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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "A comparison of a two-dimensional depth averaged flow model and a three-dimensional RANS model for predicting tsunami inundation and fluid forces" *by* Xinsheng Qin et al.

Anonymous Referee #2

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The paper compares tsunami inundation and forces based on two modelling approaches based on: (1) nonlinear shallow water equations, and (2) Reynolds-averaged Navier Stokes equations coupled with k-omega SST turbulence closure. The models are first validated against experiments involving bore impinging onto a single square column. They are then utilized to simulate tsunami inundation of a physical model of Seaside, Oregon, USA. Proper CFD simulations of tsunamis are relatively rare in the literature, hence making this work novel. Differences are found in details of the flow, e.g. near the initial impact, demonstrating the usefulness of CFD in this context. The

work is generally well organized and written, though I have several suggestions for improved clarity below. Overall, I suggest that the work be accepted pending minor revisions, wherein the more detailed comments below are addressed.

1. As mentioned above, CFD of tsunamis are relatively rare. As this is much of the novelty of the present work, a more thorough literature review on this general topic would seem warranted, as several seemingly relevant papers are not cited. Such works seemingly include: Biscarini (2010), Montagna et al. (2011), Larsen et al. (2017), as well as Aniel-Quiroga et al. (2018).

2. p. 5: B(x,y) is ambiguously defined as the topography. Does this mean the bed elevation? Please clarify.

3. p. 9: It is stated that z is perpendicular to the flume bottom. Would it not be simpler to state that z is vertical?

4. p. 9: Discussing mesh resolution strictly in dimensional terms gives little physical meaning. Please also add discussion in terms of wall units, zˆ+=z*U_f/nu, where U_f is the friction velocity and nu the kinematic fluid viscosity.

5. p. 9: I am not convinced that simply making B(x,y) very large properly simulates a column. How exactly has this been tested? Why should the vertical column wall be modelled differently than other vertical walls?

6. Please add axes with labels to Figure 1, this will greatly improve clarity.

7. Forces are estimated using a drag coefficient in Eq. 20. Why is the more general Morrison equation not used?

8. p. 15: The function s(t) is given, but without specifying parameters A and beta, hence the reader is given no information regarding the duration. Please clearly define these parameters. Sufficient information must always be given such that scientific work is repeatable. Also, this equation is repeated as Eq. 24. To improve efficiency, please give this an equation number on first use, and avoid repetition of equations.

9. p. 18: It is stated that zero fluctuations in the along shore directions are assumed. This makes no sense - turbulence is always three dimensional, and there is no physical situation where such an assumption is justified, and Eq. 23 is not a proper estimation of k. The turbulent kinetic energy k can be approximated by one component, but this should involve a factor 1.25 (see e.g. Scott et al. 2005) rather than 0.5 in Eq. 23. Please correct this and revise accordingly.

10. Table 1: fixedValue is indicated for the velocities - which value? (Presumably this is zero, but this certainly needs to be clarified).

References

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