

## ***Interactive comment on “Numerical Simulations of the 2004 Indian Ocean Tsunami Deposits Thicknesses and Emplacements” by Syamsidik et al.***

### **Anonymous Referee #1**

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Dear Editor, many thanks for the opportunity to review this manuscript on "Numerical Simulations of the 2004 Indian Ocean Tsunami Deposits Thicknesses and Emplacements" by Syamsidik et al.

Dear authors, I read with pleasure your manuscript focusing on coupling field and numerical data in an Indonesian region affected by the 26th of December tsunami.

Your manuscript is well-written and is easy to follow. However, I suggest that you largely reduce the number of images. Some are redundant while others can be easily merged (e.g. 3 and 4; 6, 7 and 8; 9, 10 and 11; 15, 16, 17 and 18).

Regarding the literature review there are several very important papers that are not

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mentioned in the manuscript and need to be added (Paris et al., 2007; 2008; 2009; Costa et al., 2012; Szczuciński, 2012; among others). These papers discuss crucial aspects such as inundation phases, tsunami sediment sources and paths, geomorphological constrains, preservation issues and the authors will certainly benefit for reading these manuscripts. Some of their reasoning is questioned by these papers (for example, number of waves or inundation limit) and the authors need to acknowledge this and explain it.

For example, when the authors discuss post-depositional poor preservation, they need to understand and explain the natural processes behind it and clearly described in Szczuciński (2012). Moreover, when the authors mention that only two waves occurred in this region, they should discuss this in relation with the 7 waves described in nearby Lhok Nga (see papers by Paris mentioned above).

Furthermore, the geological criteria to identify tsunami deposits is very poorly described (e.g. "presence of sea shells") and some images are not clear enough to see the lithostratigraphical contrast (e.g. 11). To ascribe a deposit to a tsunami event you need many other criteria and you should clearly express that in the manuscript.

Finally, when you mention "As shown in Fig. 13, backwash produced a sediment deposit that was 0.38 m thick during the second wave." how did you confirmed that in the field? Costa et al. (2012) differentiated inundation and backwash with the shape of zircons, rounded and euhedral. Can you discuss this?

Your results are interesting and move tsunami geoscience forward. Coupling COMCOT with Delft 3D is interesting but you simply accepted the sediment transport formulas by default. You accepted Van Rijn formulas 1997 and 2007, why did you not test other formulas (please see Delft 3D FLOW Manual for many examples). Apotsos and Gelfenbaum work applied Delft 3D in a very specific context in American Samoa. The formula tests and results these authors obtained are obviously related with a context. You need to do the same and test, at least, other sediment transport formulas provided

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by Delft 3D-FLOW.

Again, this is an interesting manuscript despite its weaknesses in sedimentological aspects and the straightforward application of a very competent open-source software.

In my opinion, this manuscript requires major changes before it is accepted for publication on NHESS. As mentioned above, the science is there but the authors need to redo some figures, add references, test new sediment transport formulas and totally reconsider its discussion based on previous findings described in the papers mentioned above.

Kind regards

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2018-348>, 2018.