Nat. Hazards Earth Syst. Sci. Discuss.,
doi:10.5194/nhess-2016-353-AC1, 2017

Natural Hazards and Earth System Sciences
Open Access
Discussions

Interactive comment on "Simulations of Moving Effect of Coastal Vegetation on Tsunami Damping" *by* Ching-Piao Tsai et al.

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Received and published: 28 February 2017

The authors would like to express their deep appreciation to Professor Maza for the invaluable comments and suggestions, which have led us to improve the quality of the paper. Detailed responses are listed below.

General comments:

The discussion paper "Simulations of moving effect of coastal vegetation on tsunami damping" is well structure and covers an interesting topic, i.e. the effect of vegetation movement under flow action in the resulting wave attenuation. However, there are some weak points in the statement of the problem as well as along the validation and discussion of the results. These points are highlighted in the following sections. In addition, a strong effort should be done in the English grammar correction since there

are some grammar mistakes and sentences that are not well written. Response: The authors have followed the reviewer's suggestions to make the necessary revisions, which can be seen as RED fonts in the revised manuscript. We also change "Moving Effect" in the title to "Swaying Effect" for complying with the topic of this paper.

Specific comments:

1. The manuscript is focus on mangrove forests. However, authors do not provide with evidences of mangroves movement under wave action. Previous studies have argued that mangroves are stiff enough to be considered as rigid-not moving bodies under flow action (e.g.: Zhang et al. 2015, "Hydrodynamics in mangrove prop roots and their physical properties"). Since the model used in the manuscript only considers the movement with respect to the bottom by using a spring, authors maybe can state this is a very simplified way to represent some movements induced by sediment scour and mangrove uproot states, as shown in the field campaing performed by Yanagisawa et al. (2009): "The reduction effects of mangrove forest on a tsunami based on field surveys at Pakarang Cape, Thailand and numerical analysis".

Response: We have added several literatures about the mangroves movement under wave action, which can be seen as RED fonts in Introduction. We also referred to Kazemi et al. (2015) to demonstrate that the flexibility of the roots was modeled by attaching rigid cylinders to torsional connectors. We also state that this is also a simplified way to represent some movements of mangroves induced by sediment scour, tilting or uprooting states (page 2, Lines 24-28).

2. Introduction: in general literature review of the problem is poor. a. There are more recent papers about tsunami reduction by mangroves than the ones highlighted in the manuscript. b. Another important point is the lack of literature review on models able to capture vegetation flexibility under waves action (e.g.: Maza et al. 2013, "A coupled model of submerged vegetation under oscillatory flow using Navier–Stokes equations"). Since they are proposing a new model to represent vegetation motion

they should perform a literature review of this issue to find the models that have been already proposed to solve that problem. This will allow them to highlight the advantages of the proposed approach. c. Authors mentioned Paul et al. (2012) work to point out the importance of considering vegetation motion but that paper was performed for submerged vegetation under tidal current action, something that is far from the problem faced here (emergent vegetation under solitary wave action). They should find a different reference to point out the importance of considering that aspect in mangrove forests or explain the implications of Paul's paper in their work. d. Also, in the last paragraph they talk about "vegetation is deformable" but mangrove, in general are not.

Response: a. We have added more than10 references in general literature review. b. The paper of Maza et al. (2013) has been referred (page 2, Lines 22-24). c. The reference of Paul et al. (2012) has been removed. d. The mistake has been revised.

3. Numerical model description: a. There are some variables that are not defined: "uj, xi, xj, t". Notation is not consistent along the different equations: authors used sometimes vector notation, in some other equations they use Einstein notation. You should write everything following the same criterion. b. Turbulence closure model: authors should explain why they are using RNG k-epsilon model instead some other options such as k-omega SST that is aim to be the most suitable one for cases where there is flow-structure (in this case cylinder) interaction.

Response: a. Those variables have been defined. All equations have been expressed in tensor forms. b. The use of RNG k-epsilon model was described in pages 3 and 4. When the RANS equations with RNG model was solved by FAVOR technique and VOF method, we can obtain accurate results for the present wave-structure interaction. Based on the comparisons shown in Figs. 3 and 4, we can find that the accuracy between SST k-omega used in Maza et al (2015) and the present RNG is almost no difference.

4. Validation: a. Different mesh discretizations can be better understand if authors

provide with the number of points defining cylinder's diameter for each case. In addition, figure 2 does not provide with very valuable information, a zoom in around one cylinder will be useful to better visualize the mesh. b. No validation is shown for cases where cylinders move. Experiments consider here where performed using rigid stationary cylinders so that information is not available. However, authors can refer some other applications of the GMO model to shows its capabilities on solving similar problems. c. Figure 4: y-axis scale is not providing enough information; there are only two points. Furthermore, it will be more helpful to set the axis as no dimensional variables: H/Hincident and X/Lcylinders, for example. Data from Maza et al. (2015) looks different than the one provided at panel C in figure 14 of that paper. How is that data obtained?

Response: a. The number of points defining cylinder's diameter for each mesh has been added. In addition, figure 2 has been represented using one cylinder. (page 5, Lines 29-30 and page 11, Fig. 2) b. Unfortunately, the experimental information on the swaying cylinders by solitary waves is lacking. So we could only implement the validation by the case of stationary cylinders. We have added some references to state other applications of the GMO model to shows its capabilities on solving similar problems. (Page 5, Lines 10-12) c. Figure 4 has been revised by using H/Hi vs x/L. We would like to thanks Prof. Maza providing their data for our use for Figs. 3 and 4.

5. Results and discussion: a. Authors are using a spring constant equal to 1kgw/m and specific cylinder gravity equal to 0.25, why? They should explain where these values come from. b. Section 4.2: "The weakly wave reflection can be found at the front row of the stationary cylinders while it is not obviously for the moving cylinders" how do you see this effect? I do not see any significant reflected wave for any case. c. Figure 8 is not giving any valuable additional information. d. Figure 10 shows turbulent kinetic energy dissipation or turbulent kinetic energy (k)? e. Section 4.3: "DTKE is calculated from the total computed meshes of the numerical tank", do you mean it is the integrated value in the entered domain? It is not clear how you compute this value. In addition, authors say they are calculating DTKE after wave crest passes each gauge, is that a

good way to evaluate TKE evolution in the problem? The maximum TKE is produced after wave crest passes, that is there is a lag between the maximum wave elevation and the occurrence of maximum TKE. Then, I think values represented in figure 11 are not providing with all the required information to understand what is happening with this variable, especially when thinking about maximum TKE values. Instead, authors can provide, for example, with the maximum TKE value at each mesh cell recorded in the whole simulation. That way they will provide with a map of the maximum TKE along the entered domain for both cases (stationary and moving).

Response: a. We have stated the use of spring constant and specific cylinder gravity, as in page 6, Lines 18-23. Besides, we added Figs. 6 and 7 to discuss the wave height evolution and the deflection angle variation of cylinder using different spring constants. b. The statement about wave reflection has been omitted because it is not significant. c. The original Fig. 8 has been removed. d. Original Fig. 10 shows turbulent kinetic energy dissipation (epsilon). In the revised version, we show snapshots of both turbulent kinetic energy (k) and turbulent kinetic energy dissipation (epsilon), as shown in Figs. 11 and 12. e. We have rewritten the section 4.3 for turbulent kinetic energy (TKE) evolution. The time variations of total TKE at each section (Fig. 13) is added in the revised version to show the time lag between the occurrence of maximum TKE of the stationary cylinders and the maximum wave elevation. We also added the evolution of vertical profile of TKE to show the swaying effect of cylinders will induce multiple shear layers.

6. Conclusions: authors talk about TKE dissipation rate but they are not providing with any rate values. Response: We have discussed more about the damping of turbulent kinetic energy evolution along the cylinder array in Section 4.3 for the Conclusions.

Technical corrections:

1. IHFOAM is misspelled along the entered manuscript; authors are using IHFORM, which is wrong.

Response: The typo has been corrected.

2.Page 2, line 11: change “For most of” to “Most of”.

Response: The original statement has been removed.

3. Page 2, line 20: rephrase “to involve the motion of the vegetation accompanied by wave”, by something like “including vegetation motion under waves action”.

Response: The statement has been rephrased (page 3, Line 5).

4. Page 2, line 23: “shown” by “has been shown”.

Response: The typo has been corrected.

5. Page 4, validation section: there are several grammar mistakes or sentences that are not very well written such as: “The arrangement of cylinders with density of 560 and with field length. . .”, “Fig. 4 shows the maximum wave height at each wave gauge probe between numerical results and experimental. . .”.

Response: The grammar mistakes have been corrected.

6. Figure 5: it would be better if the color scale is different (similar to the one shown in figure 10 for example) to better observe the differences between two approaches.

Response: The color scale of this figure (now is Fig. 9) has been improved.

7. Figure 6: Wave gauges names are very small, please increase the names or set them on top of the panels.

Response: The fonts of this figure and all figures have been enlarged.

8. Page 5, line 3: “the same previous section” to “the same as in previous section”.

Response: The mistake has been corrected.

9. Page 5, lines 3 – 5: you have already explain the experimental set-up so you don’t need to include again the values of the water depth and cylinders field.

Response: We have removed the repeated description.

10. Section 4: there are many grammar mistakes: “while the wave crest passing over. . .”, “resulting less water velocity. . .”, “keeps with the same”, “yet it is nearly”, “the important mechanism”, “the dissipation is less than the stationary”. . ..

Response: These grammar mistakes have been corrected.

11. Page 7, line 1: “note that it might be overestimated for tsunami damping if. . .” to “that is, tsunami damping can be overestimated if. . .”.

Response: We have corrected it following your suggestion.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2016-353/nhess-2016-353-AC1-supplement.pdf

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., doi:10.5194/nhess-2016-353, 2016.