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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Development and assessment of uni- and multi-variable flood loss models for Emilia-Romagna (Italy)" *by* Francesca Carisi et al.

Anonymous Referee #1

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Review of the manuscript NHESS-2017-342 The paper describes the development of flood loss models on the basis of a remarkable dataset of observed flood losses. This dataset was used to develop different kinds of loss models and to validate these models, as well as other models available in the literature. In general, the paper presents an interesting study. The novelty lays in the approach for developing a new approach for multi-variable flood loss models. However, while reading the paper, some questions arose. With some explanations added, the paper will be of interest for the flood loss modelling community. The main and principle question that arises is, if the random forest approach is sensitive to heteroscedasticity in the data. As figure 10 shows, the deviations from the observed data vary with magnitude. It is highly recommended to

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test the data for heteroscedasticity and to tackle with this issue in the development of the models if necessary. It would be of interest how the residuals are distributed. Furthermore, as in the introduction is stated, the slope of the floodplain is very regular. Thus, flow depths vary only in case of backwater effects of hydraulic obstacles. Hence, flow velocities in this relatively homogeneous case study may not be considered as independent variables (dependent on flow depth). I don't know how the flood model used for the analysis computes velocity and flow depth. Anyway, they are interlinked through the model used. However, this is a hypothesis and the contrary should be demonstrated. While looking at Fig. 11, a question arises if both cases Bastiglia and Bomporto do have relatively homogeneous flow depths inside of their samples but differs remarkably between both. This may lead to an overrepresentation of a certain flow depth interval and hampers the transferability of a model calibrated on one case study to the other case study. Figure 1 strengthens this observation, although the flow depths are not visible below the clustered points. I recommend showing a box plot of the flow depths at the single buildings for both case studies. The authors are asked to assess the reliability of the flood loss estimations (in monetary terms) by the home owners immediate after the flood event. I suspect that all home owners have the competency for estimating the damages to their buildings as professionals have (insurance experts and craftsmen commissioned to restore the building). The authors should describe how these estimations were "verified for authenticity" by the administration. If this verification was made following a reliable approach, the refunded value should be used for the analyses and not the estimations. Another weak point is the use of the market value for the estimation of the building's values. It is not described, if this value comprises the cost of the land too. Furthermore, it is not documented if this value is given for the area of the building footprint or for the living space that should be multiplied by the number of floors. The comparison between different flood loss models should consider the used base value for assets. It would be of interest which approach the authors followed for the geolocation of the loss data. p. 8, ln. 19: is the size of 1 to 200 m for element length or area of the element? p-11, ln. 26 chapter 4.2.1. It is not

defined what “best performance” means here. Results section. The model structure of the multi-variate model, i.e. the outcomes of the random forest analysis, should be described. Which parameter with which weights have been identified and structure the prediction model. In its present form, the reproducibility it is not given. One solution could be to adapt Fig. 4 and insert the resulting model structure. p. 14, ln. 28-29. In addition to the comparison of the predicted losses with observed ones, it would be of interest splitting the dataset stochastically. Together with the comparison between both calibration datasets with the opposite case study data, the conclusion of the transferability could be grounded more reliably. A sensitivity test of the SMV model should be done. p.17, ln.1-5. There is a conflict between text and figure 11. In the text, the grey dots are described as observations. In the figure, no blue dots are visible as mentioned in the text. p.17, ln.10. “in the sake of brevity”. This can be shown in the appendix p.18, ln. 16. What is “Sec. 8”? Fig. 1: The authors are asked to explain why they mapped only flow depths >10 cm. Are the analyses based on the full range of flow depths or are flow depths >10 cm generally omitted throughout the study?

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