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Author(s): R. Hasanzadeh Nafari, T. Ngo, and W. Lehman
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Anonymous Referee #3

Comments to the Authors

The authors aim to quantify the physical consequences of floods to residential structures, i.e. the direct damage to residential buildings, in Australia. For this purpose, they conduct a case study on two cities in Queensland/Australia. They apply three damage models to estimate residential building damage, one from an Australian Authority (Geoscience Australia), the well-known HAZUS methodology from the U.S. and their own newly developed model. They validate model results with official damage data from the respective Queensland authority.

The topic is not new, as similar studies have been conducted before, but not for this region. Furthermore, validating damage models with actual data always deserves credit. Hence, it is desirable, that the work will be available for the scientific community.

In the present form, there are different major obstacles that prevent the publication of this paper, and minor imprecisions:

1 The results do not fully meet the stated objectives

The stated objectives are:

- quantify the direct physical damage to residential structures that are prone to floods in Australia.

This is fulfilled: The authors develop a model for quantifying flood loss for several building classes and validate the results with actual loss data

- assess the applicability and transferability of overseas flood damage models to Australian (geographical conditions):

The authors use two other models (except their own) for damage estimation and compare the results. One of these other models is not an overseas model. They do not describe the U.S. flood damage model and its approach in enough detail. Points that may hamper the applicability and transferability and that may explain the differences in performance are neither analyzed nor discussed.

2 The wording, and especially technical terms are not always used with care. Generally, the paper lacks precision in describing data and models and using technical terms, e.g. validation and calibration sometimes seem to be mixed up.

3 The literature, while sufficient in amount, does not always support the statements, e.g. p.3824, l. 25: the cited papers don't support the view that flood probability and value of the exposed property have increased exponentially.

4 For model development, calibration and validation the authors use the same dataset. This is not scientifically sound. Validation should use an independent dataset. If such data are not available, this kind of dataset should be created using methods like split- sample or leave-one-out cross validation.

5 The empirical data do not look like real world loss data.
I want to put special emphasis on this point. The empirical data are the foundation of this work as the new model is based on this dataset and validation and model comparisons use these data. The data description lacks detail (how many cases are used, number of loss cases in each class) and is very complicated. It is not always clear who (the authors, the survey data provider) has done what in the process of modifying and selecting the data that were finally used. Looking at the unnatural distribution of losses in figures 6, 7, and 8, I was wondering, whether the original dataset from the Queensland authority already give typical damage per water stage/range. This would represent a somewhat empirically derived model itself. More likely, the process of transferring the qualitative loss categories back into absolute losses and then relative damage/damage ratios obscures the real loss, suffered by the affected buildings.
The empirical cases generally do not look like real loss cases (e.g. Figs. 6-8; Tab. 3, 2nd column): Residential damages tend to show huge variance, but the presented data look very "clean": Little to no spread, no outliers, and a clear order (higher water stage – higher loss ratio).

Recommendations:

In my view, it is indispensable to describe the empirical data in more detail (e.g. provide a table giving number of loss cases, number of loss cases used, cases per building/construction class, and maybe plot all cases in a figure giving water stage and damage). In addition, the data transformation process is not clear enough. So far, the reader cannot really judge the suitability of the dataset for the purposes, it is used for. Additionally, the authors should discuss the quality, aptitude and uncertainties in the loss data and see, whether the applied analyses (e.g. error calculation) are reasonable using these data.

The manuscript might become acceptable after major revisions, but, given the amount of work that would be necessary, a resubmission probably is the better choice.

I encourage the authors to put emphasis on understanding and rethink, what can (and cannot) be done with these data and adjust the scope of their study accordingly.

Some additional minor points:

Tab.2: Why is r=1.5 not tested? It looks visually like a good fit (fig.4), definitely better then r=2.5.

Tab.3: Is it feasible to exclude underestimation (results <1) from the error statistics?

Figures 1 and 2: The location maps (figs 1+2) give very little and purely administrative information. At least hydrology and elevation should be part of the study area description, maybe even inundation (if available).

In Tab. 1 and Fig. 3, the subassembly classes do not fully match. How do you separate the relative value of foundation and below first floor?

Best Regards!