experiments at this scale, and reproducibility is challenging due to time and resource constraints).

Reply: We understand the reviewer's point of view as the differences are not pronounced. Before our experiments, we ran 12 pilot tests (all bare/without inclusion of vegetation) to test which settings we wanted to use. During these pilot tests, we saw that similar systems started to develop. Once we included vegetation, we started seeing differences in system development (in particular, the development of a more extended main channel). Therefore, we argue that these differences are not attributed to noise. We will provide more information on the pilots in the paper/supplement.

Furthermore, we are contemplating using the figure below to highlight global trends in the zonation of processes. Instead of small zones of 1 m by 3 m, we would show larger zones (2 m by 3 m) to level out local variations.

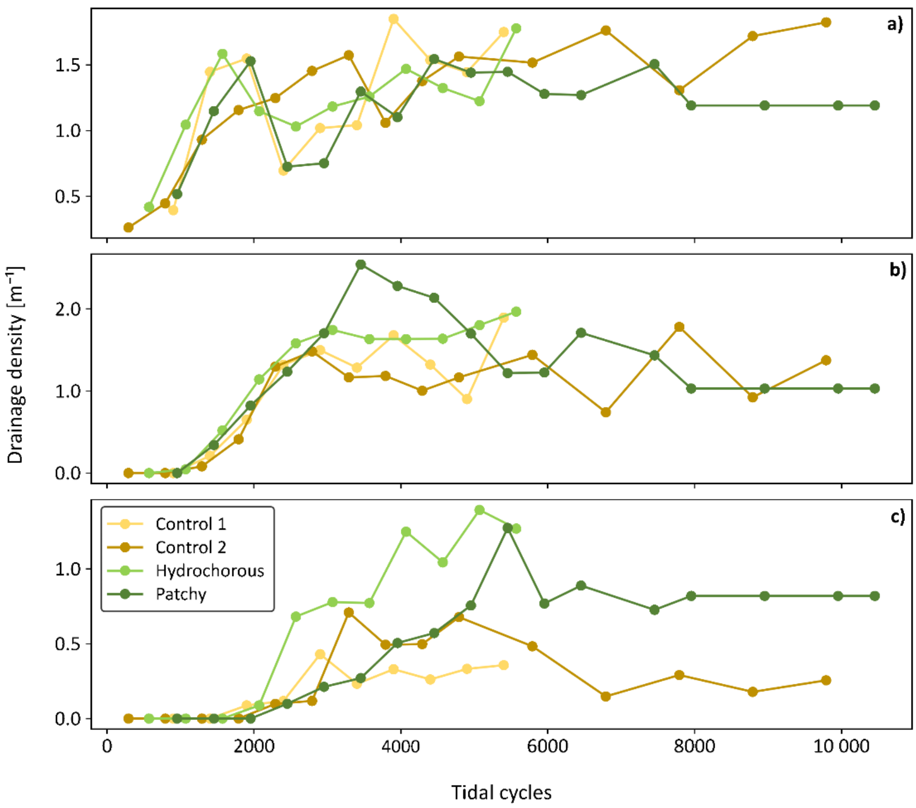

Figure: Local drainage densities shown over zones of 2 m (length) by 3 m (wide): a) is the most seaward zone (0–2 m), b) is a more landward zone (2–4 m) and c) is the most landward zone (4-6 m)

Line 395: "A slight reduction in tidal prism" raises questions about its feasibility. Since the width of the inlet is fixed, reducing tidal prism would require decreasing depth. I suspect that the inlet can hardly become shallower during the course of an experiment; in fact, it likely tends to deepen continuously until equilibrium depth is reached. Please provide clarification.

Reply: There was indeed some scouring around the barriers. However, the tidal prism can, in principle, be reduced by the import of sediment from the ebb delta to shallow the inlet on average, unlike in the aforementioned experiments of Stefanon, or by increased flow friction within the tidal basin. The results of the second control and the hydrochorous experiment indicated that the tidal prism stabilized and slightly reduced over time. The stabilization/slight reduction may be related to the ebb-delta that kept growing in height and width over time and reduced the volume of water entering the system.

Line 420: "The more spatially homogeneous and hence weaker hydrodynamics in bare systems may be responsible for the lower degree of channelization." This holds true because we are examining the landward site of the basin, where hydrodynamics are weaker regardless of vegetation. Bare, unvegetated parts of a tidal basin typically experience higher hydrodynamic stresses (waves+tides). For instance, tidal networks develop on bare mudflats where hydrodynamics are dominated by sheet flow, with inertia playing a comparatively more significant role than in vegetated salt marshes.

Reply: We will support our sentence with references to several studies showing that tidal channel drainage densities are usually lower on bare mudflats than on adjacent vegetated marshes (e.g., Kearney & Fagherazzi, 2016; Vandenbruwaene et al., 2013). Field studies, such as of Vandenbruwaene et al. (2015), have shown this is related to more spatially homogeneous sheet flow conditions on bare mudflats (because of more spatially homogeneous bed friction), while in vegetated marshes, spatial heterogeneity in vegetation-induced friction promotes flow concentration towards bare channels inside vegetated marshes.

Line 445: "In conclusion, our results suggest a zonal domination of abiotic processes at the seaward side of intertidal basins with high hydrodynamic energy levels. In contrast, biotic processes dominate system development more toward the landward side with intermediate hydrodynamic energy levels." This conclusion holds for a large-scale tidal basin, such as the one reproduced by the model (see to comment n.1). However, I am curious whether these results can be downscaled to study tidal networks cutting through individual marsh islands, such as those shown in Figure 1, where energy levels are consistently low, and there is no tidal inlet, fixed or freely evolving (again, see reloated comment n.1)

Reply: We agree that our results are representative of tidal systems with an overall landward gradient of decreasing tidal hydrodynamic forces, such as in back-barrier tidal basins with a seaward tidal inlet and a seaward sloping topography or wide open-coast tidal flats/marshes with an overall seaward sloping topography. For smaller-scale individual marsh islands and more complex topographies, gradients in tidal hydrodynamic forces will likely show a more complex spatial pattern. The situation can be more complex than demonstrated in our experiments. We will add this point to the discussion of our revised manuscript.

Minor: The text consistently uses the number of tidal cycles as a proxy for time, which is generally acceptable, given that it is known that the tidal period T= 40 seconds. However, including references to the actual duration of the experiments at various points in the text would improve clarity, eliminating the need for readers to search for T value and calculate the actual time duration.

Reply: We will include this in the text.

Minor: a relevant reference to recent experimental work on tidal channel network formation is missing:

Geng, L., Gong, Z., Zhou, Z., Lanzoni, S., and D'Alpaos, A. (2020) "Assessing the relative contributions of the flood tide and the ebb tide to tidal channel network dynamics." Earth Surf. Process. Landforms, 45: 237–250.

Reply: This is useful and we will add the reference and discuss their results.

Minor (but important): I support the comments from other reviewers suggesting the inclusion of movies of the experiments in the supplemental material to provide a better understanding of the experimental runs. Furthermore, making the experimental data freely accessible by placing them in a public repository with a DOI would be highly beneficial for the whole community.

Reply: We will include some movies of the experiments in the supplementary material and provide the data online.