

Comment on hess-2021-300

Anonymous Referee #1

Referee comment on "Physical controls and a priori estimation of raising land surface elevation across the southwestern Bangladesh delta using tidal river management" by Md Feroz Islam et al., Hydrol. Earth Syst. Sci. Discuss.,
<https://doi.org/10.5194/hess-2021-300-RC1>, 2021

This study aims to quantify the potential effects of Tidal River Management (TRM) in elevating low-lying areas (beels) in southwestern Bangladesh, building upon an existing work by Adnan et. al. (2020). The study addresses the non-linear nature of sediment deposition during TRM, which underpins its main contribution. However, I have a few observations in terms of motivation, clarity, and justification of this study. I would like the authors to address the following comments diligently before the manuscript can be considered for publication.

- Line 19-20: "Beels in the western part retain more sediment because of lower average land surface elevation". Does sediment deposition only depend on the existing elevation of the selected beels? Sediment concentrations in adjacent rivers of the select beels vary, which may also cause heterogeneity in the deposition. The authors have also acknowledged this fact in the discussion section.

Line 376-377: "This means that sediment deposition in beels depends mostly on suspended sediment concentrations (SSC) in the feeding river". They should revise the abstract to provide a clear message from their study.

- Line 22-23: "Thus, the length of time of TRM application in cyclic order will need to vary across the delta to counterbalance RSLR". It is not clear what type of variation in the length of time of TRM application that the authors are referring to.
- Line 94-95: "Although their regression model had a coefficient of determination (R^2) of 0.88, it remarkably did not include tidal range (TR), suspended sediment concentration (SSC) and surface area of the beel." I think this statement is partially correct. The authors have only referred to the criteria for flood susceptibility modelling in Adnan et. al. (2020), ignoring the indicators used for simulating sediment deposition in 234 beels.

Section 2.5.1 in Adnan et. al. (2020) includes the following statements:

"To identify suitable TRM sites, five indicators were selected: i) tidal prism; ii) river salinity; iii) flood-prone areas; iv) crop production; and v) size of the 'beel'."

So, I would suggest the authors revise the statements written in lines 90 – 95.

- The contribution of this study needs to be clarified. I feel the authors should clearly write the main argument of their study. They could summarize the key research gaps in the existing relevant literature at the beginning of the last paragraph in the introduction section.
- This study used a range of datasets. It would be convenient for readers if the authors provide a summary table (including sources and resolution where applicable) of data used in this study.
- Figure 3: How were the spatial boundaries of four sample beels determined? It is not clear in the manuscript.
- The policy implications of this study are not clear. In the discussion section, the authors have critically evaluated the effects of the physical controls on sediment depositions across various beels, and their impacts on land elevation. However, it is equally important to provide a clear message to the policymakers by translating the scientific finding into policy measures.
- This study quantified TRM's impact on land elevation only from the perspective of physical environment. But historically the success of TRM was interrupted by various social factors such as social unrest, conflict, and issues related to compensation. I understand these are outside of the scope of this study. However, I would like the authors to provide a few statements on potential uncertainties in the results.