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

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article

Responses to the referee-II

The referee mainly points out that the manuscript is lengthy and that the key insight is difficult to single out. As a result of this and the criticisms brought in by the two other referees, we shortened the manuscript as much as possible. The referee also reports specific points for which our answers follow below.

1. The referee mentions " 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."

   We agree with the referee in the sense that the resonant frequencies that we calculate are independent from nonlinear effects, because we linearize the boundary conditions at the toe of the slope. We appended a sentence to clarify this out. However we want to keep the CG approach in order to be able to calculate the runup by taking nonlinear effects of shoaling into account. This is important from the hazard point of view.

2. The referee mentions "the model bathymetry in Figure 1 is introduced without any explanation regarding the discontinuity. Please briefly explain the aim of introducing the discontinuity here. It would be easier for readers to understand the latter sections."

We used the dicontinuity both to mimic a natural bathymetric setting and also to see the influence of the size of the discontinuity on resonance. As found, there is very little resonance for MODEL-I when the discontinuity is zero. We added a sentence to point this out.

3. The referee mentions "he 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."

   In most cases yes but not all cases such as, for instance the Hurricane Katrina which followed a curved track, resulting in time-varying parameters. Perhaps "meteotsunamis" rather than "typical" storm surges fit better to our case. On the other hand. The wind forcing can generate Kelvin waves which are time-periodic. Such Kelvin waves can trigger the type of waves we are considering in the bays. This may or may not be termed as storm surge due to the non-specificity of the jargon. So if the editorial finds it fit, we can change the title from storm surges to meteo-tsunamis to be in a more relevant wave frequency band:

   http://www.nat-hazards-earth-syst-sci.net/6/1035/2006/nhess-6-1035-2006.pdf

4. The referee mentions "Equation (22) may be (20). If so, please check the argument of the expo- nential function in (25)"

   There is indeed an inconsistency. The equations (20) and (22) are correct (equation numbering according to the original manuscript). But the t_0 term in the exponential term in equation (23) must be cancelled as it is already an argument of the Green’s function. Actually the same inconsistency persist in the equation

(24) but the result in equation (25) is correct. We fixed this inconsistency in the revised manuscript.

5. The referee mentions "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."

   We eliminated the two consecutive slopes case completely in the revised manuscript.

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

   corrected

7. "This us $=>$ This is"

   corrected

8. The referee mentions ".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."

   In previous studies, such as Stefanakis et al.(2015) only η and runup curves have been displayed. Shoreline velocities have been displayed using colour rather than curves. This causes the velocity discontinuity not to show up visually in the graphs (see Fig(6) of the mentioned paper). From the figure it is obvious that the derivative of the runup with respect to time is discontinuous. So we did not smooth the incoming wave -in the linear model-when runup is displayed as a function of time so that future researchers can compare their results with us without having to introduce a smoothing parameter. On the other hand, since the fluid velocity ,u, is intrinsically discontinuous when the wave starts abruptly at t_0 smoothing becomes unavoidable. Smoothing is an absolute necessity also for the nonlinear case because otherwise the CG transformation becomes discontinuous ($\lambda = t - u$).

9. The referee mentions: " 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."

   We agree. We now only give the results for the first mode. We also include the two figures in a single composite figure. We also modified the caption in order to eliminate the confusion by including the value of D in parenthesis (the modes are obviously D-dependent). The method used is the residues, also mentioned in the new caption.

10. The referee mentions: "Fig 8: It is better to simply compare the linear and non-linear 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)"

    Corrected as recommended by the referee

11. The referee mentions:"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."

    We added a justification paragraph at the beginning of the section. It basically points out the fact that most publications on runup deal with infinite slopes. We included the section for comparison purposes for future research. Infinite slope runup for steady-state regime has been calculated by Pelinovsky and Mazova (1992), we generalized this to the transient regime. We also show that there is very little resonance even when the wavemaker sits on the node of the standing waves.

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

    corrected

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

    We added the governing equations

14. The referee mentions: " 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."

    We had to remove all energy flux discussion due to the criticism of other referees. However the ray path figure clearly shows the concentration of the rays near the corners of the mouth where the energy fluxes are large.

15. The referee mentions "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."

    We partially agree in the sense that a certain idealization is necessary (multiple constant slopes or a bay of parabolic cross-section). However the residues that we calculate are independent of the character of the incident wave, so they can be computed beforehand and can be used later for any kind of wave for a given locality synchronously as the wave approaches.

16. The referee mentions: "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."

    We think that we are testing the hypothesis of the relevance of runup resonance. However we are of course open to any alternative suggestion for the title.