Responses to comments from Prof. Efim Pelinovsky

I appreciate the reviewer for his comments to reinforce the manuscript. I will revise the manuscript with valuable inputs.

General comment

The paper under review is devoted to an interesting problem of the connection between the moving shoreline fluctuations and the recording of sea level fluctuations at a fixed point (tide-gauge). Usually such a connection with the incident wave characteristics is considered, but in this case, the tide-gauge record is the superposition of the incident and reflected waves. The obtained solution is important for recalculating the available tide gauge records into moving shoreline fluctuations but with strict constraints on the coastal zone geometry (the rectangular channel, the linearly inclined beach). The solution is obtained strictly within the linear theory framework taking the linear friction into account. Then it is applied to the analysis of the 2011 Tohoku tsunami. I have no objections to the reviewed paper, only a few minor comments.

Response: I appreciate the reviewer for the positive evaluation of the manuscript.

Individual comments

1. The authors correctly note that "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". I would like to add that in the linearly inclined bottom case, it is easy to recalculate the results within the framework of the linear theory for nonlinear moving shoreline oscillations (if there is no wave breaking). I would also like to note that the maximum runup characteristics important for practice turn out to be identical in the linear and nonlinear theory. This fact is noted in several works cited by the author, in particular in the paper (Pelinovsky & Mazova, 1992). I think, it should be mentioned in the reviewed paper as it will reinforce the importance of the linear results.

Response: I agree with the reviewer that the importance of linear solutions should be more stressed. I added the suggested points to L79-80 of the revised manuscript.

2. The author justifies the linear damping introduction only by the need for an analytical solution. Meanwhile, in tsunami practice, this term is relatively widely used, see, for example, the latest work (Davies G, Romano F and Lorito S Global Dissipation Models for Simulating Tsunamis at Far-Field Coasts up to 60 hours Post-Earthquake: Multi-Site Tests in Australia. Front. Earth Sci. 2020, vol. 8: 598235. Doi: 10.3389 / feart.2020.598235) and references therein. The references to such works are sure to improve the transition from the theoretical work and tsunami practice.

Response: I appreciate the valuable information. The description of the previous applications in tsunami modeling were added in L96-103 of the revised manuscript, which support the use of the linear damping in the present work.

3. A long time ago the paper by Mazova, R.Kh., Osipenko, NN, and Pelinovskiy, Ye.N. "A dissipative model of the runup of long waves on shore" (Oceanology, 1990, vol. 30, N. 1, 29 – 30) was published. In the above-mentioned work, the same linear shallow water equations were solved, only a monochromatic incident wave was considered as an input. It is worth referring to in reviewed manuscript.

Response: I was not aware of the work. I cited the work in L101-103 of the revised manuscript.

4. I would like to note the confusion in the list of references. No pages are indicated in the papers of Chan & Liu and Didenkulova et al. The paper by Choi et al seems to be mixed with some other paper (therefore, the authors and pages should be checked).

Response: I am sorry for the confusion. I corrected the reference list.

Responses to comments from Reviewer #2

I appreciate the reviewer for the in-depth review. The thoughtful comments and feedback would help improve the manuscript. Each comment has been carefully considered point by point. I hope that I did not mistake the reviewer's points.

General comment

The proposed manuscript deals with the prediction of the coastline inundation through the use of proper analytical kernels for the Linear Shallow Water Equations. The basic idea has been already developed in a previous paper of the author (i.e. Shimozono 2020) and is further inspected here in order to define a more straightforward procedure for the data assignment. In particular, the formulation proposed allows one to assign a boundary datum that includes both incident- and reflected-wave components. In addition, the author proposes the use of a damping factor which accounts for the energy dissipation encountered by the wave during its path toward the shoreline.

My overall opinion is that the manuscript is well written and that the mathematical approach is sound. Further, the proposed analytical solution is compared with measurements from real tsunami and this makes the present contributions interesting for the readers of NHESS. There are, in any case, some aspects that deserve an accurate inspection and a deeper insight. Below, I list the points that have to be addressed before the paper may be accepted.

Response: I appreciate the reviewer for the positive evaluation and for raising critical points to improve the manuscript.

Major comments

1. The author states that the proposed solution applies to a generic datum (that is, a signal that includes both reflected and incident components). In any case, differently from his previous work (Shimozono 2020), he does not provide any evidence of this.

   My personal opinion is that this statement by the author is not correct and this is confirmed by the occurrence of a singular kernel in the equation (14) as a consequence of the assignment in (11) Indeed, in absence of dissipation (namely, α=0), the complex values of p such that I0(2s) = 0 correspond to a null wave elevation datum at the seaward boundary (while in general

G(s) ≠ 0). This occurs when the reflected and incident components of the wave elevation are in opposite phase at x = 1. In the same case, the velocity at x=1 is generally different from zero see the equation (10).

This points has to be clarified with care. I think that the cause of the presence of singularities in the kernel is simple due to the assignment on variables which do not exclusively represent the whole incident signal.

Response: I had the same concern as the reviewer during this work, but the statement in the manuscript is correct. The kernel was formulated with a generic datum, $\eta_0(t)$, that contains both incident and reflected components. In the case of $\alpha = 0$, the complex values of s such that $I_0(2s) = 0$ corresponds to complex frequencies at which a node is formed at x=1, i.e. a null wave elevation as stated by the reviewer. I interpreted the reviewer's concern as the incident wave signal may not be derived from the generic datum at x=1 for such frequency components. Here I would like to stress that the kernel was formulated with an initial condition (stationary water) using the Laplace transform. Therefore, in principle, we could get the incident wave signal from $\eta_0(t)$ by following back its history to the initial state (This is what the kernel convolution does).

The present kernel was derived through mathematically rigorous procedures without any approximation. The resulting formulation suggests that it is possible to get $\eta(x,t)$ from $\eta_0(t)$ through the principal-value integral despite the singularities. Please see my reply to Comment 3, in which I newly demonstrate a critical case supporting that the kernel perfectly works even when a node is formed at x=1.

2. The presence of the dissipation (i.e. $\alpha \neq 0$) seems to mitigate somehow the singularity of the kernel, since the poles are not aligned along the imaginary axis [see the equation (19)]. I think some comments about the differences between the inviscid and viscous solutions should be added.

Response: The presence of the dissipation does not essentially change the singular kernel structure. Indeed, the singular structure remains in the kernel of $\alpha \neq 0$. The shift of poles by α from the imaginary axis introduces a decaying exponential of time into the kernel. This

means that we only need a shorter history of η_0 to isolate the incident wave and then obtain wave solutions over the slope. The dissipation changes the causal relation and makes the kernel more compact in time (we do not need to look back to the initial state). It was explained in the original manuscript as the differences between the inviscid and viscous cases. This difference can be interpreted as "mitigation" in a practical sense, but I believe it is not what the reviewer means. I provided an evidence of this in response to Comment 3.

3. Section 3.2. As shown in the previous sections, the singularities in the kernel are "accounted for" through the integration along the Bromwich path. This means that the singularities are handled through principal-value integrals. This is a well established procedure. What I do not understand is why the solution for η in the equation (33) is obtained by substituting the equation (32) inside the equation (12) straightforwardly. The solution that is obtained is obviously ill-posed and cannot be accepted in this form.

   This is clear if we observe the Figure 5. For example the panel (a) shows an amplitude which is $O(10^2)$ while the amplitude at the seaward limit is $O(1)$! More in general, the overall behaviour of the amplitude described in this figure is quite odd and seems not physical.

   Once again, I stress that the occurrence of singularities in the solution for η is here mitigated by the presence of the damping factor [see the expressions for A and B just after the equation (35)]. In fact, if $\alpha = 0$, we would obtain a singular solution of η for those values of λ such that $I0(2i\lambda) = 0$.

   In conclusion, I think that all this section has to be rewritten by deriving a solution for η obtained through the use of principal-value integrals (as done in the previous sections).

Response: The case of monochromatic waves in 3.2 might be a bit confusing, but the results are physically correct. In this case, a monochromatic wave of unit amplitude is observed at x=1 as a result of a superposition of incident and reflected waves. Therein, we look at waves in an equilibrium (steady) state neglecting the initial transient phase, namely, partial standing waves over the slope. The solution can be obtained by inverting Eq. (33) into the time domain using the residue theorem. For the equilibrium solution under the viscous condition, we do not need to consider the residues associated with zeros of the modified Bessel function which form a transient part of the solution vanishing with time.

The odd behaviors of the wave amplitude in the figure are because a node is formed at x=1 due to the superposition at some frequencies. In the case of small dissipation, the ratio of wave amplitudes at x=0 to x=1 becomes very large because the node of the partial standing wave is formed at x=1, as shown in the top panel of Figure 5a. Also, the node could be formed on the slope where the ratio becomes very small, as shown in the lower panels of Figure 5a. Furthermore, if $\alpha = 0$, the ratio goes to infinity at $\lambda = c_k/2$ when a complete node is formed due to a full standing wave. This is what the reviewer pointed out, and this occurs because the amplitude is fixed to be unity at x=1 (the amplitude is not known a priori, especially for viscous cases).

In response to the comments by the reviewer, including previous ones, I added a critical case of $\alpha = 0$ as evidence for the kernel works even when a node is formed at x=1. Now we consider a transient monochromatic incident wave given by

$$\eta_i(t) = \tanh(\frac{\lambda t}{10})\sin(\lambda t) \tag{2}$$

As illustrated in Figure 1a, the incident wave is growing to a monochromatic wave from an initially stationary state. The tanh function was introduced for a smooth transition from the stationary state to a monochromatic wave during several wave periods. When we set $\lambda = c_1/2 = 1.2024$, a node will be formed at x=1. Figure 1b shows $\eta_0=\eta(1,t)$ computed from η_i using the incident-wave kernel proposed in my previous paper (Shimozono, 2020). The null elevation datum occurs after the initial transient phase due to the formation of a full standing wave. Figure 1c compares the shoreline elevation, ξ, computed from η_i using the incident-wave kernel and that computed from η_0 using the present kernel. The two results show a perfect agreement, and the wave amplitude approaches the analytical solution of the monochromatic wave runup, $2/\sqrt{J_0(c_1)^2 + J_1(c_1)^2} \approx 3.85$. Even if the null datum occurs at $\lambda = c_k/2$, the kernel convolution can predict a waveform at any location over the slope from the initial transient part of the wave history at x=1. 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.)

In the revised manuscript, I revised Section 3.2 for clarity and explain the raised point more clearly with the new case shown here. Please see L286-300 of the revised

manuscript. I believe this provides an evidence that the kernel works for a generic datum even in case of α = 0.

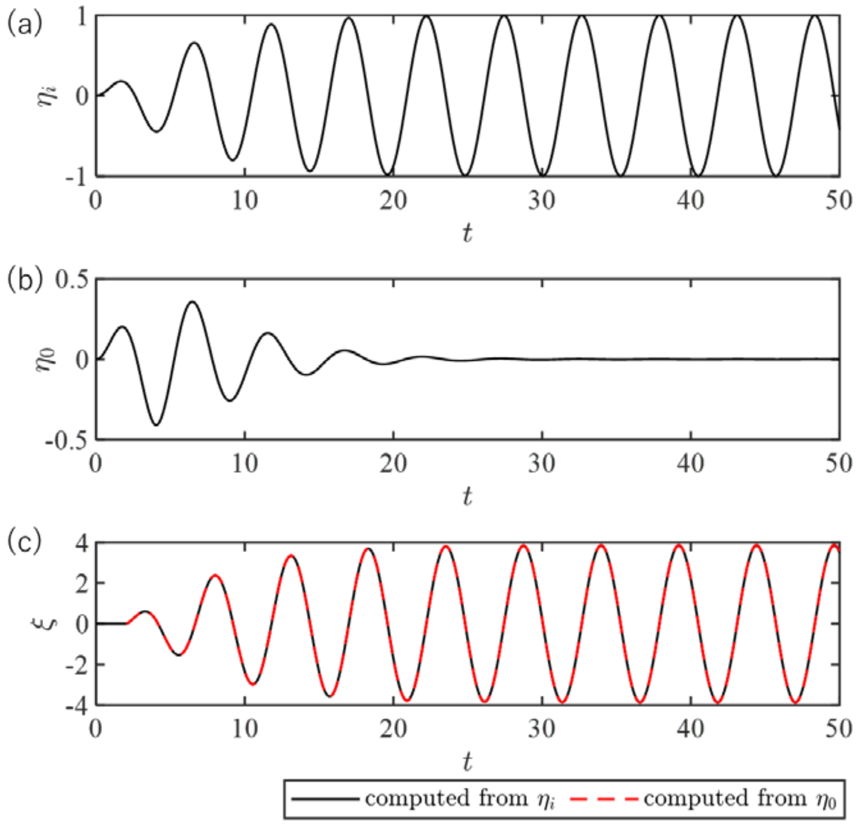

Figure 1 Kernel validation for a monochromatic incident wave starting from the stationary water: (a) incident wave, $\eta_i(t)$, (b) observed wave at x=1, $\eta_0(t)$, and (c) shoreline elevation data computed from $\eta_i(t)$ and $\eta_0(t)$

Minor comments

4. Page 6, line 144. Maybe $\widehat{\Psi}_1$ instead of Ψ_1.

Response: I corrected it.

5. Page 6, line 154. The author states "Both $\Psi_0(x, s)$ and $\Psi_1(x, s)$ have a pole at s = 0…". Actually, it seems to me that $\Psi_1(x, s)$ has a removable singularity at s = 0 [I refer to the second formula in the equation (14)]. Please check again.

Response: The reviewer is right. I revised the part accordingly (L163-165).

6. I think that the expressions for $t^{\pm}$ in the equation (24) are simply the two branches of some specific characteristic curves in the (x, t)-plane. Indeed, they may be cast in the following compact form:

$$x = [t - 2 - T(m-1)]^2$$

where $T = 4$ is the time "period" that takes a signal to travel back and forth in the fluid region.

Response: I appreciate the comment. I added this viewpoint to the revised manuscript (L200-201).

7. Equation (19). Here the author should point out that the damping factor has an upper-limit. Specifically, it should be $\alpha < c_1$ where $c_1 \approx 2.405$ is the first real zero of the Bessel function J_0.

Response: I agree with the reviewer. I described the limit of α in L170 of the revised manuscript.

Reference:

Shimozono T. 2020: Kernel representation of long-wave dynamics on a uniform slope, Proc. R. Soc. A.476, 20200333