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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.

Pedro Costa (Referee)

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Dear authors, the manuscript by Masaya et al. presents a modeling exercise to better understand tsunami sedimentary dynamics on Phra Tong Island in Thailand.

The manuscript has quality and clearly has potential to be published after some moderate/major revisions. First of all, the title needs to be changed. It does not correspond to the work present and is confusing. In fact, there are occasional sentences that need to be rewritten (e.g. Lines 380 and following) and some spelling mistakes as well (e.g. palaotsunami instead of palaeotsunami). So, the manuscript clearly needs to be carefully revised by a native English speaker.

There are minor aspects that also need to be changed: Line 94 - change "conditions" to "setting" Line 254 to 256 - Move to Methods Line 292 - delet extra "." Figure 6 - Add scale and North arrow As I mentioned above, there are ocasional repetitions (e.g. "but that...but that...") that make the text less fluent. All these are minor aspects that should be corrected after detailed proof reading.

However, may main concern is related with the results (main source of tsunami sediment is the shallow offshore region) and the relevance of the Manning coefficient. These two aspects need to be fully revised and further discussed. It is common that tsunami deposits are fed by dune and beach sediment (when I mention beach I am considering both the emerged and submerged areas). There are many examples that suggest tsunami deposits are essentially sourced once they started moving sediment in the shallower nearshore areas. The authors acknowledge that but they also mention the palaeontological content to support their reasoning. The fact is that palaeontological content is from deeper regions but is transported and deposited by the tsunami because it is floating in the water column. So, you can have sediment from the coastal fringe and palaeontological from much deeper areas. These findinds are common and theydo not contradict each other. I suggest the authors make this clearer to the reader by adding a couple of sentences on this - clear definition of what offshore area is and clear definition of source. The other two main points I would like to raise are related with the use of an oversimplified grain-size coastal profile. You explain on the discussion the limitations of this approach but I strongly recommend that you make an attempt with varying grain-sizes according with the sedimentary environment - deep offshore; shallow offshore; beach (emerged and submerged); dune and depositional basin. What will be the changes if the grain-size varies in a way closer to reality - dune sediments are slightly finer than beach for example. What is the model response. I strongly advise the authors to do this because it would greatly improve the manuscript. I am aware a modeling exercise is a simplification of reality but it is important to not oversimplify things. Finally, I strongly recommend that you add a couple of sentences and present control tests on varying roughness coefficient. How does it affect the end

result?

Overall a nice manuscript that needs some slight improvments before it is accepted for publication. Therefore, and based on the above, I recommend this manuscript to be accepted for publication subject to major changes. Kind regards and congrats on a nice work Pedro JM Costa