Dear colleagues,

meanwhile I received the referee opinions of this second round of review.

As you can see, referee #1 who was already involved in the first round has some minor comments, mainly based on final clarification of the manuscript content and some linguistic remarks. I have the feeling that you will be able to address these issues during a final minor revision.

Referee #3 who was not involved in the first round raised some more general questions. The overall debate in physical vulnerability assessment is whether this is based on deterministic/empirical loss functions or on more detailed fragility curves based on an individual assessment of structures. The latter, as far as I understood also from the first review round, remarks to referee #2 (Bret Webb), was not possible due to data protection and other restrictions associated with the exposure data you used. This is an overall challenge in vulnerability assessment, and has also been experienced by many other fellows working with insurance claims and/or aggregated loss data (to give an example, if you only know the degree of loss composed from the building value and the loss height, but have not additional information on the building type, this is the way to proceed). So my suggestion with respect to the comments of referee #2, though they are very valuable, is to add a paragraph in the final section on the limits of your approach. This could also been done by extending the last paragraph of Section 4 (see as an example Fuchs et al., 2019, section 3).

Based on the options of both of the referees and my own judgement I consider your work as timely and publishable, however, I kindly ask you to re-consider the issues raised above in terms of a minor revision before I will accept your very interesting piece of work.

I am looking forward to receiving your revised manuscript.

Kind regards,

Sven Fuchs (Editor NHESS)

Response:

Dear Sven.

Yes, we added a paragraph at the end of section 4 explaining the limitations of our approach regarding the available information.

We would like to thank you for the help over these months.

Best,

Andres

Reviewer3

The paper develops damage curves using the insurance claim data. Although the work should be of great interest, I expect rigorous efforts from the authors before it can be considered for publication in NHESS. My suggestions are as follows:

Please use what type of structures you are considering in the study. Since the classification of structures is quite risky due to the variation in the attributes, classifying data into a bin requires a very high level of understanding. Please mention how did you define classes of structures, also please explain the attributes of structures and the reason behind homogenization. Insurance claims will be randomly done for all structures and creating damage curves for structures does not guarantee representativeness. Rather, it would be imperative to disaggregate the data into several classes and define fragility functions or vulnerability functions. An introduction of vulnerability/fragility is also welcomed.

I suggest the authors use fragility curves rather than damage curves. At some point, these terms differ at least when we go for the classical definition of fragility. Usually, if you say damage curve, it becomes rather deterministic and may even miss the accumulating nature. What I mean is, higher damage by default carries the lower one.

Since flooding damage is not confined to Europe only, please include a global literature review on empirical fragility/vulnerability models such as:

- From flood risk mapping toward reducing vulnerability: the case of Addis Ababa by De Risi et al.

- An analysis of physical vulnerability to flash floods in the small mountainous watershed of Aceh Besar Regency, Aceh province, Indonesia by Azmeri and Isa

- Catchment-scale flood hazard mapping and flood vulnerability analysis of residential buildings: The case of Khando River in eastern Nepal by Thapa et al.

- Multi-hazard vulnerability of structures and lifelines due to the 2015 Gorkha earthquake and 2017 central Nepal flash flood by Gautam and Dong

Moreover, a comprehensive literature review can be found in the paper by Fuchs et al.: Recent advances in vulnerability assessment for the built environment exposed to torrential hazards: challenges and the way forward.

The authors should present a comprehensive discussion on why the least square approach is used. For me, the maximum likelihood estimation is also impressive due to several merits. A more detailed explanation of statistical modeling and selection of intensity measures is to be provided in the manuscript.

How did you estimate the damage level of each structure and on what basis did you estimate the damage level or damage factor?

Response:

Dear reviewer 3.

Thank you for your comment. The reason why we decided to use the definition of damage curves instead of fragility curves is that we did not have more information than the damage ratio (the ratio of claim value to total insured value). In the previous round of discussion (that you unfortunately missed) this question was addressed and in the reply to the reviewers, was explained that unfortunately due to the new European data protection policy (GDPR - General Data Protection Regulation), the AXA insurance company cannot share more information with us than currently already listed in the paper.

Nevertheless, at the end of the section 4 one more paragraph was added mentioning this limitation on the paper as the editor recommended based on your comments.

Regarding the question related to the parameters adjustment method, in the present paper we use the L-moments method (included in the package lmomco in R) which is a common method to fit parameter distributions, and we mentioned other methods exist, like the least square method, or as you are now commenting, the maximum likelihood method for this task. It is true that the selection of the method can influence the results, but the scope of the present paper is not to delve into the statistical methods for damage curve development, but to research on the variables that correlate most to the damage ratio due to storm Xynthia. Indeed, a previous second reviewer commented on how other different distribution functions would affect the results, and as a complementary analysis, two newer distribution functions were added (and the analysis was repeated), showing that the water depth (d) and the total force are still the most representative variables to develop the damage curves, and more probably this result will not change by using a different parameter fitting method. Nonetheless, at the end of section 3 a small sentence was added regarding this topic.

Bret Webb (Reviwer 2)

The authors have done a good job addressing my comments, questions, and requests. I have a few very minor comments/questions below, but I don't think these warrant any sort of substantial revision of the manuscript.

I'm not sure that the authors improved the description of Xynthia as was suggested in my comments. In fact, there appears to be less text describing the storm event now. I still think it would be worthwhile to have a more thorough description of the event and its impacts to the coast and infrastructure.

Now that you have clarified the nature of the swell wave height measurements used in Figure 5, did you consider exporting the corresponding swell wave height predictions from SWAN instead of simply using the significant wave height results? I only ask because the model-data comparison may be much more robust than your analysis indicates.

On the figure of the left will not improve on the right will improve

There are some minor spelling, grammatical, and typesetting issues throughout the manuscript that I'm sure the publishing/layout staff will find so I won't list them here.

Response:

Dear Bret,

Thank you for your comments.

In the previous version we increased the description of Xynthia storm, but we re-structured the paper, therefore maybe there is a feeling of less text. Nevertheless, now a new paragraph is added in section 2.1.2 describing the storm and the damages/casualties for the event a bit more.

It is a good point about the Swell, unfortunately since in figure 5 left, SWAN significant wave height is underestimating the Swell and Figure 5 right is overestimating (comparing hsig and swell), therefore this will not improve the analysis on average.

For this final version, co-author Jeremy Bricker who is a native English speaker has reviewed the English.

Thank you all.,

Manuel Andres Diaz Loaiza et al.