

Reviewer reply to the revised manuscript ‘Influence of alluvial slope on avulsion in river deltas’

This is my 2nd review round for this manuscript by Prasajo et al. Previously I mentioned that:

- (1) Not all arguments and choices in the design and analysis of this study are clear to me.
- (2) Some arguments in the text seem to contradict, and not all claims are clearly supported by the results.

In their revised manuscript, Prasajo et al. improved on the clarity of their model setup and analysis. I would like to thank the authors for their revisions and clarifications in their comments to an earlier review of this manuscript. On a few minor points, I am still not convinced by the evidence presented in the manuscript, mainly concerning claims on downstream controls, the avulsion illustrated in figure 5, and the clarity of the model setup, as detailed below. Overall, the data, analysis and discussion presented in this manuscript is a valuable read to those interested in avulsion controls.

The points below refer to the author’s reply to the previous round of review.

Point 1 and point 23, concerning the debate of downstream controls on delta avulsion.

The manuscript does not systematically test downstream controls on delta avulsion, yet at several points claims that there is no correlation. In my opinion this is not evidenced in the paper. The authors refer to figure 6 and S5 for this claim. Although those figures contain data on RSLR for various deltas, the manuscript does not test whether avulsion timescales in a single delta will be different under different RSLR scenarios.

Point 6, Figure 1, and point 9, about the model setup and location of the avulsion node.

Figure 1c shows that the expected avulsion node is exactly at the end of the valley, bounded by non-erodible bed at 5m above sea level. To me, the manuscript and author’s comments do not make clear whether the avulsion node is there because of some scaling between L_a and L_s , or because of the river is no longer confined by the valley walls. Continuing on the location of avulsion nodes, I don’t understand the answer by the authors to point 9 and I’d recommend the authors to clarify throughout the manuscript whether an avulsion length is the real measured length or the length until the expected avulsion node.

Point 16, about an example of an avulsion in figure 5.

I very much appreciate the author’s efforts to explore an alternative explanation for the avulsion demonstrated in figure 5.

Unfortunately, I still do not see the depositional character of an avulsion in figure 5, other than figure 5h. Figure 5a-g show an incision, as demonstrated by a change from dark orange to blue colours in the new branch. From the colour map, I cannot resolve if and why figure h suggests deposition. From the colour maps, the process seems more incisive rather than depositional.

Finally, a comment about **figure 6**. I noticed data in the figure changed compared to a previous version and it is not clear why.