Summary, Impression, and Comments

This manuscript presented a series of analyses on two physical delta experiments to understand the effect of non-fluvial sedimentation (e.g., wetland accretion) on delta channel morphology and kinematics. Some of the interesting findings are that non-fluvial sedimentation led to the development of longer and deeper trunk channels and slope break of the channel bed (e.g., the presence of platform), more channeled flow in the distal delta region, less overbank flow, and a backwater reach that is more analogous to natural systems.

Overall, the manuscript is well-written and organized, and the figures are well-made. I also appreciate the thorough and detailed literature review in the introduction. As deltas worldwide are experiencing rapid land loss, the topics this manuscript explores are critical, especially given that the role of non-fluvial sedimentation remains a relatively less understood area in the broader discipline of delta geomorphology. This manuscript would be well-received by readers of ESurf and the general surface processes community. I have some comments that should be addressed before consideration of publications. I also want to acknowledge that I have read the comments from Reviewer 1.

My comments are about bolstering the discussions, clarifying the method sections, and improving the figures. I hope these suggestions will help improve the quality of the work for the readers. There are several interesting findings that the discussion either briefly mentioned or implicitly hinted at. Below are my recommendations:

1 Although part of the results, the compensation timescales are not used in the discussion. The compensation timescale is implicitly discussed as it relates to channel mobility and avulsion dynamics. I'd suggest elaborating on Tc. There are the same for both experiments, which is very interesting. See comments 3 and 4 for more details.

2 Previous works showed that hydrograph variability (e.g., Barefoot et al., 2021) could produce elongated deep channels. Also, bed material size finning (e.g., Nittrouer et al., 2012, Dong et al., 2016, 2019, Delorme et al., 2017) and loss in valley confinement (mentioned by manuscript at L309) can produce slope break in channel bed profile. The manuscript should emphasize that non-fluvial sedimentation is a new control of delta channel morphology and kinematics. This is an excellent opportunity to elaborate because many deltas have very fine bed material sizes (no downstream fining) and damped hydrographs due to damming yet maintain platform-like profiles. This is also the first time I have seen a bed slope break produced in a delta flume without variable discharge and grain size.

3 It is odd that basin-wide migration between the control and treatment are similar (Tmob, in paragraph L316), yet the experiments have different channel dynamics (Figure 7b-d). I agree with the explanation in section L316, but I'd recommend that the manuscript elaborate. My recommendation is to think about it in terms of timescale. Because the experiment is set up at equilibrium, i.e., sediment supply equals to the relative sea level rise (RLSR), at the longest timescale, it is not surprising that Tmob is similar because the long-term migration rate is sediment supply limited, while avulsion setup is RSLR limited. The manuscript writes about this at L325 implicitly. However, the temporal evolution of migration, or shorter timescale dynamic, has to be different between the two cases. This is shown in Figures 5 and B8: the cyclicity of backwater length and e-folding differ between the experiments. Please consider doing a power spectrum analysis of the two graphs' data. I also recommend using both dimensional and nondimensional parameters (normalizing the backwater length by the trunk channel width or mean shoreline, both are proxies for delta size). I also recommend that the manuscript writes about what short- and long-term channel dynamics mean for coastal restoration and stratigraphy.

4 It is also very odd that the elongated backwater length is not impacting the avulsion dynamics. This is related to comment 3. In the long term, the experiment setup limits the average avulsion timescale, i.e., sediment supply and RSLR. But in the shorter terms, the treatment has to be different; this is shown by the longer channels (Figure 4). Non-fluvial sedimentation adds mass to the floodplain and grows the levees, thus prolonging the avulsion setup (channels need to aggrade more space) while limiting overbank flow. This is similar to elongated backwater length due to lobe progradation (Ganti et al., 2014, Moodie et al., 2019, Sam Brook et al., 2022). The treatment case may be allowing lobe progradation. I'd recommend normalizing the x-axis in Figures 3, 4, 6, and 7 by mean shoreline length to show lobe progradation and backwater length to show the difference in the avulsion dynamics.

Line-by-line
L65 Patterns

L93 I'd suggest adding one to two sentences or a table to summarize boundary conditions quickly to make the manuscript more complete for the readers.

L96-99 I'd suggest moving these sentences to the end of the Introduction or earlier in the Method to emphasize the rationale of using physical experiments.

L106 The logic is that vegetation impacts hydrodynamics which then causes sediment deposition or erosion.

L117 Why is the treatment case scanned every 2 hours? Is it because marsh deposition occurs every 2 hours?

L118 Why is the experiment paused? Is this when a dry LiDAR scan occurs?

L119-125 I'd suggest simplifying this section into one to two sentences. For example, channels are mapped manually because uncertainty in hand-mapped and automatically tracked channels are similar (reference). In addition, shorelines are tracked via threshold holding (reference).

L128 Try "including" instead of "such as" Otherwise, are there more variables? Reference a table?

L145 I don't understand this. Are these areas calculated between two radial transects in the planforms, vertically, or over the entire delta?

L146 I'd suggest adding Channel Length (Lc)

L147 Is trunk width measured at where trunk depth is measured?

L158 Why 16%? It will be helpful for the readers to have a quick explanation.

L230 Any thoughts on why channel width narrows in treatment towards the shoreline? This is very interesting for several reasons. 1) width scales with lateral migration rate and lateral migration rate of certain groups of coastal meandering rivers tend to slow down and get narrower towards the coast (Chen et al., in press). On the scale of the experiment, I'd think about them as a compressed version of a coastal river and its delta. One of the causes for such width reduction is an increase in clay content downstream, hence, the kaolinite in the experiment. 2) The treatment case also has more channels, which is counterintuitive because numerical models show that finer systems have more channels (Caldwell and Edmonds, 2014, Figures 4 and 5). Please elaborate on the discussion. See the comment below for L260.

L230 add “relative to the control.”

L258 This is very interesting that backwater length is not impacting avulsion dynamics; see main comment 4, and please elaborate more in the discussion.

L260 Given the presence of kaolinite, I think the treatment’s downstream channels should migrate slower. One explanation, I believe, is that the treatment case has many small channels in the downstream end, so the mobility timescale is shorter (less area). Perhaps, there is an optimum of channel numbers: coarse bed material will create sheet flows, while very fine material will create a single channel. Also, see main comments 3 and 4.

L266-269 Can non-fluvial deposition occur in the channel as well?

L310-315 This is a fair point, but I think this is an opportunity to explore further (see the main comments 3 and 4)

Figure Comments

Overall, the figures are understandable. I have some stylistic comments. All the experiment photos and planform plots are in low resolution. I am sure this will change at publication. If you still need to, please update.

Figure 1. The brown platform is hard to see. This may change with high-resolution images. 1b) The marsh window’s annotation (arrows) needs to be modified. It is a vertical window, but on the first read, I thought this was a length scale (in a sense, it is)

Figure 2, are these just example transects to show the elevation profiles? In reality, there are many transects spaced at 5 mm, correct?

Figure 3a, please set the y-axis limit to 1 for consistency.

Figures 3,4,6 and 7. I’d suggest removing the outlines of the 1sigma and leaving only the shaded regions. Also, please consider decreasing the alpha of shaded areas more. I also echo reviewer 1’s comment about consistency with the color schemes and line thickness. Although nicely made, the plots may be too busy for the readers. In addition, for shoreline positions, I’d suggest moving the diamonds onto the mean elevation profile (or whatever the y-axis shows) or changing them to vertical lines. At first glance, they look like outliers of some data. But in reality, they are just marking shoreline position.

Figure 5 It is hard to see x on blue lines. Try another color.

Figure 6 shows that the y-limits for all four panes are cropping out data for the control. Please adjust. Also, all the y-variable for the control case seem high. Please consider elaborating, as Review One has mentioned. 6b) Please consider plotting the width-depth ratio as well. 6a and c) This is minor, but it could be intuitive to normalize the y-axis by RLSR to show whether the system is outpacing RLSR. But I will leave it up to the manuscript.