Response to Reviewer 1:

In this paper, the authors have compared two experiments to evaluate how marsh sediment accretion may influence deltas. They show the influence of marsh sediment accretion on delta channel morphology, hydrodynamics and morphodynamics, using a set of well-chosen figures and simple but appropriate analyses.

I think this is a great paper. The questions are well defined; the experiments are suitably designed to address those questions; the findings are clearly presented and generally well explained, and the paper is very well-written (to the point that I think I will share the published version with my grad students as an example of good academic writing). I have recommended minor revisions as there are a few points I feel could receive more attention: a few experimental details need inclusion; a few figure details need tweaking; some results' physical explanation could be better elaborated, and I feel the experiments' temporal dynamics warrant some further comment. Other than that, my comments are mostly minor and editorial (these are included in attached pdf comments).

Thank you very much for the helpful comments and kind words about the quality of writing. We appreciate your thorough review and believe the new version adequately addresses all the concerns raised. Specifically, we have added details about the experimental setup, tweaked the figures as needed, and expanded on the discussion of our results to better illustrate the physical mechanisms that are at play in both the control and treatment experiments. However, due to the length of this manuscript and the nature of temporal dynamics in the experiments, we did not expand much on this herein. Because the experiments were run in equilibrium conditions, the temporal dynamics observed are a function of autogenic dynamics (see response to last comment). While interesting and warranting study, another manuscript that analyzes the temporal dynamics of shorelines and how that controls wetland sediment accumulation is in preparation (see lines 110-111). Please find our responses to your comments below in blue, italicized text, as well as in the attached pdf where we have responded directly to your comments. All changes can be found in bold text in the revised manuscript.

General comments:

I know these experiments are covered in another paper, but some very basic detail including the flow rate, sediment supply rate, fluvial sediment size and distribution thereof, and scaling method (or justification, if none used) should be added. I think this will help other experimentalists to contextualise your work.

We agree that more context will help the reader better understand the experimental set-up. As such, we added Table 1 to outline the boundary conditions of the two experiments. Further, we have elaborated on the scaling justification for the marsh proxy (lines 120-123) and added information about the grain size distribution of the river sediment (lines 125-126).

For all your figures, I recommend checking the consistency of colours for the points marking shoreline positions, and check that text is not too cramped in the legend.

Thank you for pointing out the inconsistency with the shoreline diamonds colors. The legend was incorrect for all figures and has been fixed. Further, we moved the shoreline diamonds onto the line per the suggestion of R2, and changed them to open circles. Other figure tweaks were also made, including increasing the spacing and size of the legends and removing black lines surrounding the 1 sigma standard deviation bands. We note here that we did not change the aerial images in Fig. 3c and d or Fig. 8 as suggested. In Fig. 3 c and d, the timesteps were chosen to represent average flow conditions and in Fig. 8, the timesteps were chosen to illustrate the difference in channel morphology. Please refer to the pdf for an in depth response to your comments related to these figures. Because the experiments are run in an equilibrium state, the different time steps are not in a different delta growth stage and since we are not comparing absolute elevations, time step is not a factor here.

Third, I've commented a few places where I think you could give a bit more detail on the physical mechanisms that explain your observations.

We have responded directly to all comments you made in the pdf (please refer there for specifics) and expanded on the physical mechanisms where asked (e.g., additional text in lines 278-281, 282-286, 344-348, and lines 402-411).

Finally, I think the paper could benefit from some consideration of the temporal dynamics of the experiments – was there any temporal variability in behaviour? Either trend or periodicity? If so, did it differ between the experiments? If there was no real temporal pattern to the experiments' behaviour then I think this is worth just mentioning too. For instance, I was initially expecting to see some discussion of progradation and changing behaviour as the delta prograded. In hindsight, it makes sense that your fixed SLR rate dampened progradation, but others might have the same question so I think it would be helpful to either discuss any interesting temporal dynamics or comment on why you have not.

Thank you for this comment. We did not do a great job explaining the experimental set-up, but we run the experiments at an equilibrium state. Therefore, all temporal dynamics arise from stochasticity and autogenic dynamics and not from progradation, as this stage of delta formation occurs prior to data collection. We have added some detail on this in the Methods section (see lines 110-111). However, R2 raised a similar question related to shoreline variability over time. The complexity of these experiments and the amount of data collected allows us to address a range of questions, and we have a manuscript in preparation related to stratigraphic preservation of the various units and how preservation is related to the shoreline position (using spectral analysis). For this reason and in an effort to keep the length of the manuscript in check, we stand firm in our decision to keep the scope of the current manuscript to channel properties and kinematics without much discussion of the shoreline dynamics and temporal variability. However, we did add text in the Results section (lines 250-253) to illustrate the range of shoreline positions for context.

Line comments: in attached pdf. Hope they're helpful!

Thank you! Your comments were extremely helpful! We have responded to and addressed all relevant line comments directly. Please see attached pdf.

Response to Reviewer 2:

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.

Thank you for your thoughtful review of the manuscript! We have addressed all comments and edited the manuscript accordingly. We believe these edits have greatly improved the clarity of our findings and we hope you find so too. Please refer to the blue, italicized text below for our responses and the bolded text in the revised manuscript.

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.

We agree that the compensation timescale is an important part of the reason lateral mobility remains similar between the two experiments. We have added discussion on this (see bold text from lines 378-380 and 383-389) and believe it helps clarify this counterintuitive result. We have further addressed this below in your comment L260.

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.

We agree that this is an interesting result and are glad you took that away from the manuscript! This point is so important that we did suggest a new control on hydrodynamic backwater in lines 367-371, where we state that "Because we did not alter the flow conditions (or hydrodynamics) between the two experiments and flow was held constant throughout the entire experiment, we suggest a new control on the hydrodynamic backwater: non-fluvial sedimentation (i.e., wetland accretion)." We also previously suggested a new control on delta top slope break in Sanks et al. (2022). Further, the title "Marsh induced backwater: the influence of non-fluvial sedimentation on a delta's channel morphology and kinematics" further emphasizes this result. We emphasize this point in both the abstract and conclusions as well. Because our understanding of the channel bed slope break result is limited to the mean channel profile, we do not want to emphasize this point beyond this average condition.

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.

We agree this finding is surprising! We spent significant time considering alternate measurements of lateral mobility. Despite our best efforts, we were never able to produce a difference in basin-wide channel mobility, and as such this result is solid. Please see Appendix B for some of the analyses computed related to channel mobility. We have bolstered the discussion about the lateral mobility from lines 378-380 and 383-389 and believe these changes help clarify that lateral mobility must be similar between the two experiments because of the experimental setup. We further expand on how the proxy might change lateral mobility in other experimental set-ups in lines 402-411.

While temporal evolution of channels and shorelines is an interesting topic warranting further analysis, in an attempt to keep the manuscript length in check, we do not expand much on this topic herein. Another manuscript by one of our colleagues is currently in prep that addresses temporal variability in shorelines and subsequent impacts on stratigraphic preservation of river *and marsh sediment. Please see added text in the results (lines 272-273) and discussion (lines 372-373) related to temporal variability in backwater length.*

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.

Because the delta top size does not vary much between the two experiments, we have chosen not to normalize any of the axes. Since marsh deposition was the only variable that changed between the two experiments, we also don't have a difference in scale between the two experiments. Further, the shorelines are very rugose and variable (see lines 250-253 for added information about shoreline positions). If the reader would like to normalize axes, all data and code is contained in the repository hosted on Github and they are free to do so!

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.

The elongated backwater length does impact the avulsion dynamics, but not the avulsions themselves. The kaolinite marsh proxy changes the mechanism of lateral movement as discussed in section 4.1 and in the conclusions. The channel in-filling ratio also changed between the two experiments because increased sedimentation in the lobes and subsequent topographic flow expansions are muted in the treatment experiment as compared to the control (section 4.1). We have added a sentence to connect our findings to some of the research you suggested (lines 339-342).

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.

Again, we decided not to normalize the x-axis for reasons stated previously, but readers can do so if they choose.

Line-by-line

L65 Patterns

Thanks for catching this!

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.

We added a table and a few more sentences (lines 120-123, 125-126, and 134-137) to the methods for clarity on the experimental set-up.

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.

We have moved the applicable section to the beginning of the Methods (lines 100-103).

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

Correct. However, the mass flux we add to these regions represents this 'sediment trapping' process, though we do not model it explicitly.

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

Correct. Marsh deposition sometimes takes more than a full hour to complete, so we are only able to pause the experiments every 2 hours to take a dry LiDAR scan. As such, when we calculate sedimentation rates in the control, we use every other LiDAR scan to avoid Saddler effects, as discussed in lines 222-223. We have added text for clarification (see lines 134-137).

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

To obtain a dry LiDAR scan (without water flowing in the channels), the experiments must be paused (lines 132-134). Dry LiDAR scans provide sub mm precision elevations, which are utilized for all relevant analyses. We added text to expand on the resolution of the dry and wet LiDAR scans (lines 134-139).

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).

We have simplified this section. For your clarification, shorelines are not tracked via thresholding, only the delta top flow is tracked via thresholding (please refer to lines 139-143). We make no mention of shorelines here, so we hope this is clear to you and future readers.

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

Yes, there are more variables. We have referenced the table and changed "such as" to "including but not limited to" (line 147).

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

A channel fraction is calculated for each radial transect. We state that we compute 'time-averaged channel area for each radial transect'. In other words, we take the average channel area over time for each radial transect, so every radial transect will have one mean channel area (with some deviation) and we can use that to calculate the channel fraction, as plotted in Figure 3a.

L146 I'd suggest adding Channel Length (Lc)

Done.

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

Not necessarily, though the deepest channels probably correspond to the widest channels. We added text to clarify (see lines 152-155).

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

16% represents one standard deviation, so it has statistical meaning, but was ultimately a choice. However, we tried different thresholds, and while the absolute backwater length was somewhat sensitive to this choice, the fact that the treatment experiment exhibited a significant backwater reach and the control did not, was not sensitive to this choice. We have expanded on this here (lines 187-188).

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.

We agree that the narrowing of channels in the treatment experiment is an interesting result. This is likely due to the fact that the number of channels increase downstream in the treatment experiment. Because the treatment experiment does not lose as much flow to overbank flooding and flow must be conserved, if the number of channels increases, the channels must be shallow and narrow in the downstream direction. The treatment experiment has the same river *sediment, as the control experiment, but has more fine-grained material from the proxy, and as such this is not counterintuitive and agrees with the numerical models that show finer systems have more channels, such as Caldwell and Edmonds (2014). We have added discussion on this in lines 341-348.*

L230 add "relative to the control."

We added "as the control" (now line 262).

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

To clarify, the increased backwater length does impact the avulsion dynamics, just not the timescale. Please refer to discussion section 4.1 and earlier comments.

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.

Thank you for this comment. We agree that this was a surprising result and spent a lot of time thinking about channel mobility and exploring many alternate ways to look at channel mobility (see Appendix B, Figs. B3-B8), as well as others not included in the Appendix (like manually and automatically tracking avulsion locations). We have found no statistically significant differences in lateral channel mobility or avulsion location with any method. As such, we are confident in this result. We think argue this has to do with the experimental set-up. First, the river sediment has an added polymer for cohesion, which is likely much stronger than any added cohesion from the kaolinite proxy. Second, the experiments evolve in equilibrium, which forces them to aggrade to relative sea level. We agree that this will also be a surprising and counterintuitive finding for readers, so we have expanded on these thoughts in the discussion (see lines 374-401).

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

This is an interesting question! Non-fluvial deposition can occur in the channels because the channels are smaller than our depositional bins. However, because deposition occurs while the experiment is running, the sediment gets immediately washed out and transported offshore. We have visual evidence of this not only during the experimental run, but also in the preserved channel sand bodies (in the stratigraphy). We clarify in the methods (lines 130-131)

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

Please see response to comments 3 and 4 and refer to the expanded discussion section 4.1.

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)

The pre-print reduces the resolution of the image, but both the sediment dispenser and the brown kaolinite marsh proxy are visible in the high-resolution image. We edited Fig. 1b to avoid confusion on the vertical scale of the marsh window. You are correct though that the marsh window is both a horizontal (length) and vertical (elevation) window, though the horizontal scale is not fixed like the vertical scale. That is why we annotated the specific elevations at the beginning and end of the marsh window.

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

Yes, correct this is one 5 mm transect for illustrative purposes. There is a transect every 5 mm or 5 cm depending on the channel property (see lines 149-152).

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

We decided not to do this because if we set the y-axis limit to 1, variation in channel fraction is not easy to see. Likewise, if we set the y-axis limit to 0.8 for the overbank fraction, then we cut out data. However, we have moved shoreline positions to the respective lines and increased the size of the legends per suggestions by both you and R1.

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.

Thanks for your helpful comments on the figures! The color scheme and line thickness are the same throughout but get scaled in the pdf – this issue should be resolved in the final version. We have removed the black line around 1 sigma for all figures. The alpha for 1 sigma was set at 0.15 and we decreased to 0.1. We have also moved the shoreline diamonds to the lines, *changed them to open circles, and fixed the legends per the suggestions of you and R1. However, we left the shoreline diamonds (now open circles) slightly above the violin plots in Figure 4c, so as not to cover up the data in the violin plots.*

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

Thanks! We moved the black x's to the treatment mean line and now they are visible.

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.

Since those two points cut off on Fig. 6a and 6c are likely outliers and adding them to the plot means making the y-axis double the size, we decide to keep the plot as is, but add the full plot (without cropped data) to Appendix C. We have noted this in the caption of Fig. 6. Further, the data cut off on Fig. 6d was data from the entrance channel, which is fixed in place. As such, we have removed all data <0.1 m from the apex and have noted that in the caption as well. We again choose not to normalize any axes, since we do not have a difference in scale between the two experiments. We also choose not to plot the width to depth ratio to keep the figure to 4 panels, but all data and code is available and readers can choose to plot that if they like!