eDear editor and reviewers,

We thank you for your time and constructive comments/suggestions on our manuscript. In the following, you will find our replies to your remarks and the corrected text passages. The reviewer reports are copied below in black, and our responses are shown in blue.

Kind regards,

Sarah Hautekiet

RC 1

Dear Editor and Associate Editor,

the paper describes a large-scale experiment on vegetation effects on the evolution of a tidal network. The experiments were carried out in the Metronome at the Utrecht University. Vegetation seeds were sown by dispersing them or manually creating some patches during the evolution of the system. The experiments were then compared to control experiments where no vegetation was used. Pictures of the tidal system were taken at different stages of the experiments to monitor vegetation growth and the channel geometric properties (with and without vegetation). In the experiments, vegetation favoured the development of a denser and more efficient network.

The issue is timely and of broad interest, particularly in view of the current great concern about the fate of coastal environments threatened as they are by climate change. The paper is well-written and well-conceived, but it would benefit from some minor clarifications and corrections. Thus, I recommend publication with minor revisions as the manuscript meets the kind of broad interest commanded by the readership of your Journal.

Please read below my comments (italics refer to the text of the manuscript).

Questions

- What was the duration of each experiment (days) and what would it correspond to in a real tidal system (years)?

Reply: We clarified this in the paper by adding the following sentences (lines 141-146):

"The experiments were run for 5000 (first control experiment), 5500 (hydrochorous vegetation experiment), and 10 000 tidal cycles (second control and patchy vegetation experiment). These numbers correspond to about 7, 8, and 14 years of natural tidal cycles in case of a semi-diurnal tidal regime. As every tidal cycle in the Metronome takes 40 seconds, the experiments took us 5 and 10 days to run for the first and second control experiment, respectively. The vegetated experiments took longer to run as we had to wait for four days between sowing events for seeds to germinate and grow sprouts. They both took around one month and a half (± 50 days) to run."

- How did you ensure that the morphodynamic equilibrium was reached? Did you use a local or global criterion? The average difference between two successive DSM was computed and, if the difference was smaller than a certain threshold, the morphodynamic equilibrium was reached? Did the vegetation encroachment increase the speed at which the system reached the morphodynamic equilibrium? Could this trend be in line with previous field investigations in tidal environments?

Reply: We ensured that the morphodynamic equilibrium was reached by quantification of global system properties such as rate of change in eroded sediment volume and rate of channel migration (e.g., Figs 9 & 10 and supplementary Fig 6). In these graphs, we can see that around 5000 tidal cycles the rate of change of these system properties stabilizes, indicating that we have reached a "morphodynamic equilibrium". We added a brief explanation between brackets as clarification (lines 219-220).

"The first control experiment ended after 5000 tidal cycles when a morphodynamic equilibrium was reached (i.e. when the rate of change of system properties, such as eroded volume and channel migration, stabilized; Fig. 9, 10 and supplementary Fig. 6)."

The presence of vegetation did not affect the speed at which the systems reached a morphodynamic equilibrium. Considering that 5000 tidal cycles would correspond with 7 years in real semi-diurnal tidal systems, we can argue that this time scale over which morphodynamic equilibrium is reached, is not in line with previous field investigations (morphodynamic equilibrium is usually reached after multiple decades or even a century). Therefore, it seems that the morphodynamics in our experiments are developing ahead of the tidal timescale.

- What are the limitations of using this vegetation species for large-scale experiments on the morphodynamic evolution of a tidal network? Vegetation has many functions in a tidal environment, for example it increases friction and favours deposition by sediment trapping and organic production. Are all these functions potentially reproduced (or reproducible) in the experiments? Or is there a function that is more dominant than the others in the experiments?

Reply:

Previous research by Lokhorst et al. (2019) showed that this plant species could reproduce multiple functions, such as increased friction, flow retardation, and bank stabilization. Similar findings were found in Metronome experiments on estuaries using this species (Kleinhans et al., 2022; Weisscher et al., 2022). We addressed this by adding the following (line 171): "it shows eco-engineering traits similar to marsh vegetation such as increased friction and bank stabilization".

In our experiments on saltmarsh systems, increased friction is the dominant function as this plant occurs in a high density. We did not observe sediment trapping to the same extent as in nature and in the experiments by Weisscher et al., where suspended sediment with lower density was added. However, some sediment was trapped in some locations containing dense vegetation patches.

One of the limitations of using this species is that the seeds do not disperse over the whole tidal basin when they are spread hydrochorously. They only end up in areas close to channels. This might be one of the reasons why vegetation cover remained low during the hydrochorous experiment. Furthermore, using this species could be a disadvantage if you want to simulate plant establishment at locations that are higher in the intertidal zone. Other plant species that are transported in suspension could be used for this purpose (e.g., *Veronica beccabunga*). We added a sentence to touch upon this limitation in lines 194-195:

"The low vegetation cover could be related to the fact that *Lotus* seeds did not disperse over the whole tidal basin during sowing events since they are transported as bed load."

- Does the vegetation grow in a specific elevation range related to the tidal range? Does the vegetation biomass peak at a specific elevation?

Reply: The vegetation mainly grew above mean sea level, in the upper half of the intertidal zone of the tidal basin (see figure below which was added in the supplementary material). The following sentence was added to provide this information (lines 333-334):

"Overall, vegetation mainly grew above mean sea level, in the upper half of the intertidal zone (see Supplementary material – Fig. S7)."

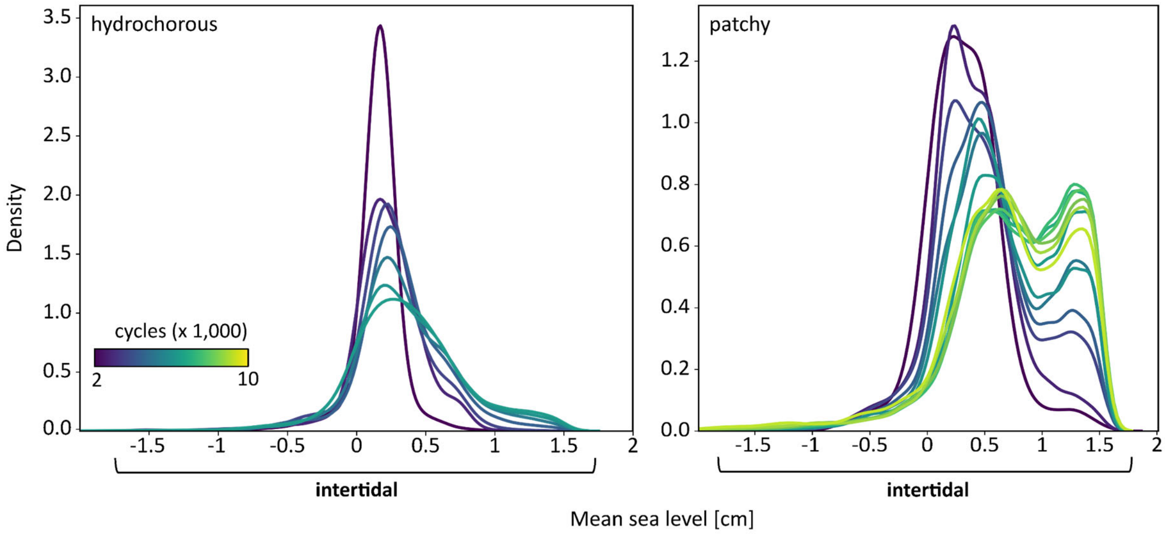

Figure S7: Vegetation density as function of mean sea level over time in the hydrochorous and patchy vegetation experiments.

Figures

Figures 4-11 and their labels look a bit blurred on my pdf file. This could be due to the pdf file compression but, anyway, please make sure that the overall resolution of each picture is high enough (usually 300 dpi).

Reply: This was solved in the revised manuscript.

Figure 1. On the top left, the panel labelling seems starting with "b)" and it gets a bit confusing when I look at the inset on the top right. I would replace label "b)" on the top left with "a)" and delete the labels in the inset on the top right leaving only the black square.

Reply: We changed this.

Figure 2. Could you add photographs where you see the channel network and the vegetation on the floodplains? I am thinking of photographs like those in Figure 5 in Weisscher et al., 2022. It helps to better visualise the experimental setup.

Reply: Thank you for your suggestion, we added a figure with photographs (Figure 6) which changed the figure numbering.

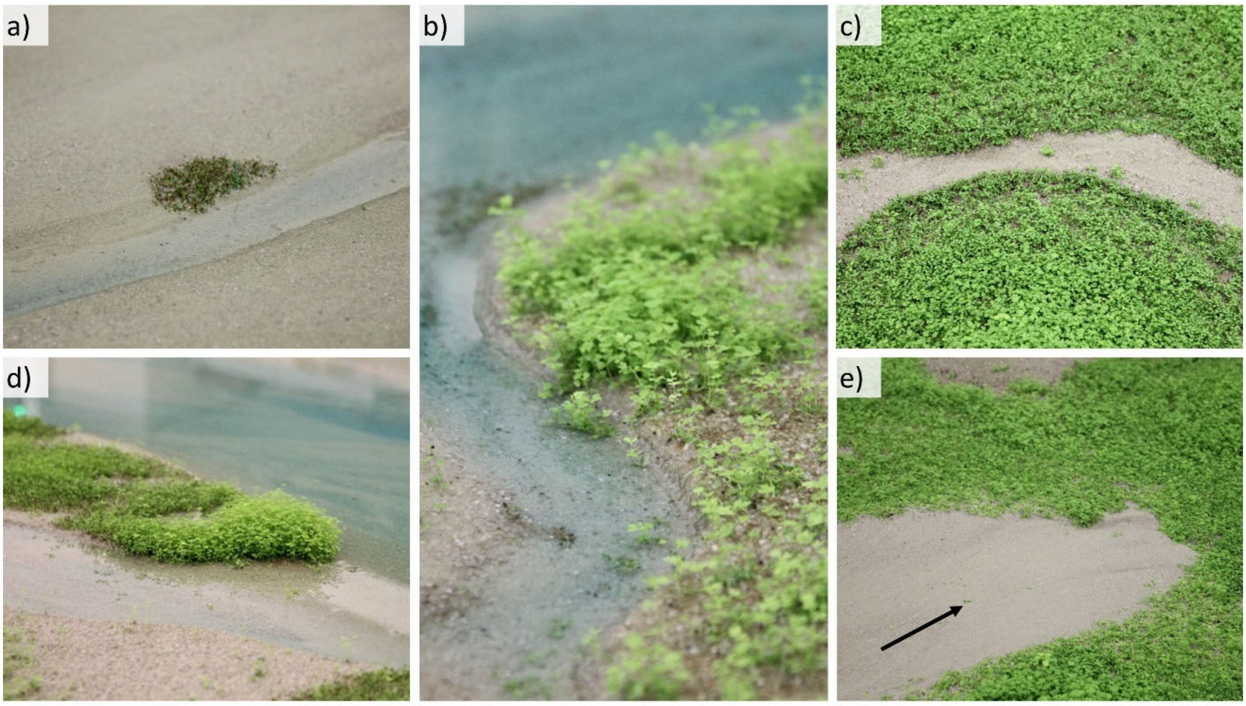

Figure 6: Photographs of vegetation-induced features in the experiments: (a) a germinating patch of seeds sown next to a channel, (b) a vegetated levee fringing along a channel, (c) channel initiation induced by vegetation, (d) channel bank stabilization by vegetation, (e) sediment trapping induced by blocking of tidal flow in front of vegetation (flood flow direction indicated by arrow)

Figures 3, 4, Sup_f01, Sup_f02, Sup_f03, Sup_f04, Sup_f05. Tick values on some vertical axes are missing.

Reply: We added the tick values.

Figure Sup_f05. In the panel referring to hydrochorus vegetation at 3071 cycles, the sea basin is represented with a darker blue. I think it is only a plotting issue, but it could be worth checking it out.

Reply: This is not a plotting issue, but a consequence of the flume not being completely drained . The darker blue color shows the remaining water in the “seafloor”. This is explained in the figure caption.

Figure 5. This is a very nice picture. It seems that you have a figure composed of only a single column. Is it done on purpose? If you stretch the horizontal axis as in Figure 8, does it get too distorted? Moreover, tick values on some vertical axes are missing. If you use panel labelling, you can refer to specific panels when describing the figure in Lines 256-269.

Reply: We chose to plot the figure in this format to avoid making it too distorted/too big, as we do not consider it a highlight in the paper. It is an example of all experiments' erosion and sedimentation maps. Since we could not discern big differences between the experiments, we show only one plot. The tick values on the vertical axes were added.

References

Lines 606, 625. “/a -n/a” appears in the references Temmerman et al. (2005) and Vandenbruwaene et al. (2011). Is that right?

Reply: We corrected this and have added the page numbers in these references.

Lined comments

- Line 146. “Based on Lokhorst et al. (2019), experience in the Weisscher et al. (2022) experiments and pilot experiments not reported here”. Is there a reference (paper or conference abstract) for the "pilot experiments not reported here" (are the pilot experiments mentioned in line 173?)? I would simplify the sentence as "Based on previous experiments (Lokhorst et al., 2019; Wesscher et al., 2022;, ...), a single plant species was ..."

  Reply: We added a sentence that mentions what we did in the pilot experiments (lines 146-148): “The selected experimental settings were based on 12 pilot experiments (all without inclusion of vegetation) in which settings such as sea level and tidal inlet width were varied until a dendritic channel network was created while avoiding interference with the flume walls.”

  We simplified the sentence as suggested (lines 166-167): “A single plant species was selected for the vegetation experiments based on previous experiments (Lokhorst et al., 2019; Weisscher et al., 2022) and pilot tests (see Supplementary material): *Lotus pedunculatus*”.

- Line 149. “alfalfa”. Plant species were written in italics in the manuscript. Please write alfalfa in italics.

  Reply: Alfalfa is not the Latin name of the plant species (*Medicago sativo*) and is therefore not written in italics.

- Line 150. “it does not establish in unsuitable locations”. Do you mean “grow”?

  Reply: We specifically use the verb “establish” as we focus on the seedlings' establishment phase and uprooted seedlings that can settle in other locations via the tidal flow. Plant growth was not observed in these experiments as the absence of nutrients prevents further plant development as seen in Lokhorst et al. (2019).

- Line 196. “… with a resolution of 4000 of 6000 pixels …”. Is it 4K or 6K?

  Reply: We changed this to “4000 by 6000 pixels”.

- Lines 198-199. “The images were calibrated for internal and external parameters (i.e., lens correction, geometric rectification) before they were stitched.”. Which calibration method did you use for correcting lens distortion? Checkerboard method by Zhang (1998) or another one? Could you add the reference for the method?
  Zhang, Z. (1998). A flexible new technique for camera calibration. Technical Report MSRTR-98-71 Microsoft.

  Reply: The lens distortion was corrected in the standard Matlab toolbox. A pinhole camera model was applied for calibration, which uses calibration techniques involving the checkerboard method. Afterwards the “undistortImage” function was used to correct the images for lens distortion. We added this explanation to the Supplementary material and to refer to it in line 230:

  “The images were calibrated for internal and external parameters (i.e., lens correction, geometric rectification) before they were stitched (see Supplementary material and Leuven et al., 2018).”

- Was the geometric rectification carried out using python built-in functions in OpenCV? If so, please add a reference of the python package (e.g., Bradsky, 2000).
  Bradski, G. (2000). The OpenCV Library. Dr. Dobb's Journal of Software Tools, 120; 122-125.

  Reply: The geometric rectification was carried out in Matlab by applying a projective transformation using the "fitgeotrans" function based on a correspondence between fixed (coordinates in real world) and moving points (corresponding points in images captured by the cameras). We added this explanation to the Supplementary material and we refer to it in line 230:

  "The images were calibrated for internal and external parameters (i.e., lens correction, geometric rectification) before they were stitched (see Supplementary material and Leuven et al., 2018)."

  Line 206. "Basic corrections were applied.". Please give a brief description of the basic corrections applied.

  Reply: The basic corrections applied are the following: a correction value was retrieved by calculating the median of the differences between two DSMs (one 'undisturbed' DSM and one DSM after the position of the laser-camera system changed). This value was then added to the elevation data. This was added in the methods description of the revised manuscript (lines 237-239).

  "Specifically, a correction value was retrieved by calculating the median of the differences between two DSMs (one 'undisturbed' DSM and one DSM after the position of the laser-camera system changed). This value was then added to the elevation data. "

- Line 259. By introducing panel labelling, you could refer to specific panels in figure 5 when describing evolution phases.

  Reply: We followed your suggestion and added panel labelling to figure 5.

Minor typographic/grammatical corrections were made as indicated. Suggestions about the plots were implemented.

RC 2

I can definitely say I enjoyed reading the paper that is well written, well organized, concise but exhaustive in describing the methodology and in presenting and discussing the results.

Based on scaled flume experiments the study investigates the relative roles of abiotic and biotic processes in the development of tidal landscapes. Experiments are indeed novel as, for the first time (to my knowledge) they combine the analysis of the role of vegetation on the channel network formation and development in an intertidal basin context.

The experiments highlighted the role of vegetation in driving the development and evolution of channel networks investigating the differences associated with two different mechanisms of plant colonization (patchy and hydrochorous). Based on my personal experience I know the effort necessary to design, set up, and carry out physical experiments which made me further appreciate the study. I particularly appreciated the interesting way of concluding the introduction saying what the Authors expect (hypothesize) from their experiments.

Globally speaking I can say I recommend the manuscript for publication in Earth Surface Dynamics after minor revision. I provide the following few suggestions that are mostly issues of clarification and comments aimed, I hope, at further improving the readability and the quality of an already very good paper. No need to stay anonymous I am Luca Carniello.

SPECIFIC POINTS

Line 45: “Such channels are ebb-dominant in the unvegetated state, …” I suggest supporting this statement in some way. Being in the introduction the best option is to add some references.

Reply: We added some references as suggested (lines 49-50): (Kleinhans et al., 2009; D’Alpaos et al., 2005; Mariotti and Fagherazzi, 2011).

Line 60: “history traits, such as plant recruitment strategies, can influence” I suggest adding two commas.

Reply: Done.

Line 160: “The first 10 tidal cycles were necessary to wet the tidal basin and re-establish a normal flow pattern.” Why is this necessary? Did you stop the experiment before each sowing event? I suppose not as at the beginning you state "During the experiment, Lotus seeds were dropped..." so, why do you need to re-establish the flow pattern? please clarify.

Reply: We realize we should have explained clearly that the flume was drained before every sowing event, which required some tidal cycles to re-establish the tidal flow pattern. A clarification was added in the revised manuscript using the following sentences (lines 184-186):

“Before every sowing event, the flume was drained, as acquiring digital surface models requires a dry sand bed. Afterwards, the first ten tidal cycles were run without adding seeds to re-establish a normal tidal flow pattern.”

Line 165: “Around 160 000 seeds (i.e., 200 g) were supplied per sowing event to obtain a vegetation cover equaling about half the tidal basin at the end of the experiment.” Please explain why 160000 seeds are necessary to cover half of the basin.

Reply: We discussed the sowing procedure in more detail in the Supplementary material and refer to it in lines 177-178: "More information about the sowing procedure can be found in the Supplementary material."

In the Supplementary material, it reads then:

"The number of seeds supplied to the tidal inlet was based on the vegetation protocol used in Weisscher et al. (2022). The authors of this study aimed to obtain vegetation covering half the Metronome flume by supplying 400 000 seeds per sowing event (160 000 seeds at the seaward tidal inlet – 240 000 seeds at the landward river inlet). Since, in our case, only one-half of the flume was used and no landward river inlet was present, but instead a closed landward boundary, we decided to use the same quantity supplied at the tidal inlet in Weisscher et al. (2022). "

Line 170: "while tilting of the flume was halted." How long was the tilting halted? For the Hydrochloric sowing experiment, you say the experiment was stopped for 4 days. What happened when adopting the patchy sowing procedure? This important piece of information is actually missing.

Reply: The flume was also halted for four days in the patchy experiment. We made changes in the text to clarify this missing information (lines 207-208):

"Similar to the hydrochorous experiment, the flume was stopped for four days to allow germination and development of the sprouts."

Line 190: Can you explain the rationale that suggested you to run longer only one of the experiments with vegetation to check if the morphodynamic equilibrium was indeed reached? Why did you choose the patchy one? This is just a curiosity.

Reply: We ran the patchy experiment after the hydrochorous experiment, because we wanted to see first what plant density developed in the hydrochorous experiment, in order to decide then what plant density (similar density) we wanted to obtain in the patchy experiment. Since it was not feasible to run both experiments for 10 000 tidal cycles (too time consuming), we decided only to run the last experiment for a more extended period.

Line 201: "First, the raw laser line scanner data underwent a calibration and correction process for the laser-camera system." Can you specify a little bit more in detail what the calibration and correction process consists of?

Reply: The lens distortion was corrected in the standard Matlab toolbox. A pinhole camera model was applied for calibration, which uses calibration techniques involving the checkerboard method. Afterwards the "undistortImage" function was used to correct the images for lens distortion. We added this explanation to the Supplementary material and to refer to it in line 230:

"The images were calibrated for internal and external parameters (i.e., lens correction, geometric rectification) before they were stitched (see Supplementary material and Leuven et al., 2018)."

Line 201: "If the difference between the window median and the local pixel elevation was below a certain threshold (respectively, 0.0015, 0.005 and 0.0055 m) for at least one window size, the local pixel was identified as a channel." I do not understand why this occurrence can ensure the selected pixel is in a channel. I suppose that the difference between the window median and the local pixel elevation can be below a certain threshold also for pixels pertaining to the adjacent flat areas. Can you clarify, please?

Reply: We discussed the channel network extraction procedure in more detail (lines 258-262).

"A raw channel network skeleton was retrieved containing some "erroneous" skeleton sections that did not connect to the channel network. This skeleton was cleaned in the next step by applying a threshold ratio (i.e., skeleton section length divided by the distance between the downstream node and the remaining skeleton). If a skeleton section length is below this threshold (1.7 in this case), it is removed."

Line 286: "we observed that the left channel bend (as seen from vertical top-view)" I guess this is to explain to the reader what you mean by "left" channel bend but it is not clear to me what vertical means. I suggest defining left and right for example assuming an observer looking the experiment from the inlet landward.

Reply: Thank you for your suggestion. We changed this (line 326) to "assuming an observer looks from the seaward inlet into the landward direction".

Line 337: I suggest remembering here that "DL" is the local drainage densities. It has been defined quite far above and I personally forgot.

Reply: We have adjusted this in the text.

Line 380: "In the hydrochorous seeding experiment, the vegetation cover increased slowly over time and remained lower than in the patchy seeding experiment." It would be very interesting the investigate the effect of increasing the amount of seeds supplied per sowing event performing other hydrochorous seeding experiments. This is of course not a request of integration for this contribution but a suggestion for a further paper.

Reply: Thank you for your suggestion. Another hydrochorous seeding experiment as part of the Msc thesis of Thomas Veerman has been carried out with a higher amount of seeds, while using two more plant species (*Veronica beccabunga* and *Medicago sativa*). The final vegetation cover in this multispecies experiment also stayed very low (20%) even though double to triple the amount of seeds were supplied. However, we do not have sufficient control over the effects of the different species so we chose not to include this experiment in the paper.

RC3

Overview: This study focuses on understanding biotic vs abiotic controls on tidal marsh development using four experiments. They conducted two unvegetated controls and two vegetated experiments, one with vegetation colonization by water and the other by air, with the goal of examining how these different experiments evolve channel networks. The authors step through a variety of metrics to examine the development of vegetation patterns and channel networks, concluding that channel network development is dominated by hydrodynamics near the sea and more strongly influenced by vegetation as you move landward. The paper is well-organized and easy to follow with nice figures. Below I outline a number of suggestions to improve the paper, which I believe constitutes minor revisions.

Comments:

The background is notably missing some references of previous vegetated experiments. Though tidal experiments with plants were not necessarily performed, there is a broad body of literature that informs the results found here, including experiments of deltas with vegetation (e.g., Piliouras et al., 2017, Piliouras and Kim 2018, 2019) and experiments at a smaller scale examining flow through and around patches of vegetation (e.g., Chen et al., 2012; Meire et al., 2014; Ortiz et al., 2013; Zong and Nepf 2010, 2011, 2012). These papers provide context to the results in discussing many of the processes at play, such as how flow is steered around patches of vegetation, how patches may encourage bifurcation to increase channel density, and how vegetation influences topographic change. I suggest including a paragraph in the background to elaborate on what we know about vegetation-flow-sediment feedbacks and drawing on this knowledge in the discussion, as well.

Reply: In the third paragraph of the introduction of the original paper, a brief summary was provided of previous real-scale experiments on the small scale of flow through and around vegetation patches. There, we added the references suggested here by the reviewer (line 60):

"Vegetation slows down flow velocity locally within patches (Zong and Nepf, 2010, 2011, 2012; Ortiz et al., 2013; Meire et al., 2014)"….

Further, in the fifth paragraph of the intro, a summary was provided of previous scaled flume experiments on tidal landscape morphodynamics and the role of vegetation. There, we added a reference to the suggested studies (lines 85-86):

"In general, scaled laboratory experiments have been used to study the formation and evolution of fluvial and tidal landscapes with live vegetation in a simplified setting (Kleinhans et al., 2015a; Tal and Paola, 2007; Piliouras et al., 2017; Piliouras and Kim, 2019a, b; Zhou et al., 2014). "

I would appreciate more information on the selection of this vegetation species, as it is not widely used in the literature for morphodynamics experiments, in addition to more information on the vegetation protocols. Can you provide some context on plant traits and impact on sediment transport? L165: You state 160,000 seeds to obtain coverage of half of an area – with what intended density? Is this the same density as in the other vegetated experiment? How was this density chosen? What impact might this have on your results? All of this should be justified in the manuscript. Finally, how was the timing of sowing determined? You've got 1000 cycles unvegetated, 10 cycles for wetting (is it not already wet?), 25 cycles to release, 25 cycles to spread, 500 cycles of just water/sediment transport, then more sowing – how were all of these things determined? You describe the experiments as scaled – are these numbers scaled to some timing of seed dispersal in a natural system? How was the number of patches and patch size determined for the manual sowing? Please address these issues in the manuscript text.

Reply: We discussed the sowing procedure in more detail in the supplementary material. We explained it as indicated below.

For our plant species selection, we relied on (1) the work from Lokhorst et al. (2019), in which the growth rate, hydraulic resistance, and bank strength of 19 different plant species were tested in Metronome-like tidal flume conditions, (2) previous experience/research done in the Metronome (e.g., Weisscher et al., 2022) and (3) observations from pilot tests.

(1) The fast growth rate *of Lotus pedunculatus* in the applied timescale (four days) is one of the main reasons it was selected. Furthermore, the results of Lokhorst et al. (2019) indicate that *Lotus* can represent grass-like species (like those found in natural marshes) in scaled landscape experiments as it can trap sediments and grow in dense patches with high flow resistance.

(2) The results from Weisscher et al. (2022) and Kleinhans et al. (2022) suggest that vegetation (including this species) could stabilize channel banks and influence the morphological development of (partially-filled) estuaries.

(3) Pilot tests were conducted to select a plant species that could rapidly germinate in both vegetated experiments where different sowing procedures were applied. Since previous experiments only relied on seed dispersal via the tidal flow, we had to examine whether seed viability would not suffer from the sowing procedure used in the patchy experiment. This was done by a simple test in which *Lotus* and *Veronica beccabunga* seeds, subjected to the procedures used in the experiments (i.e., soaking seeds for 24 hours and soaking seeds for 24 hours followed by drying them the next day), were deposited in buckets filled with poorly sorted sand. Afterwards, the seeds were watered daily and the fraction of germinated seeds along with germination time was monitored. We observed a very low fraction of germinated *Veronica* seeds in both procedures, while the *Lotus* seeds showed more promising results. Approximately 50% of the seeds germinated and we could not discern major differences in germination between the different procedures.

The number of seeds supplied to the tidal inlet was based on the vegetation protocol used in Weisscher et al. (2022). The authors of this study aimed to obtain vegetation covering half the Metronome flume by supplying 400 000 seeds per sowing event (160 000 seeds at the seaward tidal inlet – 240 000 seeds at the landward river inlet). Since, in our case, only one-half of the flume was used and no landward river inlet was present, but instead a closed landward boundary, we decided to use the same quantity supplied at the tidal inlet in Weisscher et al. (2022). The plant density obtained within vegetation patches in the hydrochorous experiment (ca. five plants/cm²) was subsequently used to decide what plant density we wanted to achieve in the patchy experiment, i.e. how many seeds were sown in the pre-defined patches.

The timing of the sowing events was also based on previous experiments conducted in the Metronome (Kleinhans et al., 2022; Weisscher et al., 2022). We initiated sowing events after 1000 tidal cycles, as channel network formation had started by then but had not reached equilibrium yet, hence trying to prevent fixation of the network by establishing vegetation. Every sowing event occurred with an interval of 440 (hydrochorous vegetation experiment) or 500 tidal cycles (patchy vegetation experiment). Each sowing event started by acquiring a DSM. Therefore, we had to drain the flume and refill it afterwards. Hence, every sowing event of the hydrochorous experiment required a short wetting phase (during 10 tidal cycles) to re-establish a normal flow pattern, followed by the release of seeds (during 25 tidal cycles) and dispersal of seeds (during another 25 tidal cycles). After a four-day germination period, a new 440 tidal cycle interval was run to obtain a total of 1500 tidal cycles since the start of the experiment. At this point, the procedure was repeated until 5000 tidal cycles were run, starting with a new DSM acquisition and a new sowing event (during 60 tidal cycles), followed by 440 tidal cycles. Etcetera.

In the patchy experiment, the number of patches and their sizes were determined as such that 60% of the tidal basin would be covered by vegetation patches after 5000 tidal cycles, with a first sowing event after 1000 tidal cycles followed by subsequent sowing events every 500 tidal cycles, i.e., totalling eight sowing events after 4500 tidal cycles. Apart from that, we wanted to ensure that the patch size and number would create sufficient interaction between vegetation patches and tidal flow. Moreover, the decision had to be practically feasible since all patches were manually sown (which required a lot of work).

L193: How did you determine if equilibrium was reached?

Reply: We ensured that the morphodynamic equilibrium was reached by quantification of global system properties such as rate of change in eroded sediment volume and rate of channel migration (e.g., Figs 9 & 10 and supplementary Fig 6). In these graphs, we can see that around 5000 tidal cycles the rate of change of these system properties stabilizes, indicating that we have reached a "morphodynamic equilibrium". We added a brief explanation between brackets as clarification (lines 219-220).

"The first control experiment ended after 5000 tidal cycles when a morphodynamic equilibrium was reached (i.e. when the rate of change of system properties, such as eroded volume and channel migration, stabilized; Fig. 9, 10 and supplementary Fig. 6)."

Section 3.2: It seems from your methods that you are intentionally not sowing seeds in channels and not re-sowing if channels eroded through parts of patches. It's worth a sentence or two in this section to explain that in the context of your results because you are pointing out things like channel bends having the lowest vegetation cover, but this is not surprising given your methods that intentionally do not sow in channels.

Reply: We added a short explanation about this in the manuscript (lines 204-205):

"To resemble natural processes, we did not re-sow in areas where (part of) a patch disappeared or where a channel developed (see Fig. 6a). Neither were new patches sown in channels."

Figure 9 and other similar figures: It would help to differentiate the runs a bit more by changing line style and marker shape, in addition to color. This will likely also help folks who have difficulty discerning color.

Reply: We changed the line style and marker shape of the vegetated experiments in the figures.

I strongly suggest including movies of the experiments in supplemental. The results section is quite difficult to follow without having the movie available to the reader, especially when you talk about specific channels developing in specific runs and how that affected various metrics.

Reply: Thank you for your suggestion, we included time-lapses of the experiments in the supplementary material.

386-388: though they were not tidal experiments, this result is quite similar to that found by Piliouras et al., 2017, that rapid vegetation colonization can inhibit channel network development. Their later papers also found similar results about plant patches encouraging channelization and bifurcation around them. I suggest including these citations, as described in more detail above.

Reply: We followed your suggestion and added some text (lines 431-432):

"This finding is quite similar to previous non-tidal experimental results, suggesting that rapid vegetation colonization can inhibit channel network development (Piliouras et al., 2017; Tal and Paola, 2007) and it adds new insights compared to previous studies (Schwarz et al., 2018, 2022)."

Figure 12: what are the gray shaded regions?

Reply: We added an explanation to the figure caption: "The grey shaded regions represent zones varying in levels of tidal prism in which we see the highest drainage density for bare and vegetated landscapes."

Regarding the phrasing of your conclusion that vegetation 'dominates' in the landward zone – does it really dominate? Or does it just have additional influence here? It seems that channel development is still a physical process that is initiated by abiotic factors, but that the initiation and evolution of the network is more strongly controlled by the combination of biotic and abiotic factors in the landward region, compared to a real dominance of abiotic factors near the coast. I suggest rewording this.

Reply: This sentence was reworded as suggested (lines 492-495):

"Biotic factors (i.e., vegetation colonization) start having an additional effect in more landward zones with an intermediate level of hydrodynamic energy as they allow plant-flow interaction. The vegetation patches lead to concentrated flow around vegetation patches, promoting channel development and a higher channel network efficiency."

Data are not accessible. Please place data in a repository with DOI.

Reply: The data have been placed in a DOI repository.

References:

Meire, D., Kondziolka, J. M., & Nepf, H. M. (2014). Interaction between neighboring vegetation patches: Impact on flow and deposition. Water Resources Research, doi: 10.1002/2013WR015070

Ortiz, A. C., Ashton, A., & Nepf, H. (2013). Mean and turbulent velocity fields near rigid and flexible plants and the implications for deposition. Journal of Geophysical Research: Earth Surface, doi: 10.1002/2013JF002858

Piliouras, A. and W. Kim. (2019). Upstream and downstream boundary conditions control the physical and biological development of river deltas. Geophysical Research Letters. doi: 10.1029/2019GL084045

Piliouras, A. and W. Kim. (2018). Delta size and plant patchiness as controls on channel network organization in experimental deltas. Earth Surface Processes and Landforms. doi: 10.1002/esp.4492.

Piliouras, A., W. Kim, and B. Carlson. (2017). Balancing aggradation and progradation on a vegetated delta: The importance of fluctuating discharge in depositional systems. Journal of Geophysical Research – Earth Surface. doi: 10.1002/2017JF004378.

Zong, L. J., & Nepf, H. (2010). Flow and deposition in and around a finite patch of vegetation. Geomorphology, doi: 10.1016/j.geomorph.2009.11.020

Zong, L. J., & Nepf, H. (2011). Spatial distribution of deposition within a patch of vegetation. Water Resources Research, doi: 10.1029/2010WR009516

Zong, L. J., & Nepf, H. (2012). Vortex development behind a finite porous obstruction in a channel. Journal of Fluid Mechanics, doi: 10.1017/jfm.2011.479

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.

In this figure we can see 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 discussed it in the revised manuscript as follows (line 129-131).

“We decided to use fixed barriers to avoid interference with the flume walls and to create a branching channel network similar to the results 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 discussed this briefly in the revised manuscript (lines 155-157).

"These monochromatic waves impacted initial system development and the rate of channel development as sediments were mobilized by the combined action of waves and tidal currents."

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 provided more information on the pilots in the paper (lines 146-148):

"The selected experimental settings were based on 12 pilot experiments in which settings such as sea level and tidal inlet width were varied until a dendritic channel network was created while avoiding interference with the flume walls."

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 when the ebb delta grows in height and width, or by increased flow friction within the tidal basin, which can both reduce the volume of water entering the tidal basin. The results of the second control and the hydrochorous experiment indicated that the tidal prism stabilized and slightly reduced over time. We added the following sentence to clarify "the slight reduction in tidal prism" (lines 444-446):

"This stabilization or slight reduction in tidal prism might be related to the ebb-delta, which kept growing in height and width over the course of the experiments 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 supported our sentence with references to several studies showing that tidal channel drainage densities are usually lower on bare mudflats than on adjacent vegetated marshes and added the following (lines 468-471):

"…degree of channelization (e.g.,Temmerman et al., 2005; Vandenbruwaene et al., 2013, 2015; Kearney and Fagherazzi, 2016). Field studies (such as Vandenbruwaene et al., 2015) have shown that this is related to more spatially homogenous sheet flow conditions on bare mudflats because of more spatially homogenous bed friction. In vegetated marshes, spatial heterogeneity in vegetation-induced friction promotes flow concentration towards bare channels. "

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. In such cases, the situation will be indeed more complex than demonstrated in our experiments. We added this point to the discussion of our revised manuscript (lines 477-481):

"Our findings might have important implications for understanding the relative role of abiotic and biotic processes on channel network development in tidal systems with an overall landward gradient of decreasing tidal hydrodynamic forces (e.g., back-barrier tidal basins with a seaward tidal inlet, or tidal flats-marsh complexes with an overall seaward sloping topography). Smaller-scale individual marsh islands with more complex topographies will likely show a more complex spatial pattern in tidal hydrodynamic forces than our experiments."

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: Thank you for the suggestion. We included proxies for time at various points 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: We added the reference (lines 47-48):

"The evolution of these channels is typically driven by headward erosion (Kleinhans et al., 2012; D'Alpaos et al., 2005; Stefanon et al., 2010), which is mainly induced by ebb currents (Geng et al., 2020)."

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 included some time-lapses of the experiments in the supplementary material and placed them in a public repository with a DOI.