Natural Hazards
and Earth System
Sciences

Open Access

EGU

Discussions

*Interactive comment on* "On the Resonance Hypothesis of Tsunami and Storm Surge Runup" *by* Nazmi Postacioglu et al.

**Anonymous Referee #2**

Received and published: 20 December 2016

<General comment>

The paper is devoted to coastal resonance of long waves which is particularly relevant to the problem of tsunamis. The authors investigate the natural frequencies for 1D and 2D idealized bathymetric settings that consist of a uniform slope connected to a flat bed, taking "radiation damping" into account. To highlight the role of the wave radiation, they introduce a discontinuity with varying depth into the jointing point of the bathymetry. The problem is mathematically reduced to determination of complex zeros of Bessel functions via residue theorem. They solve the problem through the asymptotic approach as well as Muller method and obtain the complex frequencies together with runup amplification factor. Comparing the results from different bathymetric settings, they discuss how the coastal resonance is excited by monochromatic incident waves. The fundamental study provides new insight into coastal resonance phenom-

ena, clarifying the role of the wave radiation. However, I feel that the paper is lengthy and cumbersome. The key insight is difficult to single out. Also, it is hard to follow the mathematical derivation in some places due to lack of information. Therefore, I suggest the authors to revise the paper for more clarity and conciseness.

<Specific comments>

1. overall: The merit of using CG transform is not clear. The authors do not discuss much about the effect of nonlinearity in the sloping part of the bathymetry. I think the key results of this paper can be described more simply and concisely with linear models without using CG transform.

2. P.3 L23: The model bathymetry in Figure 1 is introduced without any explanation re-garding the discontinuity. Please briefly explain the aim of introducing the discontinuity here. It would be easier for readers to understand the latter sections.

3. P.4 L1: The authors point out the model relevance to the storm surges in Tokyo bay. The storm surge in the semi-enclosed bay is generated by continuous forcing by wind stress. It is significantly developed when the typhoon track coincides with the bay axis. I think the case is not very relevant to the present model in which the wave is generated by short-time forcing out of the bay. Please take more relevant examples if the authors wish to keep "storm surges" in the title.

4. P.8 L25: Equation (22) may be (20). If so, please check the argument of the expo-nential function in (25).

5. P. 10 L11: I think this paragraph and Figure 2 can be omitted as it is distracting. The problem with two consecutive slopes is out of the initial model settings and seems not to be necessary for the latter discussion.

6. P. 15 L6: 18 => (18). Please follow the format of the journal.

7. P.15 L18: This us => This is

8. P.15 L13: The transient incident wave is initially given with Heaviside function, but it is later switched to tanh function on an ad-hoc manner to avoid discontinuities in wave profiles. I think this interrupts the flow of the discussion. As the authors mention, waves do not "switch on" at a given time in nature. The incident wave can be given with tanh function with a transitional scale from the beginning. The Heaviside case can be given with the zero transitional scale if it is really necessary for the discussion here.

9. Fig 5 and 6: I do not understand why the authors show the results of different modes in the two figures (1st mode in Fig 5 and 2nd mode in Fig 6). To see the effect of the discontinuity, the results of the same mode should be compared. Also, please clarify which of the two methods is used to obtain these results.

10. Fig 8: It is better to simply compare the linear and nonlinear wave profiles. It is not easy to know the quantitative difference from the present figure. However, I think this part can be omitted, if it just presents the nonlinear distortion of the time axis which is well-known to potential readers of this paper (This leads to my comment #1).

11. P.17 Section 5: I understand the role of this section, but the problem here is out of the initial model settings again. Please briefly describe the purpose of dealing with the infinite slope case at the beginning of the section.

12. P.17 L6: on => one.

13. P. 20 Eq (43): The authors need to describe the 2D governing equations, boundary conditions and approximations before presenting the analytical solution. The information is necessary to understand the following derivation.

14. Fig 9 and 10: Is it possible to combine these two figures? Then, we could see the overall picture of the energy flow over the 2D model bathymetry.

15. Section 7: The authors mention the future extension of the residue approach for engineering practices. However, the actual bathymetry and incident waves in nature are much more complicated. In practice, numerical models based on the 2D nonlinear

shallow water equations are widely used to predict long wave propagation and runup. The advantage of the proposed method is not clear in practical view point.

16. Title: I do not think the paper's title fits well with the contents. Please clarify what "hypothesis" the authors examine in the paper if they wish to keep the title.