

Comments on the manuscript

**“Effective coastal boundary conditions for tsunami wave run-up over sloping bathymetry” (Ref. n. NPG-2014-16)**

by **W., Kristina, O., Bokhove & E., van Groesen**

This paper illustrates an approach to model the run-up of tsunami waves based on a domain-decomposition-type analysis, here called “effective boundary condition”, which sees the coupling of a numerical solution (intermediate-water domain) and an analytical solution of the Nonlinear Shallow Water Equations (shallow-water domain). The topic of the paper is clearly of interest for the readers of *Nonlinear Processes in Geophysics* and the underlying idea is attractive. However, the illustration and description of the approach is not satisfactory, this making the paper unsuitable for publication in its present form and in need of a moderate revision.

Also following the suggestions detailed in “Detailed comments”, the Authors are invited to:

- better highlight the significance of Antuono & Brocchini’s contribution for the model here described;
- clarify a few technical issues related with the model.

**Detailed comments**

1. page 319, lines 15-18. This is a good idea and the Authors can find a rather similar approach (i.e. to substitute a wall boundary with an “effective shoreline”) in a rather different context (i.e. the inclusion of the swash dynamics into wave-averaged circulation models):

M. Brocchini & D.H. Peregrine (1996). “Integral flow properties of the swash zone and averaging.” *J. Fluid Mech.* **317**, 241-273;

M. Brocchini & G. Bellotti (2002). “Integral flow properties of the swash zone and averaging. Part 2. The shoreline boundary conditions for wave-averaged models.” *J. Fluid Mech.* **458**, 269-281;

2. page 320, lines 12-15. Please, note that the work of Antuono & Brocchini (2010) is not characterized by the limitation mentioned by the Authors, as Antuono & Brocchini describe a method for prescribing a completely general data at the seaward boundary. Further, the decomposition into incoming and outgoing signals is, basically, the same described into Antuono & Brocchini (2007, 2010).

The important and nice novelty of the present work is the coupling of Antuono & Brocchini’s solution with a numerical model for the offshore regions. This

represents a useful application of such a solution, which, however is pivotal for the present model.

Both the above issues should be properly acknowledged. Further, please avoid the misspelling of “Antuono” in “Antunono”;

3. section 2. One important issue to be checked is the following.

Equation (1), like Bernoulli equation, is an “energy equation”. Further, equation (1) matches the depth-integrated Bernoulli equation only if the depth-average of the potential  $\Phi$  is zero. Since the definition of the potential involves an arbitrary constant, within a linearized system we can subtract from  $\Phi$  its depth-average.

Have the Authors checked that in their numerical implementation (e.g. equations (45b) or (47)) the above constraint is satisfied? Or, is it implicitly satisfied? Or it is not important because only derivatives of  $\Phi$  appear in their model?

4. equations (3) to (5) and related text. The steps leading from equation (3) to the “linear variables” of equation (5a) are not clear. Some terms are neglected while others are retained but it not clear why/how (also in view of the fact that the retained terms are nonlinear). It seems that only second-order, or higher, nonlinearities are neglected. This should be clarified. Further, please provide an explicit definition of the “linear variables”;

5. equations (13a) to (14) and related text. Since equations (13a) and (14) are identical, is it not a tautology to state that both equations are used to transfer the information between the two domains?

6. section 3. Since the shallow water model is, essentially, that of Antuono & Brocchini (2007, 2010), this should be explicitly stated.

Further, as already mentioned on point 2 above, also the decomposition into incoming and outgoing signals is, basically, the same described into Antuono & Brocchini (2007, 2010). This also should be explicitly stated;

7. page 339, lines 21-23. Please, amend as suggested on points 2 and 6 above;

8. page 340, lines 1-5. Please, amend as suggested on points 2 and 6 above;

9. equation (58). Is this in agreement with the definition of  $\varepsilon$  given on line 20 of page 332? Further, if constraint (58) comes from the assumption  $\delta \ll 1$ , then  $\delta$  should not appear there. Please, clarify;

10. page 347, lines 12-14. Please, remind the readers that also the flow decomposition into incoming/outgoing signals comes from Antuono & Brocchini (2007, 2010);

11. page 347, lines 19-23. Please, note that use of Ryrie's method (also for small incidence angles) can only be used for pulse-like waves. In the case of periodic waves the growth of a longshore drift cannot be properly modelled by Ryrie's approach. A clarification on this should be given.