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Natural Hazards and Earth System Sciences
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Discussions

Interactive comment on "Characteristics of building fragility curves for seismic and non-seismic tsunamis: case studies of the 2018 Sunda Strait, 2018 Sulawesi-Palu and 2004 Indian Ocean tsunamis" *by* Elisa Lahcene et al.

Anonymous Referee #2

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Lahcene et al. 2020 use field data and numerical modelling to present building fragility curves for three past tsunami events in Indonesia and Thailand. They use data from the 2004 Indian Ocean tsunami, the 2018 Palu tsunami and the 2018 Sunda Strait tsunami. Their goal is to demonstrate that the differences in the building fragility curves are due to hydrodynamic parameters of the waves, ground shaking and liquefaction. It is of great interest to understand how ground shaking or liquefaction weakens buildings posterior to a tsunami impact. However, the authors do not sufficiently demonstrate how building fragility curves may contribute to this understanding in the manuscript.

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Discussion paper

The authors use numerical modelling for some of the cases, but the application of models, their sources, and modelling framework are not specified sufficiently. The application of the advanced statistical methods used is not very clear. The authors should add a statistical methods section to explain why, what, and how they use the different statistical tools and cite corresponding references. The authors should rewrite the conclusions because they are not based on the study the authors performed. Finally, parts of the manuscript could be better structured and, in some sections, the excessive use of passive voice phrases reduces the readability. I suggest a careful review with a native speaker or using a grammar and spelling checker. Nevertheless, I believe the manuscript has some potential and could be eventually considered for publication if the authors consider the following comments.

Major comments:

1: The authors present the fragility curves for the three events in the four locations (Fig. 16 a). The curves demonstrate that for low flow depth values (less than 2 m flow dept) building fragility was largest in Palu 2018 followed by 2004 Banda Aceh, followed by 2004 Khao Lak, followed by Sunda Strait 2018. Above 2 m flow depth, the curves for 2004 Banda Aceh demonstrate the largest building fragility. The authors conclude that ground shaking and liquefaction contributed to the fragility curves for Palu 2018 and Banda Aceh 2004. Although it is possible that both ground shaking and liquefaction may contribute to the fragility curves, the authors do not demonstrate this. Hence their claim is pure speculation. To demonstrate that ground shaking played a significant role, the authors should present a measure of ground shaking that allows inferring the damage of buildings that have not been hit by the tsunami. Maybe they could use seismic intensities or peak ground acceleration to infer the damage of buildings that have not been hit by the tsunami. Alternatively, they could comment on which extent buildings were damaged outside the tsunami inundation area to foster their hypothesis.

2: The authors write in the conclusions, page 30, line 573 f. : '. . ., it is clearly demonstrated that liquefaction events can increase building susceptibility.' and page 30, line

574 f. : '. . ., the building were previously affected by severe liquefaction episodes.' This sentence is not a conclusion from their work. Most importantly is to mention that the largest liquefaction areas were located outside the tsunami inundation areas. The authors should consult Watkinson and Hall (2019) and Syifa et al. (2019). Even though Sassa and Takagawa (2019) conclude that they found evidence for extensive liquefaction in coastal areas, the authors should quantify how many of the database's observed buildings were affected by liquefaction. The authors could overlap the liquefaction areas with figure 8 to see how many of the buildings were affected. The current state of the manuscript does not allow to conclude that buildings were weakened by liquefaction. Moreover, Mas et al. (2020) write that tsunami hydrodynamic and debris impact forces may have been the principal causes of failure and collapse in Palu Bay's waterfront area.

3: The conclusion that the building fragility curves for Banda Aceh and Khao Lak are different because of the ground shaking in Banda Aceh are incomplete. Just because the locations were hit bit the same tsunami event, does not necessarily mean that the wave period was the same in both locations. The rupture at the Sunda Megathrust was longer than 1000 km, and slip rates along the fault were heterogeneous (Rhie et al. 2007, Koshimura et al. 2009). Consequently, waves with different periods and hydrodynamic features may have impacted Khao Lak and Banda Aceh. Applying numerical models for both sites could show the differences, but the authors do not present tsunami simulations for Banda Aceh and Khao Lak.

4: Regarding the numerical modelling in the manuscript, the authors should comment on why they use modelling for the 2018 Palu and Sunda Strait events but not for the 2004 Indian Ocean tsunami. I believe the authors could draw interesting conclusions if they would compare impacting wave shapes for the 2004 Indian Ocean tsunami in Khao Lak and Banda Aceh.

5: Further, the authors should clearly state the motivation for their numerical modelling efforts. It is not clear why they use models where many observations exist.

6: The Digital Elevation Model (DEM) for Palu has a resolution of 1 m. The DEM for the Sunda Strait event has a resolution of 20 m. If the authors want to compare the cases, they should use the same cell size for DEMs or explain why they believe that simulations are comparable when using 400 times bigger cell size. Apart from that, I believe the reader would be interested in the data that allows building a DEM with 1 m resolution for Palu. The authors should also name the references for the dataset used in all DEMs.

7: The authors use hypothetical landslide sources for the 2018 Palu event. Those are not in agreement with some other published studies (Ulrich et al. 2019, Gusman et al. 2019). In figure 6, the authors present those hypothetical landslides as principal tsunami source without explaining why they have this assumption. The authors must include a review of previously published sources and comment on the reasons for modifying the sources in their manuscript. In figure 9, they compare the observed with simulated flow depth and claim that their model is in good agreement with the observations. To my understanding, figure 9 in the manuscript demonstrates that the simulation does not match the observation. The authors must explain why they believe the model is of good quality. Further in the manuscript's conclusion, the authors write on page 30, line 574 f.: 'Although Palu-Bay was hit by a non-seismic tsunami….'. These are the authors' assumptions and therefore are not valid as a conclusion and need to be rewritten. Please also note that figure 9 is misspelt.

8: The authors should include a paragraph on proposed flank collapse sources from other studies on the 2018 Sunda Strait tsunami. Please include Williams et al. (2019), Grilli et al. (2019), Omira and Ramalho (2020), Dogan et al. (2021).

9: The authors write that they automatically corrected the flow depth traces for the 2018 Sunda Strait event. It is not clear how the authors do that. Are they using GPS field measurements or LIDAR data? If the authors use a method previously presented, then they must cite the corresponding reference. They observe a mean difference of flow depth values of 0.28 m for 94 traces. How far is this value representative for the 94 traces, and it is not clear if the authors use this value for correction? I suggest rewriting this section. The resolution of the DEM is 20 m. Do the authors believe this resolution is sufficient to obtain reasonable values of flow depth and flow velocity?

10: Consistency with abbreviations and variables: Sometimes the authors use for Indian Ocean tsunami IOT (e.g. page 1, line 23, line 31; page 3, line 91; etc.) sometimes they use IO (e.g. page 2, line 81; page 4, line 101; page 20, line 416). YouThey authors use GEM as an abbreviation on page 2, line 82 but only introduce the Global Earthquake Model (GEM) on page 13. The authors also use the abbreviation AIC without stating what the abbreviation stands for (page 18 ff.). In 4.1 the authors use the Greek letter pi as probability, and in 4.2 they use P. What is the difference?

Minor comments:

Page 2, line 51: Some other studies consider less volume and other particularities of the collapse. Please include them (Williams et al. 2019, Grilli et al. 2019, Omira and Ramalho 2020, Dogan et al. 2021).

Page 2, line 81: IO or IOT? Please be consistent.

Page 2, line 82: Please indicate what the abbreviation GEM stands for.

Page 3, line 91: I suggest rewriting the sentences since seismic and non-seismic curves is not clear.

Page 4, line 101: IO or IOT? Consistency!

Page 4, line 106: Why do the authors believe the databases are statistically representative since they explain later in section 4 that they use reduced samples of the databases DB_Palu2018’ and DB_Sunda2018’. For example, from 463 observations in Palu they use 124 observation. I recommend restructuring the manuscript combining section 2 and 4 or put the final databases used in section 2. Is the number of timber buildings enough to be statistically significant?

Page 5, line 142 f.: Please specify the appropriate kinematic and dynamic boundary conditions for the interfacing layers.

Page 6, Eq. 7 ff.: In equation 7 is d a constant? Please define theta.

Page 7, figure 2: It is not clear what the building occupation ratio is. Please introduce a definition. What do the polygons in 2 (d) represent? Please add a legend. Are those cells 100% covered? What about the rest of the layer? 0% occupation? Please clarify!

Page 7, line 180: Please avoid having two letters for the same variable.

Page 8, figure 3: It is unclear how the corrections are applied to the Digital Surface Model.

Page 9, figure 4: What do mean by profile realized? Are those measurements?

Page 10, figure 5: Do the triangles represent single buildings? It is probably better to choose a representative area on a scale with many surveyed buildings like a city or village instead of large parts of the coast. The figure now does not illustrate well the flow depth close to the buildings. What about the flow velocity plots?

Page 10, line 213: Is landslide S8 oriented towards the city? Isn't that the slide that was captured by the pilot in the departing plane? Isn't the slide direction perpendicular to the bay?

Page 10, line 215 f.: What is meant by landslide ratio of 1.2?

Page 10, line 217 f.: Why do you only overlay 175 traces?

Page 10, line 220 – 225: This section is not very clear.

Page 11, table 3: What are the sources for the volume of the landslides?

Page 12, figure 8: What is the source of the topography and bathymetry data for the DEM with 1 m resolution.

Page 12, figure 9: Figure is misspelt. The figure demonstrates that the model does not well represent the observations. What is an S8 ratio of 1.2?

Page 12, line 237 – page 13, line 239: I suggest putting the number and type of buildings for all locations in a table.

Page 13, table 4: Please specify why you only use 124 out of 463 flow depth values for Palu.

Page 13, line 252: Please put 'Global Earthquake Model' the first time you use the abbreviation.

Page 13, section 4.1 & section 4.2: What is the difference? First, you identify the explanatory variable for building damage. Then in section 4.2 you include the damage states and the model selection. I believe you could make this section 4 much shorter by focusing on the relevant information. I suggest preparing a short and concise paragraph on the statistical methods used and then present the results for each site. I also miss a short introductory phrase to the Akaike Information Criterion (AIC) and the likelihood ratio tests you applied.

Page 14, 284 f.: There is something wrong in this sentence 'The intercept of the curves for the two material types appear be sustainably different.'

Page 15, 16 and 17, figures 10, 11 and 12: I recommend plotting the confidence intervals with lines only without shaded areas because in the overlapping areas you get different colours than depicted in the legend. In case some reader would like to print the manuscript in black and white, only it will be more illustrative. Furthermore, I suggest introducing a symbol for the variable flow depth. I believe it is better to delete the word material in the legend since it creates some ambiguity with the symbols used. Also, confined masonry is not a material; it is a construction or building type. In figures 10 and 11, you use the same symbol for timber and reinforced concrete, I suggest selecting a unique symbol for each construction type.

Page 15, line 301: Instead of material I would suggest using construction or building type. Confined masonry or reinforced concrete are construction types, not materials.

Page 15, figure 10 and line 293, etc.: You use a couple of times ‘in order to’. There is no need for using ‘in order’, a simple ‘to’ is enough.

Page 16, line 305 f.: Instead of ‘GLM models are finally fitted to DB_Thailand2004 in order to construct fragility curves and their 90 % confidence intervals for the three individual damage states, as depicted in Fig. 12.’ I suggest to writing: ‘We fit GLM model to DB_Thailand2004 to construct fragility curves for the three damage states and plot them with their 90 % confidence interval in Fig. 12.’. Generally, I suggest avoiding passive voice use because of this increase the readability of a manuscript.

Page 17, Eq. 18: The indexing of the model equations is not precise. Please check the standard of the journal.

Page 18, lines 329 – 337: I suggest depicting the functions in exemplary plots, and possibly you could simplify the verbal description. Line 331: Eq. 8.2 does not exist.

Page 18, line 348: Please explain what is meant by AIC values.

Page 19, line 380: Please define hydrodynamic force and explain how you compute it.

Page 20, line 409 – 415: This is much text for a simple conclusion. Please simplify.

Page 20, line 416: IO. Please be consistent.

Page 21, line 434 f.: Here you write: ‘The curves suggest that confined masonry-type buildings have higher performance than timber structures.’ On page 19, line 388: ‘A timber building is found to sustain more damage than a confined masonry one for all damage states.’ Please clarify.

Page 22, figure 13: It is unclear which curve was produced by Syamsidik et al. (2020). The red dashed line is Syamsidik et al. (2020)? Please put the reference also in the legend of the figure to be exact. Can you explain why the curve of Syamsidik et al. (2020) is that different to yours? I do not understand why you put the data points in the figures. Is there no better way to illustrate your data? It is hard to distinguish the data points some of the red points that superimpose orange ones are hard to see. In figure 13 (a) and (b) why are the points distributed differently? Moreover, I propose putting 'confined masonry buildings' in the figure. Maybe in the legend or in a figure title. Otherwise, the reader may mix up the figures of confined masonry and timber. I suggest plotting the observed flow depth's fragility curves with the ones of the simulated flow depth. Otherwise, it is hard to see any difference. Is there a difference?

Page 23, figure 14: Please consider the comments of figure 13 also for this figure 14. I suggest putting 'timber buildings' somewhere in the figure. Maybe in the legend or a figure title. Otherwise, the reader may mix up the figures of confined masonry and timber.

Page 24, line 458: Please review and correct the following sentences: 'The fragility curves based on observation and simulation are similar enough to consider the computed curves as functions of the hydrodynamic features of the tsunami reliable (Fig. 15c,d).'

Page 25, figure 15: Please consider the comments for figures 13 and 14 for this figure. I suggest plotting the fragility curves of observed and simulated flow depth in the same graph (hence combine (a) and (b)).

Page 25, table 11: Why do you use tsunami intensity measure values different from Table 10? It makes them less comparable.

Page 26, line 472: If Koshimura et al. (2009a) building fragility curves are for mixed buildings and your curves are for mixed buildings are they comparable? What are the percentages of each construction type?

Page 26, line 488: How do you explain why Banda Aceh buildings are destroyed at 6 m/s flow velocity whereas in Palu and Sunda Strait buildings sustain this flow velocity?

Page 27, figure 16: I suggest adding the reference to the legend of the figure.

Page 28, table 13: It is not proven that a non-seismic source triggered the Palu tsunami. You need to explain what the symbols + and – mean in the lines liquefaction and ground shaking. I suggest changing the line 'Construction material' to 'Construction type'.

Page 28, line 514 f.: Why do you think the Sunda Strait event buildings reveal a better performance than in Khao Lak? How many buildings were affected in Sunda Strait with flow depth values larger than 5m? How much area was inundated with flow depth values larger than 5 m?

Page 28, line 527 f.: Please be aware that the largest areas of liquefaction are located outside of the tsunami inundation area. Please see Watkinson & Hall (2019) and Syifa et al. (2019). Although Sassa and Takagawa (2019) identified some small liquefaction areas near the coast, you need to quantify how liquefaction processes effectively damaged many buildings in your database.

Page 28, line 529: It is not proven that only landslides generated the Palu-tsunami. I suggest changing the phrasing' non-seismic source'.

Page 29, lines 532 – 536: The problem here is that your estimates of the flow velocity are based on numerical modelling for the Sunda Strait with 20 m resolution and for the Palu event with 1 m resolution. This makes them hardly comparable.

Page 29, lines 536 – 540: This argument is not enough to conclude that liquefaction was the principal cause for structural destruction. Mas et al. (2020) write that tsunami hydrodynamic, and debris impact forces may have been the principal causes of failure and collapse in Palu Bay's waterfront area.

Page 29, line 540: It is pure speculation that liquefaction episodes are mostly responsible for the building damage. Prove it.

The rest of the manuscript is based on this hypothesis. Consequently, you should present some facts or rewrite the last part of the discussion and conclusion.

References:

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