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Natural Hazards and Earth System Sciences
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Discussions

Interactive comment on “Tsunami run-up estimation based on a hybrid numerical flume and a parameterization of real topobathymetric profiles” *by* Íñigo Aniel-Quiroga et al.

Anonymous Referee #1

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The manuscript proposes a methodology to estimate tsunami runup by mixing up a classical Tsunami code (COMCOT), for the first stages, and an averaged Navier-Stokes model for the runup process. In my opinion, the manuscript exhibits a well and organized work and I suggest that it should be published after minor revision regarding specific points that shold be clarified, because they affect in the understaing and make the manuscript not fully reproducible.

Major comments:

1) Time computation is regularly mentioned, however there is no solid numbers. For example, how long it takes a regular tsunami running? How long it takes obtain the

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final runup estimation with the presented methodology ?

2) It is also not mentioned, but I guess authors have assumed an instantaneous tsunami generation. This have to be very clear. In general, there is lack of details on the tsunami modeling. Domain size, computation time, CFL condition (depending on your chosen grid size), etc. You should, at least, comment some lines due to the fact that time characteristics of the seismic source can enhance the tsunami amplification. This becomes important in huge and rare events as The 1960 Chilean Earthquake and 2004 Sumatra Earthquake, where the source time function is not well resolved (specialy in the Chilean event). Besides of all the earthquake parameters, there is the slip distribution. It is demostrated that the runup can be amplified up to six times (Geist (2002), Ruiz et al. (2015)). So, the kind of seismic sources should be clearly defined.

3) The approximation of the topo-bathymetryc profiles are fitted from the GEBCO data, but no resolution is mentioned. The authors fixed the profiles with four (4) segments: a constant depth (1) conected to two lines offshore (2) and another line inland (1). The first and natural question is why to set 4 segments ?? Is it because the 5-space of parameters is already big enough? Another issue, is that the trench morphology is not captured, or at least, not showed in the manuscript. This is because in sibduction zones, before the ocean becomes "constant", there is a huge depression, especially in The Marianas trench, where the water column is higher and faster.

4) Authors "cheats" the tsunami interaction of the reflected wave by assuming a constant and flat region with open boundary condition. However, would not this add some kind of artifacts to the model? Test regarding this issue should be do it.

5) Authors make use of analytical solution of Synolakis (1987), however, I'm not convinced that is the good one here. There are analytical solutions in piecewise bathymetries (e.g. Kanoglu & Synolakis (1998), Fuentes et al. (2015), Riquelme et al. (2015)). Actually, in figure (13) the results do not agree with those analytical solution which state that offshore slope closest to the coast controls the runup process.

6) It is not mentioned the criterion to trace the profiles. Perpendicular to the shore? Paralel to the wave travel??

7) The methodology is compared with numerical models and retrieves same estimations. The fact that inacessible high-resolution data can be overcame should be more highlighted. Again, I dont think Synolakis's formula is comparable here, since it uses a Solitary wave as initial condition, and there are analytical solutions that can handle with arbitrary shapes (Madsen & schaffer,(2010) , Fuentes (2017)).

Specific comments:

- First line of intro: Add the 2010 Chile tsunami.

- Page 9, 5: It seems that "H" is unnecessary here. Also, it should be clarified that period relation is valid in the linear regime.

- Please add geographic axis to the map plots.

- Page 36, 5: Authors say "the results are accurate". Please, add a percentage based on the results.

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