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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Efficient pan-European river flood hazard modelling through a combination of statistical and physical models" *by* Dominik Paprotny et al.

Anonymous Referee #2

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The manuscript aims to derive a pan-European flood map using the statistical approach of Bayesian Network to derive flood discharge and a 1-D model to propagate flood discharge (using steady state approach) and identify flood prone areas. The same procedural chain is applied in order to capture the effect of climate change on the extension of flooded areas. The proposed method is compared with another pan-European flood map and with different local reference maps produced by different European countries. Surely the authors carried out an important work linked to a EU project and the topic is very interesting. However I have some concerns about the proposed procedure:

1) The authors used a 1D model instead of a 2D model (used in JRC map) – in my opinion the use of 1D model in relatively flat areas like the plains of central Europe

is totally inappropriate. The consequent delineation of flood areas is invalidated by the use of the proposed procedure. Maybe the authors could describe a couple of local study cases (flat areas) in which they compare the use of 1D model against a 2D model.

2) I'm no English mother tongue but some parts of the paper are very hard to read and to understand – I suggest the use of English native speaker to re-read the paper and correct it;

3) I suggest to describe in detail in the paper the structure of Bayesian network used to estimate flood discharge since it is important to understand its structure and its capability if compared to other approaches aimed to the derivation of flood discharge (hydrological models or regionalization procedures).

4) What kind of resampling was used for the reference maps? It is important to point out this aspect since it affects the comparison with the TUD and JRC maps. Also the choice of 1.5 km buffer should be better explained.

5) The authors declare (page 7) that river water level model was calibrated comparing TUD map to JRC map (which was no calibrated). This calibration procedure is a weak point because the authors assume implicitly the need of another existing map to develop their approach. Moreover that introduces in the procedure a sort of bias since the model is forced to reproduce a map, which is not necessarily true. I suggest to not calibrate the model (trying to assess the parameter with ancillary data) and then to carry out the comparison with JRC map and reference maps.

6) In literature other metrics can be used in order to avoid the problems related to the use of Icor. For instance, the authors could use the Kappa index, (Cohen, 1960) which allows a better evaluation of the capability of proposed approach when compared to a reference map.

7) The explanation of the two map sets ("without flood protection" and "with flood pro-

tection" – page 8) is very unclear. The authors cite Scussolini et al. 2016 as reference but I think that a clearer explanation is needed to better understand and analyze the results.

8) I'd remove equations (1) and (2) since the DSV equations are well known.

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