Natural Hazards and Earth System Sciences Discussions

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC1
https://doi.org/10.5194/nhess-2021-309-RC1, 2021

Comment on nhess-2021-309

Anonymous Referee #1

Referee comment on "Modelling geographical and built-environment attributes as predictors of human vulnerability during tsunami evacuations: a multi-case-study and paths to improvement" by Jorge León et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-309-RC1, 2021

General comments:

This paper conducts a series of agent-based evacuaiton simulations and tsunami simulations for seven coastal cities in Chile to generate data to construct statistical models. Then, the authors try to find explanatory variables including geographical and built environments' variables to explain the simulated tsunami casualties, which might give implications for better spatial plannings towards future smoother evacuations. The topic of the paper falls within the scope of this journal, and the some of the results can be useful informaiton especially for better evacuation preparedness in the study areas of Chile; however, I found some concerns especially regarding the methods and discussions in this paper, which should be addressed.

The authors used various variables to explain the simulated degree of tsunami casualties. Because some of the explanatory variables seems to have corrrelations among them, I have had concerns the technical problem of statistical methods such as collinearity/multicollinearity problems. Since inappropriately constructed statistical models can lead to wrong results, I suggest additional validity checks of the statistical method. Additionally, the justifications including detailed exaplanations of the methods and the choice of the methods from similar regression models can help readers' better understanding.

Different from the previous statistical models explaining tsunami caualties base on actual data, the statistical models were constructed only based on the simulation results from agent-based simulations. Although the method has an advantage that various data can be generated from simulations, at the same time,the quality of the data and the constructed statistical is totally depends on the quality of the simulation. Therefore, inarprpriate modellings or excessive speculation from the results can lead to inappripriate implications for actual evacuation preparedness.

Along with the confirmaitn of the validity of the method and data itself, discussions in this paper should supported by additional validity check using simulaitons, and the applicability of the results should be carefully discussed.

In my view, major revisions, which might result in different conclusions, are needed to reach a publication standard. Detailed comments and further recommendations for the revision are presneted in the following Specific comments section.

Specific comments:

p.1 Line 26 - 29: The discussed integrated approach for tsunami disaster risk reduction is summarised well with its historical transitions in Koshimura & Shuto (2015), which would be useful to support the discription. The paper can be found at https://doi.org/10.1098/rsta.2014.0373

p.2 Line 31 - 33: The authors claims "this is hard to achieve...", but the reason why is not well expressed. It is better to make it clear for readers from broader reasearch fields.

p.2 Line 35: What information brings the value "15 min" ? In my opinion, the evacuation behaviour is always an effective way to save lives during tsunamis if there is sufficient lead time.

p.2 Line 37 - 39: In my understanding, "hazard" simply represents an intensity of external force and is not the term to represent how exsting condition is affected. For example, this page (https://www.preventionweb.net/understanding-disaster-risk/component-risk/disaster-risk) explains hazard as "Hazard is defined as the probability of experiencing a certain intensity of hazard (eg. Earthquake, cyclone etc) at a specific location and is usually determined by an historical or user-defined scenario, probabilistic hazard assessment, or other method. Some hazard modules can include secondary perils (such as soil liquefaction or fires caused by earthquakes, or storm surge associated with a cyclone).", with the source GFDRR, 2014. Such terminology should be consistently used, referencing reliable in ofifcial documents.

p.2 Line 47 - 50: In my view, some items are inappropriately caregorized. For example, is "elevation" exposure? Again, the abovementioned page defined the exposure as "Exposure represents the stock of property and infrastructure exposed to a hazard, and it can include socioeconomic factors". It is better to categorise them with an exact criteria, referencing corresponding sources.

p.2 Line 55: "Fragility function" is often used in this context.

p.3 Line 67 - 90: This part lines up the existing literature regarding fragility functions for tsunami casualties. Is there any criteria regarding the order of these literatures? It started from the study in 2018 and goes to 2020, but it then suddenly back to 2009. Since these studies develops their method, usually referencing old ones, it is better to present them as readers can understand the trend of these studies. If there is an intention of authors for this order, the text flow should be modified to make it clear. Additionally, the review seems to lack some literature in the same line. Additional reviews would be useful to be more comprehensive. For example, Suppasri et al., 2016 (https://doi.org/10.3389/fbuil.2016.00032); Latcharote et al., 2018 (https://doi.org/10.1016/j.ijdrr.2017.06.024); Yun et al., 2019 (https://doi.org/10.1193/082013EQS234M).

p.3 Line 93 - 94: The references seem to lack recent literature, especially which simulates detailed 2D evacuation movements. Additional literature review would help to make it more comprehensive. For example, Dohi et al., 2016 (https://doi.org/10.1142/S1793431116400108); Aguilar and Wijerathne, 2016 (https://doi.org/10.1142/S1793431116400212); Makinoshima et al., 2018 (https://doi.org/10.1016/j.simpat.2017.12.016).

p.3 Line 95: There are references that are better to be refereed here for the review of GIS-based methods. For example, Fraser et al., 2014 (https://doi.org/10.5194/nhess-14-2975-2014); Priest et al., 2016 (https://link.springer.com/article/10.1007%2Fs11069-015-2011-4).

p.4 Line 102 - 110: Since the there are tremendous amount of literature regarding tsunami evacuations, the reference here seems insufficient. Recent comprehensive review of tsunami evacuation behaviours would be useful for supporintg the discussion here. The review paper, Makinoshima et al., 2020, can be found at https://doi.org/10.1016/j.pdisas.2020.100113

p.5 Figure 1: This can be moved to the next Methodology section because detailed explanation was made in the next section, and only the name of cities are described in the first section.

p.6 Line 143: Does "35 destructive events" include "recent disasters in 2010, 2014 and 2015" ? The text can be modified for clarity.

p.6 Line 146 - 147: Here is a suitable place to present the figure 1.

p.6 Line 154: I would say "repeatedly" instead of "systematically".

p.7 Table1: It is better to present the items in "Years of recorded destructive tsunamis" with its event name and references for its mechanisms since easily accessible informaiton of the events would be useful for readers. The table captions shoule be presented at the top.

p.7 Line 161: I understand that this resolution "4x4 m" is based on the finest resolution of the tsunami simulation; however, this resolution might too fine for counting tsunami casualties in agent-based simulations. The investigation with different resolution is needed to ensure the validity of the result. If consistent important features are found in different resolution, it supports the validity of the analysys method. Reliable coarser values can be generated by integrating finer values.

p.8 Line 163 - 175: The figure showing the simulation setup for tsunami simulations (e.g., visualization of fault shape and its slip amount in a map) would be useful for better understanding of readers.

p.8 Line 178: Which part of the simulation was "enhanced" compared to the orignal

model? It should be clear.

p.8 Line 181 - 182: This desciprtion is true for agent-based modelling that simulates detailed intercations among agents (e.g., social force model), and I think readers expect this study used such model after reading this description; however, the detailed explanation of the model (p.9 Line 203 - 215) explains that the model does not simulate such complex interactions (e.g., speed down due to the congestions, which caused by detailed 2D behaviour simulations). The text should be modified to more clearly express the model capability.

p.9 Line 183: Preveously mentioned recent evacuaiton models, which simulates 2D detailed movements and complex interactions (Dohi et al., 2016 (https://doi.org/10.1142/S1793431116400108); Aguilar and Wijerathne, 2016 (https://doi.org/10.1142/S1793431116400212); Makinoshima et al., 2018 (https://doi.org/10.1016/j.simpat.2017.12.016)), is better to be refeered in this context.

p.9 Line 204 - 205: It is unclear whether "a mean time = 8 min" is the mean value of the resulting distribution or a parameter value for the probability distrubution. If "8min" refers to the parameter sigma for Rayleigh distribution, the resulting mean value of the distribution does not match this value. Presenting the mathematical expression of the rayleigh function with the parameter used in this study can avoid any confusion.

p.9 Line 211: In my understanding, the paper cited here is not the paper that first proposed the A* algorithm. Is there any reason to cite this paper here? For example, if the study used the implementaiton in the citing literature, the authors should explain so to avoid any ambiguity.

p.10 Line 217 - 218: The number of required simulation runs should be reported here instead of explaining "at least ten" because the information is useful for readers to know the simulation variance.

p.7 Line 158 - p.10 269: Sections 2.x.x includes both methods to generate data and metrics, and this mixed description can lower the readability of the manuscript. The structure of these sections can be re-structured for better readability.

p.11 Line 251 - 269: As I pointed out for p.7 Line 161, it is better to conduct the analysis with different resolution to see the effect of resolution and check the validity of the analysis.

p.11 Line 275 - 277: Although the random forest is a popular rgression model, it is better to explain what it actually is with its brief theoretical expalanation for completeness of this paper and better understanding of readers.

p.12 Line 287: I think the brief theoretical explanations on SHAP and SHAP values is necessary because most of the readers in this field are not familiar with this. At least, readers have to know the logics to estimate the importance of the explanatory variables in understanding the results presented in the rest of this paper.

p.13 Table 3 and Figure 2: The table and the figure simply displays the mean value of the variables and is not accompanied with any detailed explanations and discussions such as regional differences. As a result, current text and materials carry almost no information to readers. The reported value can be improved by reporting variances so that readers know the distribution of the variables. In stead of tables and the current figure, a scatter plot matrix representation of the raw data may be useful for understanding data. Along with the revised visual representation, detailed description of the general tendency of the data is required in the revised manuscript.

p.14 Line 307 - 312: Because some explanatory variables potentially have correlations (e.g., Maximum flood depth and Elevation; Straightness, Route length and Mean travel time), I wonder if the analysis has the problem such as collinearity/multicollinearity. The previously mentined scatter plots of raw data can help readers to consider such potential problems in data. Although such correlations may not affect the performance of the constructed model, I think it at least affect the value of importance. Confirmation of the data and the justification of the validity of the result are reuqired. Additionally, this section simply present figures and no in-depth explanations are made. Broader implications of the results or relations to the previous literature can be presented in the following Discussion section; however, this sectio at least should describe the obtained results in detail.

p.14 Line 326 - p.15 Line 341: This part discusses positive and negative factors influencing the simulated level of tsunami casualties. Althouh the all explanatory variables are not always meaningful to explain the target variables, the discussions are presented only based on a qualitative view. Because the authors conducted statistical analysis and know the importance of exaplantory variables, such discussion should be made considering whether the variables are staitstically meaningful.

p.15 Line 332 - 341: This part explains counterintuitive results and its potential cause; however, in my view, these explanations needs further validations because the data is synthesised using simulations, and the data might be generated from unintended behaviour of the simulation models. For example, combination of very local error in elevation data and the hiking function may cause unrearistically slow evacuees. Because the observed tendency is generated from data in simulations, the authors can validate their exaplantions by checking the simulation results in detail. Cause of the synthesised data can be clearly exaplained by simulations, and shoule be.

p.15 Line 338 - 339: For example, this description should be supported by showing such simulation results.

p.15 Line 343 - 345, Line 345 - 346, Line 353 - 354: Since the simulations in this study does not include realistic evacuaiton processes and are based on various assumptions (e.g., a single evacuation departure distribution), it is hard to reach general conclusion using this approach. Such limitations should be clearly expressed, and any extrapolation of the results may lead to proposing inappropriate guidelines.

p.16 Line 386 - 387: Recent study reported that tsunami evacuation processes are largely affected by socio-phychological factors and exhibit complex evacuation trips. Referencing such example would support the claim. For example, Makinoshima et al., 2021 (https://doi.org/10.1016/j.ijdrr.2021.102182).

Technical corrections:

"m." was used for the unit of length throughout the paper. It is better to replace it to "m" or "meters".