Report on the manuscript

Tsunami propagation kernel and its applications

by

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I agree with the reply made by the author in the revised version. Below, I just list some minor points that have to be addressed.

Minor Points

- Following the suggestion by the Referee # 1, the author added the following statements (lines 77-80): "*However, it is well known that the wave amplitude is not significantly affected by non-linearity unless the non-linear wave distortion leads to wave breaking (Carrier and Greenspan, 1958; Tuck and Hwang, 1972; Synolakis, 1991). The nonlinear shoreline motion can be readily derived from the linear solution via the hodograph transform, and the run-up height is unchanged from the linear case (e.g. Pelinovsky and Mazova, 1992).*

  The last sentence is inexact and has to be modified. The linear and non-linear theory give the same extrema at he shoreline when the boundary assignment in the hodograph space is linearized. In this case the hodograph transformation essentially reduces to a map that deforms the linear solution (without affecting the extrema). These are, in fact, the hypotheses under which the largest number of analytical works available in the literature are obtained.

  On the contrary, if we consider the whole boundary assignment (that is, we include the nonlinear contributions), the wave height predicted by the nonlinear theory is larger than the linear theory. An evidence of this is given in Antuono and Brocchini (2007) where the nonlinear contributions are accounted for (at least at the first order of a perturbation approach).

  Please add some comments about this point.

- Again about the difference between linear and nonlinear solutions, it is worth noting that the inclusion of nonlinear contributions substantially modifies the conditions for wave breaking (see, for example, Antuono & Brocchini 2008, Antuono & Brocchini 2010).

  Since the analytical solution proposed in the paper holds true for non-breaking waves, I think that some comments about the range of validity (namely, the range for the occurrence of non-breaking waves) should be added in the revised manuscript.

- Section 3.2. I appreciated the reply by the author. Specifically, he pointed out how the mixed data assignment (initial/boundary data) is substantially different from a boundary data assignment: *"Therefore, this case supports my previous statement above that the formulation is not mitigated by the presence of dissipation. (The reviewer's concern is true if we formulate the kernel for waves in the infinite time domain using Fourier transform.)"*

  I think that a brief comment about this point should be added in the revised manuscript.

References

Antuono M. & Brocchini M. *The Boundary Value Problem for the Nonlinear Shallow Water Equations*, Studies in Applied Mathematics, 119: 73-93, (2007)

Antuono M. & Brocchini M. *Maximum run-up, breaking conditions and dynamical forces in the swash zone: a boundary value approach*, Coastal Engineering 55 (2008) 732-740

Antuono M. & Brocchini M. *Solving the nonlinear shallow-water equations in physical space*, J. Fluid Mech. (2010), vol. 643, pp. 207-232.