Dear Professor Didenkulova,

Thank you very much for your comments and suggestions. We took into account all of them to modify our manuscript (including the comments by the fourth referee). In your comments you mention that Bowers' 1977 (JFM) paper is relevant to our article and that it should " in the limiting case of infinitely wide channel, give the result similar to ours". Bowers obtained an analytical result in closed form for a geometry where a flat-bottomed rectangular bay is connected to a channel of greater but finite width. Furthermore, he considered only the particular case (page 75, line 3 in his article) where the wavelength of the incident wave is larger than the width of the channel (in our case the "wider channel" has infinite width, the open ocean). Hence, to apply his theory to our case the wavelength of the incident wave has to tend to infinity. Both in our Model-1 and Model-2, the runup divided by the amplitude of the incident wave tends to two (2) when the incident wave frequency goes to zero. In Bower's paper, the relevant equation in page 75 gives the potential inside the bay and from this we see that his coefficient "D" is equal to half the runup. He also provides an explicit relation for "D" in the same page (before the equation (8)), in terms of the amplitude of the incident wave. In the long wave limit, D is equal to one, as you pointed out. If we apply his formula blindly for any finite frequency with the channel width being infinite, we get infinite runup at the resonant frequency which is obviously a misprediction because at a finite frequency there should be more than one radiating modes in the wider channel which his formulation can not model. In Bowers' model the wavenumber in y-direction takes discrete values due to the finiteness of the width of the channel, in our case the wavenumber in y-direction is continuous. In the re-revised version of our manuscript (in page-22, highlighted) we explain that we recover Bowers' result for the zero frequency.
We removed the word "tsunami" from the title as you saw fit. We do still mention Tsunamis within the text but only when refering other peoples' works. We also removed the word Tsunami from the title and limited the applications to storm surges and surf beats, both being long waves.

We provide a a separate letter to list the corrections that we did to address to the comments of Referee-4.

Sincerely yours

M Sinan Özeren

Responses to Referee-4

Below are the list of corrections that we did as response to the comments of the Referee-4:

1-Abstract and Conclusions. The model, though interesting, is characterized by some shortcomings (see in the following), which should be briefly acknowledged here;

2-Introduction, first 3 lines. This list of references could well accommodate the following works, all based on Carrier & Greenspan transformation but giving account of different issues, like the horizontally-2D nature of the flow, the wave groupiness and the actual boundary value nature (not initial value one) of the problem at hand: Brocchini (1998), Brocchini & Gentile (2001) and Antuono & Brocchini (2008);

We include all the references pointed by the referee in the re-revised manuscript (highlighted in the PDF text)

3- page 3, lines 19-21. Fundamentals for this important result are discussed in Antuono & Brocchini (2007), which should be recalled here;

done, see lines 19-20, page 3

4- page 4, line 11. η' is a free surface elevation, not a wave height (crest minus through elevation...);

corrected all occurrences throughout the text (highlighted in the PDF text)

5-page 6, lines 3-5. This is a fairly important matter because the mentioned linearization generally leads to a shoreline dynamics weaker than that it would be obtained from a more accurate data assignment, as demonstrated by Antuono & Brocchini (2007). This should be briefly but clearly acknowledged (here, in the Abstracts and Conclusions);

We added new text to the nonlinear section to point out this issue with a new reference to Antuono & Brocchini (2007) (see lines 20-23, page 16). We also mention the effect of the linearity (highlighted) of the seaward boundary condition linearization in the abstract and conclusion where we give a reference to Antuono & Brocchini (2007).

6- page 8, lines 1-2. The divergence here discussed, though not significantly affecting the solution in the shallow waters, is a mark of some shortcomings in the analysis of the problem. For the sake of clarity, this should be briefly acknowledged in both Abstract and Conclusions;

done (highlighted)

7- page 9, lines 6-8. This sentence calls for brief discussion of the applicability of the proposed theory to real-life conditions;

Lines 19-20, page 9, we added a sentence expressing that although the effect is minor

in Model-1, it is important for Model-2, therefore the formulation remains relevant.

8-page 9, lines 6-8. This sentence calls for brief discussion of the applicability of the proposed theory to real-life conditions;

removed, *see the previous comment*

9-page 15, lines 1-3. This is another shortcoming of the model that, for the sake of clarity, should be briefly acknowledged in both Abstract and Conclusions;

In both the abstract and the conclusion we now explicitly mention that the incident wave is linear

10-section 4.2. This section, dedicated to the role of nonlinearities, should include a discussion of the improper use of linearized boundary conditions, leading to an incorrect weakening of the shoreline dynamics, as demonstrated by Antuono & Brocchini (2007);

Now mentioned in the nonlinear section

11-page 15, line 6. This sentence, depends on the previous one and cannot stand alone;

page 15, line 6 (these are line numbers in the previous version) reads:"As long as the waves do not break, the nonlinearity arising from the shoaling over the slope is accounted for by the CG".

We do not understand what is meant in this comment.

12- page 18, line 17. Among the few analytical studies of this problem that actually provide useful insight in the physics at hand are those of Brocchini & Peregrine (1996) and Brocchini (1998), which should be recalled here.

Both now referenced (highlighted)