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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Tsunami risk assessment for multiple buildings by considering spatial correlation of wave height using copulas" *by* Yo Fukutani et al.

Anonymous Referee #1

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This paper presents a new statistical method for relating the hazard and risk at different locations due to the same scenario. From my reading of the paper, it seems like the main emphasis is to use spatial correlation methods in order to reduce the computational burden in tsunami hazard and risk studies, in particular with respect to computing local hazard maps. The method is unconventional, and there few or no similar studies in the tsunami literature of this kind, which makes the study a nice addition to the literature. However, the explanation of the methodology and results in order for other practitioner to utilize the findings in other situations is not fully clear. In several places in the paper, explanations are too brief, and sometimes key concepts are not explained. Yet, the paper might be considered appropriate for publication given that the following

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Discussion paper

items are improved and clarified:

- The description of copula methodology should be improved and elaborated. Essential characteristics of the methods needs to be spelled out in more details, especially in order to make the methodology transparent for NHESS readers that are not experts in probability theory and copulas. For instance, key concepts such as the transformed variable u is not even defined. Source mechanisms must be described better and in the full detail necessary.

- Limitations related with the methodology and results (i.e. the cases investigated) should be better clarified, both with a discussion, but also with some further quantification of uncertainties that are suppressed in the present version of the paper.

- The application of the method presented in the final part of the paper is key to understand the impact of the method, as it demonstrates that the coupled probability is necessary for understanding interspatial correlation. However, the description of the Monte Carlo type realisation is too brief. The sampling methods should be explained in more detail. I take it that the non-correlated results are simply sampled randomly from both marginal distributions, but this is not explained in sufficient detail anywhere.

-It is not described anywhere how the method can be used in probabilistic risk or hazard assessments, despite the fact that this is stressed in the motivation for the paper. It would be interesting if the authors could provide more details on how the copula methodology could be exploited using probabilistic methods.

More details are given in the line-by-line comments.

Line by line comments

Page 1-line 24: It would preferable if key concepts of probabilistic tsunami hazard assessment (PTHA) and probabilistic tsunami risk assessment are introduced here. The authors are simply referring to them without explaining what they are. Some more details on PTHA and PTRA would be preferable.

Page 1 – line 25: This study is very much on hazard as well as the risk. I suggest including hazard also in the title. Replace "the probabilistic risk. . ." with "a variety of probabilistic hazard and risk. . ."

Page 1 – line 28: Relevant overview that preferably should be added to these references would be those of Davies et al. (2018), Grezio et al. (2017), and Løvholt et al. (2015). BTW, what do you mean by "extant".

Page 1 – line 29: Remove "for a local area". In the end of the sentence, replace "in the area" with "in a local area".

Page 2 – line 3: Why are aggregates of buildings portfolios important in particular? Please elaborate.

Page 2- line 5: Please clarify in more detail why this is important. For instance give an example, otherwise the reader is a bit lost.

Page 2 – line 17: I'm not sure "simultaneous" is the right word. Perhaps "dependent" or spatially correlated is a better term. In any case, reformulate.

Page 3 – The simplification done by using response surfaces suppresses the uncertainty in the tsunami height (the authors uses the term wave height). This needs to be illuminated better. For instances, they could should error norms obtained using this fitting mechanism. Moreover, it needs to be clarified that tsunami heights can vary quite a bit in a local area. This property of a tsunami is concealed here, but the authors should actually quantify how large this variability is for one or more of the inundation simulations. This is important, as the authors method only operates on the fitted response function, which does not represent the full truth.

Page 3 – line 13: Is "slip ratio" the slip?

Page 3 – line 17: Statement starting with "Although tsunami numerical simulations. . ." is misleading. As said, fitting response functions will remove a lot of the actual variability. This needs to be explained better, otherwise it will seem that the method is better

than it actually is. . .

Page 3- line 20: As a non-expert in copula theory, this is hard to follow. Is the copula producing a unitary distribution C, mapping x to a new random variable u (with equal probability) over ui=[0,1]? Please clarified better, give more details, perhaps even a simple synthetic example. Moreover, the varible u is not even formally defined.

Page 4 – line 11: You have not introduced regions before, it is not clear what you mean. Please introduce the concept of regions. It may seem from the paper that regions refer to sources, which is quite confusing. More elaboration and clarification is needed.

Page 4 – line 15: Again essential details in the modelling is needed. The source parameters describing the focal mechnisms (slip, width, shear modulus, geometry etc) is missing. Please elaborate.

Page 4 – last paragraph: I would say that the uncertainty treatment is rather rudimentary, although some sensitivity is presented. The authors should clarify additional factors not covered by their study, such as variable (heterogeneous) slip, different possible fault configurations etc.

Page 5 – Line 9: The response surface method collapses all spatial variability into a, rather crude, single equation. In this way the uncertainty gets lost. This needs to be illuminated better. The variability from the simulations needs to be quantified.

Page 6 – line 9: "normality of the frequency distribution of the tsunami height is not secured" → "distribution of the tsunami heights do not necessarily follow a normal distribution".

Page 7 – line 18: What is [0,1] space. Be more specific. Moreover, define and introduce the AIC and BIC methods.

Page 7 – lines 24-29: Elaborate on how the different sampling technical (both with and without copulas) are carried out. For instance, you do not explain how the uncorrelated sampling is carried out.

Page 9 – line 7: I suggest that the authors explain in more detail how their findings can be used, for instance in PTHA and tsunami risk assessment. Possible use might be of value beyond the present study, but is a little bit concealed.

References

Davies, G., Griffin, J., Løvholt, F., Glimsdal, S., Harbitz, C., Thio, H. K., ... & Baptista, M. A. (2018). A global probabilistic tsunami hazard assessment from earthquake sources. Geological Society, London, Special Publications, 456(1), 219-244.

Grezio, A., Babeyko, A., Baptista, M. A., et al. (2017). Probabilistic tsunami hazard analysis: multiple sources and global applications. Reviews of Geophysics, 55(4), 1158-1198.

Løvholt, F., Griffin, J., & Salgado-Gálvez, M. (2015). Tsunami hazard and risk assessment at a global scale. Encyclopedia of complexity and systems science, 1-34.

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