EGUsphere, referee comment RC1
https://doi.org/10.5194/egusphere-2022-206-RC1, 2022

Comment on egusphere-2022-206

Anonymous Referee #1

Referee comment on "Empirical tsunami fragility modelling for hierarchical damage levels" by Fatemeh Jalayer et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-206-RC1, 2022

This manuscript presents their newly developed tsunami fragility functions using previously published survey data. I appreciate the author`s attempt in applying advanced statistical methods but still lack of advertising (or being distracted by too detailed explanations on other parts) benefit of the proposed model. In addition, I strongly feel that it will be more useful if the authors add another data set to compare results when using the proposed method. For example, building damage data from the 2018 Sulawesi tsunami can be accessed from this article.

Characteristics of Tsunami Fragility Functions Developed Using Different Sources of Damage Data from the 2018 Sulawesi Earthquake and Tsunami, Pure and Applied Geophysics, 177, 2437-2455.

Please find below for some suggestions.

Abstract: Please add some major findings also in the abstract. Currently, your abstract only explains introduction and method.

Section 1: Tsunami fragility functions were actually developed following earthquake fragility functions. I believe that it would be good to also briefly review to explain if the proposed method (in your study) had been used in developing earthquake fragility functions.

Lines 72-80: I feel that these sentences are more suitable for discussion part. Instead, the authors shall state clearly their research objectives and framework at the end of this section.

Section 2: I would suggest adding small explanations on limitations of the classical linear regression method at the beginning of this section.

Table 1: Although this is not your own data, I wonder how such detailed statistical analysis model works with data with small sample size. I also feel that the damage level description between D1 and D2 is not so clear "non-structural damage" vs "significant non-structural damage". Did they use 50% more or less to classify? Similar concern for D2 and D3. I wonder how large the bias the damage classification at the site during field survey. Such misinterpreted damage definition might largely affect when the sample size is very small.

Section 3: I think the word "flow depth" or "inundation depth" is more suitable than the currently used "water height" as I guess that the authors mean that is water height above ground level. Which model is comparable or the same as those used in Reese et al. (2011)? I would suggest discuss clearer on how the accuracy has been improved by this new work. From a general look, all results in Figures2-4 show similar results with no-cross and width of error bands.