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Discussions

Interactive comment on "Reconstruction of flow conditions from 2004 Indian Ocean tsunami deposits at the Phra Thong island using a deep neural network inverse model" *by* Rimali Mitra et al.

Anonymous Referee #2

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The paper presents reconstruction of flow conditions from the 2004 Indial Ocean tsunami deposits at the Phra Thong islant, based on a deep neural network iverse model. It bears strong similarity to the techniques developed in their 2020 paper (Mitra et al. 2020, JGR Earth Surf., DOI: 10.1029/2020JF005583, deemed M2020 below), and hence the methodology would not seem to be new. However, the paper does demonstrate novelty as a similar application at a new site (Phra Thong island in this work vs. Sendai plain in the JGR paper), thus establishing greater applicability. The work is presented reasonably, but in some instances I feel should require some fur-

ther details. I recommend eventual publication, provided the following issues can be addressed.

The figures presented as Figure 1 in this paper have already appeared in M2020 (their Figs. 1 and 2). This is at least acknowledged in the caption. Figure 2 also appears identical to Fig. 3 of M2020 (apart from some color changes), but this does not seem to be indicated in the Figure caption. Is this indeed essentially the identical figure? If so, this ought to be acknowledged. (It could just be very similar, and the differences not perceptable).

Several details of the numerical model used to generate test data sets in Section 2.1 are seemingly missing. These include, the following issues: How are the friction velocity (u_*), the setting velocity (w_s,i), the sediment entrainment coefficient (E_si), and other variables (r_0i and F_i) determined?

Regarding the friction velocity (u_*), in particular, it should be noted that great care ought to be taken for this quantity if standard (based on steady flow) friction formulas are used, as several recent research papers have shown that tsunami induced bounday layers may span only a fraction of the water depth, and hence these may well be invalid. See e.g. Lacy et al. (DOI: 10.1029/2012JC007954), Williams & Fuhrman (DOI: 10.1016/j.coastaleng.2015.12.002), Tinh & Tanaka (DOI: 10.1080/21664250.2019.1672127) or Larsen & Fuhrman (DOI: 10.1016/j.coastaleng.2019.04.011). Please clarify this point, and if this is indeed being done, this potential defficiency ought to at least be acknowledged.

Can a definition sketch of the model domain (etc.) being used for the generation of the training data sets please be provided? This will help readers immensely to get an idea as to the actual setup being used. Plots just showing performance (like Figs. 2 and 3) fail to provide this.

I do not find that the DNN architecture being used is presented with sufficient clarity. In Section 2.2 (top) it is stated that the DNN model accepts grain-size and thickness distribution at an input layer, and that the outputs are the "tsunami characteristics through several hidden layers". This is rather unclear. Further clarification is also provided in Fig. 1, though it is not clear if this is the actual architecture or just intended as an example. Please (just in a sentence or two) summarize the DNN architecture i.e. clarify precisely the no. of inputs, the number of hidden layers (and nodes in each layer), and the number of outputs to remove any ambiguity. Such details are rather important should one attempt to reproduce this work.

The reviewer hopes that the above points will help to further improve the paper.

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