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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "The role of spatial dependence for large-scale flood risk estimation" *by* Ayse Duha Metin et al.

Anonymous Referee #1

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The authors present a modelling exercise where they compare and analyse flood damages under three different assumptions regarding the spatial distribution of return periods of flood peaks throughout the catchment. They apply a complex modelling chain that goes from synthetic weather generation to flood damage estimates. The chain is composed of a weather generation algorithm, a rainfall-runoff model, a 1-D flood propagation model, a 2-D spatial inundation model and a damage estimation model. Their emphasis is on the comparison of the risk curves obtained throughout the catchment for different assumptions of spatial dependence of flood return periods, taking as reference case the option of modelled dependence. The authors present results of their analyses for the entire Elbe catchment and for its division in 29 sub-basins. They infer conclusions on the over estimation or underestimation of flood damages with respect

to the reference case.

The topic is relevant for the audience of NHESS, the objectives are clearly identified, the methodology for the analysis is adequate and the conclusions are relevant and correctly supported by the results and discussion. The analysis clearly shows the striking differences between the options of independence and complete dependence with respect to the reference condition of modelled dependence, and therefore the objective of the paper is clearly achieved. Therefore, I believe the manuscript deserves publication in NHESS.

SPECIFIC COMMENTS The authors are addressing a formidable task. They are reporting results obtained with a complex modelling chain that probably took years of work under the constraints imposed by the length of a research paper. It is only natural that some parts of their work or the models used have necessarily been left unexplained. I am suggesting a few points where I believe the reader would benefit from some additional details, such as the following:

a) On page 7, lines 161-163, the authors report that the weather generator was calibrated for the region of the Elbe basin with observed data and "was shown to capture precipitation extremes well". I think the entire analysis is dependent on the quality of the time series produced by the weather generator, particularly regarding spatial correlation and seasonality of extremes, which are very challenging. I think the paper would benefit from a more elaborate discussion of the spatial dependence of the extremes produced by the weather generator as compared to observations.

b) The topology of the model should be explained better. On page 7, line 186, the authors state that overtopping flow is calculated from the 1-D diffusive wave model. On line 170, they say that the runoff is routed by the Muskingum method and aggregated at the basin scale. This leads me to think that the possibility of overtopping is not contemplated in the reaches modelled by Muskingum. From Figure 1 I gather that the 29 sub-basins under analysis are actually composed of smaller units, which are the

ones simulated in SWIM, but this is not mentioned in the text. I think the manuscript would benefit form a brief discussion of which rivers are included in the overtopping analysis and which criteria were used to delineate the SWIM basins.

c) The operational definition of return period should also be discussed in detail. I first thought that the return period referred to peak flow and was estimated from the 10,000 year simulation in each location. However, on page 9, line 248, the authors say that they refer to a T-year flood event as resulting in the T-year damage. In addition to peak flow, flood damage is also affected by hydrograph volume, which is very relevant to determine the extent of the inundated area, at least for flash floods. Perhaps the authors might consider a brief discussion of this issue.

TECHNICAL CORRECTION From the formal standpoint, the paper is very well written, correctly organized and adequately illustrated with figures. Comparison of Figures 4 and 6 is handicapped by the fact that the return period is shown in natural scale in Figure 4, while it is shown in logarithmic scale in Figure 6. I would suggest using the same type of scale on both figures, if possible.

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