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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Investigating beach erosion related with its recovery at Phra Thong Island, Thailand caused by the 2004 Indian Ocean tsunami" *by* Ryota Masaya et al.

Anonymous Referee #3

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The work "Investigating beach erosion related with its recovery at Phra Thong Island, Thailand caused by the 2004 Indian Ocean tsunami" addresses an interesting and challenging topic that is the scope of Natural Hazards Earth Systems Science. The work develops over a modelling approach to understand the short term coastal morphodynamics in relation to a tsunami event. Despite the challenging approach, the paper does not present consistent arguments that support the relation between the tsunami and beach erosion. In fact, in the abstract the authors conclude that "Our modelling approach confirms that beaches on Phra Thong Island were significantly eroded by the 2004 tsunami" but the analysis of the results, as displayed in figure 5, also show a lot of shoreline accretion. In fact, in most locations' shoreline seems to have experienced a

minor accretion (this is especially clear in figure 5a) while significant erosion is only observed at localized sections of the coast. In fact, the large longshore variability remains mostly unresolved, an should be further discussed in the manuscript. Although the hydrodynamic component of the work seems to portray a reasonable representation of the reality, the morphodynamic component is less robust and raises some questions.

1) The first statement of the conclusions "First, it was confirmed by comparing the measured and calculated values of the sediment layer thickness that the location of beach run off identified on Phra Thong Island was reproducible and consistent with sediment transport results", do not seem to have correspondence with the data presented in the paper.

2) In section 3.1.2 "change of shoreline" authors refer that sediment transport models confirm the erosion as portrayed by satellite images, but do not present satellite images before and after the tsunami occurrence. An objective comparison of model performance with satellite date with quantitative error statistics should also be present (e.g. brier skill score). The display of satellite images just before the tsunamic also would help the reader to have perception if the coastal embayments portrayed in image 6 existed before the tsunami.

3) The comparison of tsunami deposit thickness (figures 7 and 8) with the observed sediment layer also casts serious doubts on the model performance. In fact, the locations where the larger deposition were found (> 2000 inland) are the locations where the model predicted no accumulation. Moreover, a scatter plot with estimated layer thickness against observed thickness should be presented, supplemented with objective error statistics. Although authors discuss some discrepancies, this section should be expanded.

4) When comparing the model results with validation data, it seems that it would be more useful to present more detailed data, even though at a single site.

5) Concerning model application, there are lot simplifications that can affect model

results that are not properly justified or validated. Sediment transport magnitude and consequent morphological changes are largely dependent on the chosen values for the parameters displayed in table 4 . The assumption that some parameters assume a constant should also be justified namely the friction speed (or is this critical friction?) and bottom slope correction factor.

Minor comments:

a) line. 33 how can authors “confirm” if there is no observational data?

b) Line 73 – to support the statement “reproducibility has been confirmed by comparison between the calculated and measured values” a reference is needed.

c) figure 2 – a graphical scale or different gridline numbering should ease a better perception of the scale of the figure

d) line 234 – the use of “ Manning’s roughness coefficient was fixed at n = 0.025” contradicts the recognition (l438) that “bottom surface roughness greatly affects sediment transport”

e) lines228 to 239 – presents some formatting problems

f) Lines 238 – is the “limit Shields” is the critical Shields parameter? The authors should differentiate the Shields parameter from bottom shear stress (eq. 10)

g) Table 2 - The use of significant figures should be improved.

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