

Interactive comment on “Effective coastal boundary conditions for tsunami wave run-up over sloping bathymetry” by W. Kristina et al.

Anonymous Referee #3

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Paper proposes a methodology, so called effective boundary condition (EBC), to apply boundary condition from incoming long wave at a certain threshold depth to calculate the runup over a sloping beach following Antuono and Brocchini (2010)'s approach and extending it. This is an important contribution since nested grids are widely used in tsunami modeling as in Titov et al. (2008), especially in real time warning to reduce the modeling runtime (Wei et al., 2008). Then, it is one of the important concern where to introduce boundary condition, i.e., to define boundaries of nested grids. Therefore, I would suggest publication addressing the following comments, not in the order of importance:

1. In recent studies while Ezersky et al. (2013) presented the resonance effect on C269

long wave propagation over continental shelf and slope Stefanakis et al. (2011) presented the resonance effect on sloping beach. I would urge the Authors to comment on how EBC will capture resonance effect presented in the mentioned studies.

2. Page 319, line 2: Different applications of the analytical solution of the linear shallow-water wave equation are also given in Kânoğlu and Synolakis (1998) comparing its results with experimental data.
3. Page 328, line 18: In addition to the references listed, Carrier et al. (2003) and Kânoğlu (2004) also presented initial value problem solution of the nonlinear shallow-water wave equations over a sloping beach. These studies could be acknowledged among others.
4. I would suggest to the Authors to consider the following problems where experimental data and/or analytical solutions are available. It is the way methodology should be validated, i.e., comparing with experimental data and/or analytical solutions. I do understand that it might not be possible to apply their approach to all. However, I believe second problem is essential to show the performance of their approach.
 - (a) It could have been much better if the Authors used Synolakis (1987)'s experimental data (<http://nctr.pmel.noaa.gov/benchmark/>) on solitary wave runup over canonical problem (a sloping beach connected with a constant depth segment) rather than case presented in section 5.1.

- (b) Also, Kânoğlu and Synolakis (1998) presented experimental data on solitary wave propagation over a composite beach (three slopes) fronted by vertical wall at the shoreline. Later, this problem is suggested as one of the benchmark problem by Synolakis et al. (2008) (<http://nctr.pmel.noaa.gov/benchmark/>). Application of the proposed methodology to this problem would be very important not only it will allow comparison of the results with experimental data but also it will show how the reflection from the vertical wall located at the shoreline is handled by the methodology presented here.
- (c) Along the same lines, it could have been better if the Authors used one of the benchmark problem proposed by Synolakis et al. (2008), i.e., initial value problem solution of the nonlinear shallow-water wave equation considering N-wave propagation over a sloping beach first presented by Carrier et al. (2003) and later Kânoğlu (2004). The Authors could have solved the problem as an initial value problem far from the shoreline using nonlinear shallow-water wave equations and then introduce as boundary condition near the shore using EBC, may be, at different threshold depths. Then, they could compare their results with benchmark solution.
5. I would suggest to remove sentences like (page 345, line 16-17) "The dashed and dotted-dashed lines again represent the linear model..." from the main text. These information should be given in the captions of the figures, no need to repeat in the main text. This will make the reading much more smooth.
6. In most of the figures it is hard to distinguish different lines. I do understand they are very close. If it is not to much I would suggest to use different markers.

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