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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Tsunami Hazard assessment and Scenarios Database for the Tsunami Warning System for the coast of Oman" *by* Íñigo Aniel-Quiroga et al.

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1 General Comments

The article describes the development of a tsunami scenario database for utilization in tsunami early warning and hazard assessment for the coast of Oman. From a seismic assessment of possible relevant scenarios and their determination in terms of parameters for computational fault planes, via numerical tsunami propagation and inundation modeling to the derivation of relevant hazard parameters and their visualization the authors cover a complete simulation workflow. It appears that their product is already

implemented in the operational tsunami warning system of the Sultanate of Oman.

While this work is important and relevant for practical applications, I am afraid to say that scientifically I feel the article is not publishable at the current state. The reason for this evaluation is manyfold.

1. First and foremost, the description of the usage of scenarios in an early warning situation is not state of the art. It has been shown in previous work [Behrens et al., NHESS, 2010] - and I am a little embarrassed to mention my own work here - that the sensitivity regarding source parameters in the near field is so high that early assessment uncertainty in the seismic parameters can render a simple selection based on seismic location and magnitude useless at best and misleading in the worst case. The authors should know about these works and should consider corresponding consequences.

2. Second, the usage of a worst-case based hazard assessment is not state of the art. And in particular by selecting 7 of the more than 3000 scenarios as the potentially worst appears doubtful, since local amplification can lead to unsuspected effects of even smaller sized scenarios. So, even in a worst case assessment strategy, all scenarios should be aggregated and not just a few hand selected ones. Or it should be made reasonable by scientific evidence that this selection is sound.
   In addition to this, it should be explained, why a SBTHA approach is chosen over a PTHA approach. What is the advantage in the particular setting?

3. Third, the use of a draft parameter using shallow water simulated onshore velocity data appears very doubtful. I am not aware of a scientific study that evaluates the reliability of velocity data of shallow water codes in the special situation of a wetting-and-drying region and personally I have doubts that these data are in any way realistic. So, their use in hazard maps should be scientifically assessed and motivated.

4. For the inundation simulations, the authors are not able to show validated reference scenarios. Do the inundation maps coincide with historic data (if available)? At least some data from the 1945 event in the Makran Subduction Zone should be available for validating the inundation length and flow depth. In fact, the common standards of validating operational tsunami simulation codes demand not only for a general test, but for validation with field data in the application area (see e.g. Synolakis et al., PAGEOPH, 2008). The reason for this validation step is that certain parameters need to be calibrated for the situation.

5. Furthermore, it appears that the composition of seismic scenarios by means of Okada plates in those complex non-subducting strike-slip fault regions away from the Makran Fault Zone is somewhat too simplistic. A more thorough assessment of the possible tsunami sources should be made.

6. Additionally, much more care should be taken to writing and text structuring. The nomenclature is often inconsistent (e.g. *Mw* is used as well as M_w), sentences are incomplete or redundant, and a native speaker should revise the text for proper language usage and correct the large number of typos.

2 Specific Comments

P.1 L.26 The claim that tsunamis pose a bigger threat than earthquakes, hurricanes and tornadoes needs scientific justification by references or numbers. I doubt this. And if it is true, you need to define the metric in which you compare (loss of life, property, ...)

P.2 L.41 Please use unanimous nomenclature for Mw (see abstract).

P.2 L.46 This sentence is not clear to me.

P.4 L.111 ff. It is not clear to me, why recent seismicity should reveal the structure of future worst cases. You should provide scientific references that prove this claim.

P.4 L.118 This last sentence of the paragraph is a repetition of a previous one.

P.6 L.145 ff. When differentiating your scenario approach from a unit source approach, then it would certainly be appropriate to mention also other approaches. First of all, it would be appropriate to mention [Babeyko et al., NHESS, 2010] who use a similar but more sophisticated constrained scenario approach in the GITEWS/InaTEWS system. Secondly, the JMA approach should also be mentioned where every Okada parameter is varied at each location, yielding 100thousands of scenarios.

P.6 L.172 You should conduct sensitivity studies, since it appears that in particular representing the large Mw8.5 and larger scenarios by just one Okada plate in the near field is not appropriate and yields unrealistic wave patterns. How does this effect local inundation and wave parameters is a necessary exercise in order to assess the uncertainty from the modeling approach.

P.8 L.200 f. Could you describe how you represent the more oblique strike-slip and reverse fault events by means of Okada plates? Is this realistic?

P.10 L.227 ff. What does $N70 - N90°E$ mean, is it North or East?

P.10 L.250 The inversion to obtain magnitude from the size of a plate via scaling laws is interesting and I did not know about this. But is there some reference that introduced this method? If not, then it should be explained in more detail and in particular it should be investigated more solidly by sensitivity analysis.

P.11 L.256 ff. The language in that paragraph needs major revision.

P.14 L.331 ff. Please describe in detail: Is the sea floor bathymetry AND the sea surface changed by the initial condition? And do you account for dispersion of the sea floor displacement in the water column?

P.14 L.337 ff. Please describe in more detail: How did you merge the bathymetry and topography maps, since most of the times sea level zero is different in these communities. Did you find any problems with this? Furthermore, what was the rationale behind chosing the named resolutions? Is 45 m on the finest level enough to compute realistic inundation maps? Did you perform sensitivity analysis?

P.16 L.367, Fig. 6 You give different definitions for the drag level: either $d = u \cdot h$ or $d = u \times h$. Since u is a vector and h is a scalar, these definitions are somewhat ambiguous, anyways. What exactly do you mean?

P.16 L.370 f. This sentence is incomplete.

P.16 L.373 This sentence is redundant, since the exact same informaton was given before. It would be more interesting at this point to get more information about the modeling setup, e.g. time step size, bottom roughness parameterization, dispersive/non-dispersive equation set, linear/non-linear equations, etc.

P.16 L.379 What are graphical epicenter locations versus numerical ones?

P.16 L.380 It was already mentioned in the general comments above that the selection mechanism is much too simple for the near field. This is scientifically not sound.

P.17 L.391 ff. What you call validation point is somewhat missleading. Validation is the process of checking if a simulated result represents the physical reality. This is not intended in an early warning situation. You want to *compare* or *match* the values from measurements to simulation results. So, I would suggest to rename these points.

P.20 L.422 ff. These two sentences contain no new information and can be cancelled without loss.

P.20 L.434 While reading the article in one piece, at this point I was not aware any longer that you had selected 7 scenarios for your worst case aggregation. So, the number 7 here falls a little bit out of heaven. Maybe you could add a paragraph or two, explaining in more detail your procedure to select those 7 scenarios at this point.

P.22 L.445 f. It is not clear to me, what you mean by "... using the equinoctial highest tidal level as reference". Do you mean you set this water level to be the still water level for your simulations? Or do you compare the inundation height/runup-level obtained with a mean sea level zero with this equinoctial tide level? Please explain.

P.22 L.469 Can you please define the term *people instability*?

P.28 L.576 The reference Fernandez et al. is formatted incorrectly ‘amp;‘ and is this reference peer reviewed?

P.30 L.645 ff. The reference to Strasser et al. appears twice.

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