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Discussions



# *Interactive comment on* "Assessment of peak tsunami amplitude associated with a great earthquake occurring along the southernmost Ryukyu subduction zone for Taiwan region" *by* Yu-Sheng Sun et al.

**Anonymous Referee #2**

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Assessment of peak tsunami amplitude associated with a great earthquake occurring along the southernmost Ryukyu subduction zone for Taiwan region

Yu-Sheng Sun, Po-Fei Chen, Chien-Chih Chen, Ya-Ting Lee, Kuo-Fong Ma and Tso-Ren Wu

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================ General comments ================

The manuscript presents an interesting application of tsunami modeling in the subduc-



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Discussion paper

tion zone of Taiwan, where the trench is orthogonal to the coastline of Taiwan island. Authors assume a potential earthquake of magnitude Mw 8.15 breaking a fault of dimension 120 x 70 km2 with an average slip of 8.25 m. Several tsunami scenarios are simulated using slip models generated using stochastic random fields where the power spectrum of slip is assumed to falloff as k-2 at high wavenumber. The authors focus their analysis in the variability of the maximum tsunami wave height computed at several sites located around the Taiwan island. The histograms computed at each sites show a great variability, in particular at the eastern border of Taiwan, and these results are also compared against the maximum tsunami wave height computed assuming an uniform slip rupture scenario. Results reveal that assuming an uniform slip the maximum tsunami wave height underestimates a large number of predictions from nonuniform slip scenarios, but not at all sites. Also, the tsunami wave propagation shows a complex pattern of energy radiation, mostly due to bathymetry, but also to slip distribution. The subject is of general interest and with interesting results obtained in the near field, where the whole deformation pattern of an inverse fault affects the static displacement field of the coastal border along the eastern border of Taiwan island. There are some inconsistencies in the random slip generation that must be clarified, the choice of the parameters needs some justification, and few aspects that must be clarified and discussed. Then, from my point of view it requires important revisions before publication.

I apologize because I am not native English speaker, so I can not help much on it, but from my own expertise, I feel that the authors should carefully check the grammar, spellchecking, some figures and text, which are in some parts could be better executed.

==================== Major and minor points ====================

Page 2, lines 3-5. If the earthquakes associated to the historic tsunamis mentioned in the text have any magnitude estimation, please provide the value and include the reference. For instance, the 1867 tsunami, magnitude ?.

Page 2, lines 13-15. When comparing PTHA and PSHA, authors mentioned in the text that PSHA works with ground-motion parameters. So, can you complete the idea by specifying that PTHA works with tsunami wave amplitudes, or some other wave measurements?. If there is any reference, please include it.

Page 3. Line 2. Please, provide the reference for the magnitude range, Mw 7.5-8.7.

Page 3. Line 12. About the fault geometry setting. Which is the source depth of the top (or bottom) of the fault plane ?. I think it has not been specified yet in the text.

Page 3. Line 15, please complete to "...in dip slip faults".

Page 3. Eq. (1), please, specify what is L, and W.

Page 3. Line 18. I suggest to change "constant" by "parameter". Strictly speaking, in elastic heterogeneous media, the Lamè parameters (lambda and mu) vary in space.

Page 3. In Eq. (2). Which is the value assumed for mu ?.

Page 3. Section 2.1. When the authors compute the earthquake magnitude, average slip and fault area. Did the authors compare (or contrast) these values with any magnitude/fault-size scaling relationship for subduction earthquakes ?. It could be interesting to compare these values with any magnitude/size scaling relationship for subduction zones.

Page 3, line 25. For completeness purposes, please provide the scalar seismic moment, M0 for the corresponding Mw 8.15.

Page 4. Please clarify or complete the sentence in line 8, because there is a dot at the end of the sentence, so it is not clear what Eq. (4) means or represents. The 2D Fourier spectrum amplitude of what ?.

Page 4. Line 10. Please, to be consistent with the notation in Eq. (4), please clarify the meaning of "F", or, change F by Fs,t which represents the 2D discrete Fourier transform of Dx,y. Also, for completeness purposes, specify that Dx,y is the slip distribution over a 2D lattice, for instance.

Page 4. In line 10, please complete, "...wave number.", by "...radial wavenumber.".

Page 4. Line 13, please correct "corner frequency" by "corner radial wavenumber", because kc is not a frequency.

Page 4. Line 14. What happen with the phase beyond kc ?. Please, clarify. Or, the last sentence "Within the kc,....(Geist, 2002)." could be deleted because authors are describing the overall characteristics of the slip and not describing the details of how the random slip is generated numerically in the practice.

Page 4. Eq. (5). Please, be careful and clear with the mathematical notation. What does Fˆ(-1) represent ?. Is it the inverse 2D discrete Fourier transform ?.

Page 4. Line 23. Please, specify that PDF is Probability Density Function, I think it has not been mentioned before in the text.

Page 5. Line 3. Complete the units in the sentence, "...5x5 km...", by "...5x5 kmˆ2...".

Page 5. Line 3. Please, clarify that 24x14 are along strike and dip respectively.

Page 5. Line 1-4. I will ask the authors to provide some details about how the stochastic slip distribution is generated, and to be clear on the choice of parameters and discuss about the results. Please, read the following comments.

The authors used the values of the Levy PDF suggested by Lavallee et al. (2006), so please clarify in the manuscript that those values were estimated from an stochastic 2D model in the dip slip direction, obtained for the Northridge earthquake. So, why do you use parameters from a shallow crustal earthquake occurred in California to characterize a interplate subduction zone earthquake ?. Please justify, or discuss.

Notice that according to Lavallee et al (2006) and others, the scaling exponent is (nu+1) so, the Power Spectrum Density of slip is, P(k) $\sim$ kˆ(-(nu+1)), it implies that the slip spectrum behaves as, D(k) $\sim$ kˆ(-(nu+1)/2).

The authors generate random variables using the Levy distribution, and imposed P(k) $\sim$ k^(-2) as shown in Fig. 1c, so, the slip in the wavenumber domain behaves as, D(k) $\sim$ k^(-1), and Figure 1 is ok, but the slip spectrum does not follow the k^(-2) source characteristic discussed at the beginning of Section 2.2. Please, clarify this point in the text. Also, discuss the effect in the spatial distribution of slip of this choice (falloff as k^(-1) of the slip spectrum amplitude in the wavenumber domain), versus a slip spectrum that falloff as k^(-2).

From the results shown in Fig. 1, authors generated a slip spectrum that decays as k^(-1) because they imposed the power spectrum density as P(k) $\sim$ k^(-2), but in the legend they say "This slip spectrum decays with exponent of -2 and...", so, it is an inconsistency for me. Please, be clear on the choice, and the terminology used when generating spatial random fields. Herrero & Bernard (1994), Andrews (1981), and others, used a stochastic slip model with a 2D Fourier spectrum that decays as k^-2 which means, D(k) $\sim$ k^(-2). I am not saying the authors are wrong in their choice, it is only that some parts of the text need some clarification, justification of the choice, or discussion about the assumptions done.

Page 5. Line 3. Why did you set a 5x5 subfault size ?. Did you test different subfault sizes ?.

Page 5. Line 3. Did you assume a constant slip at each subfault ?. If it is the case, how do you treat the non-smooth slip boundary condition at the boundaries of the fault ?. Did you apply a taper at all the borders, if not, authors should discuss or justify their treatment.

Page 5, line 15. I would suggest to use "computational domain" instead of "...numerical model".

Page 5. Line 15. Complete the units in 5x5 km^2.

Page 5. Lines 15-19. Same comment as done in Page 5, line 3, about the assumption of uniform slip at each subfault.

Page 5, lines 21-25. Why do you use 4 min and 1 min for the nested grids ?. Did you test a different grid size?. Which bathymetry/topography is used in the numerical simulation of the tsunami ?. Please include a reference. For instance, GEBCO (https://www.gebco.net/) provides a global 30 arc-sec bathymetry, which has a better resolution than the bathymetry used in this work. Please comment on it. Which is the boundary condition set at the coastlines (the boundary between wet and dry domains) ?. Do you assume a vertical wall condition, or do you allow inundation ?. Did you impose any friction, if yes, which one is the Manning's coefficient used in the simulation ?.

Page 6. Sentence in line 5-6 is a bit confusing, please rephrase to clarify.

Page 6. Section 3.1. If I understand, authors used the vertical seafloor displacement as initial condition to propagate the tsunami, and the horizontal motion of the seabed is not included in the simulation. I will suggest to clarify better these assumptions in Section 3.1.

Page 7. Section 3.3. Authors say basically that they computed the probability of the PTA by histograms, but from my understanding they show (Fig. 5) a probability density estimated from the numerical PTA data. I think authors could say/argue a little bit more about this, in terms of this choice and analysis. I mean, does the data follow any distribution (e.g. Gaussian, Levy, Log-normal) ?, Are the PTA data (simulated) Gaussian distributed ?. Is it possible to estimate the probability of exceeding a certain input value from these numerical results ?. I think some of these aspect are not discussed or mentioned in the text.

Page 7. Line 11. Please complete the idea that after generating the second set of slip models, the tsunami is simulated.

Page 7. Paragraph 3. When you compare PTA versus distance, how do you define or measure the distance between source and station ?. At least, it could be mentioned or discussed in the text.

Page 8. Lines 14-16. Please, provide the references for the Maule, Tohoku and Sumatra earthquakes.

Page 8. Lines 22-31. The results discussed here are obtained at several sites, but It is not clear where the sites (tides gauges) are exactly located, right at the boundary, or surrounded by a wet domain even during the tsunami evolution ?. If the latter is true, the comparison of maximum tsunami wave height (this study) is not exactly straightforward comparable to runup (analyzed in other studies). Also, authors should comment on the effect (or limitations) of the grid resolution (1 arc-min, used in this study) over the results obtained. I suspect this coarse grid may have an effect on the simulations near the coast.

Page 8, line 17. Clarify what "lecture" means.

Page 9. Line 29. I would suggest to complete the idea in the sentence, "Furthermore, interpolation has a tremendous effect for the exponent value becoming larger with grid size reducing (Tsai, 1997).", because it refers to how the exponent and correlation lengths are computed from the solutions of slip models of earthquakes. On the other hand, some authors assume k-2 slip models based on other physical considerations.

============== Figures ==============

Figure 1. Clarify units, X ?, k ? , length km or 5km ?. To avoid misunderstanding, I suggest to delete the label "Northrigde earthquake" in the Fig 1a, and you can mention it in the caption (e.g. Levy parameters were taken from Lavallee et al......obtained for the Northridge earthquake.), because the realization shown is for an Mw 8.15 earthquake and not for the Northridge earthquake. Fault axis along dip and strike are confusing too. I will suggest to plot the real distance along strike and dip directions (with the correct units) and not the "indexes" of each subfault. What do represent the colorbar ?.

See my comments about P(k) and D(k), what is shown in Fig 1c is not what is written in the caption.

Figure 2. I suggest to contextualize at the beginning the region of the study area, (e.g. Map of Taiwan...for example). Correct 5x5 km by 5x5 kmˆ2. Is the white box the nested inner grid ?. Colorbar ?.

Figure 3. I would suggest specify that the "energy propagation" corresponds to, maximum tsunami wave height, for instance. Colorbar ?.

======= Tables =======

Table 1. The description of the table and caption is a bit confusing. What is the meaning of Max(uni) ?. A suggestion is that a part of the description given at the end of the table can be moved to the caption, and authors can put the units [m] directly beneath each variable description.

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