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 by adding the suggested points. I will revise the manuscript accordingly.

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. In the revised manuscript, I will describe the previous linear damping applications in tsunami modeling, which would support the current approach.

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, and I will consider referring to it in 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 will correct the reference list.