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 #1

Received and published: 19 January 2017

The article by Paprotny et al proposes a new simplified methodology to produce flood hazard maps at pan-European level, and is applied to estimate the flood hazard in the current climate and under a few possible climate change scenarios. The overall structure of the article is sensible, though the quality of the writing should be definitely improved. On several occasions I had the feeling that the authors meant something different than what is actually written. In other cases one can guess the meaning but the sentence structure and terminology should be improved. Some examples (WHICH SHOULD BE REPHRASED) include lines 7 of page 1, l 24-25 of p1, l 27-28 p3, l 24-26 p5, l 3-5 p7, l 12-13 p7, l 16-18 p7, l 5-6 p8, l3-4 p13.

I also suggest a review by a native English speaker to correct the typos and punctuation

errors.

Besides what mentioned above, I do not see major reasons for not publishing this article. The proposed approach is relatively novel, though in practice it reflects into using a known method (i.e., 1d hydraulic modeling with planar approximation of water levels), applied to a statistical method used to derive peak discharges (Bayesian Network-based), which was published previously by the authors. As a main comment I suggest the authors to clarify this, including the possible limitations of using a non-volume constraining hydrograph into the hydraulic model (based only on peak discharge), coupled with a non-volume constraining planar approximation of the water levels. Indeed it's clear from the skill scores that results tend to overestimate those of the other regional and JRC maps in terms of flood extent.

Additional Specific comments are listed below

L8 p1: alternative to what? Perhaps novel, or new is more appropriate. Also consider the comments above on the "novelty"

L16 p1: why summarized?

L17 p1: increase of 2-4% compared to what?

I suggest avoiding colloquial forms such as "there is a sharp increase", "This is because", "This fact results in", "in one go", "This fact had", "could be observed"

L 21 p1: I'm surprised to see that the keywords don't include words such as "flood hazard" or "inundation mapping"

L 24, p1: I suggest replacing "common" with costly, damaging, catastrophic or similar

L 26 p1: this is probably because climate scenarios had traditionally high uncertainty to produce hi-res hazard maps. However, some studies did that (e.g., Rojas et al, GEC, 2013)

L 4-5 p2: This sounds unexpected. Is that outside Europe?

L 9 p2: "Alert" should be replaced with "Awareness" in both cases

L 16-17 p2: Also Winsemius et al (NCC, 2015) did a similar validation (see Supplement)

L 27 p2: perhaps "continental" instead of metropolitan?

L31 p2: improve

Sect. 2: I'm surprised not to see some more details on the climatic data used to derive extreme discharges (not even the name of the climatic model!). This is likely to be among the main sources of uncertainties in the output. Some dedicated sentences are utterly needed.

L 8, p4: "affected by flash floods", this needs a further justification, i.e., why flash flood prone catchments cannot be studied with the proposed approach.

L 32 p4: the acronym RCP should be mentioned only after its full name description (now in the following line).

L 1-2 p5: please be careful not to mix the meaning of RCP and that of SSP. In RCP the socio-economic change is not explicitly included.

L 8-9 p5: Why the model is not mentioned here (instead of simply "one of the . . . regional models")?

L5-6 p6: This sentence is not so informative, unless you state the possible reasons for including these additional terms, and then the reason for neglecting them in your approach.

L 7-8 p7: Not clear what this sentence means. Please clarify. Also, the following sentence should be justified and the parentheses should be removed.

L 20-21 p7: This sounds trivial: I assume gravity acceleration was considered as constant, while the roughness is not specified (perhaps linked to the land cover?). To be clarified. I see it's partly explained later, hence I suggest removing this information here

to avoid confusion.

L4 p8: lying

L 19-23 p8: This part could be improved.

L 27 p9: remove "from"

L20-22: Indeed a more fair evaluation should first remove the permanent water bodies from the flooded area used to assess the skill scores.

L20-22 p 11: meaning areas below 500 km^2 were not excluded in the TUD map? This part is rather confusing. I suggest some clarifications.

L 31-32 p 11: I was not provided with any Supplement, nor can I see it online. I think those files are missing.

L1 p12: how the coastal flood hazard was counted? I understand it's not an outcome of this study.

L18 p 12: remove "has"

L19 p12: "Elevated" should be replaced with a more appropriate alternative.

L25 p 12 onwards. I would find those numbers more intuitive if expressed as percentage of the initial flooded area and/or of the total land area included in the simulation. Absolute numbers are more difficult to evaluate, especially as it refers to a very large domain.

L1 p13: projected to increase

L 10-13 p 13: Those statements require a supporting reference.

L 16 p13: I think that the use of a climate scenario rather than the real climate is an important assumption to mention here, particularly if it is not bias corrected (not mentioned in the article).

L6-7 p 14: I suggest rephrasing this sentence or simply removing as it doesn't seem a good reason.

L26-27 p 14: This sentence sounds rather speculative, as no real comparison is provided on an alternative method. The computing time is clearly a tradeoff between complexity and accuracy pursued. Also, the point on the computing time is not so strong to me, as those maps are just produced ones and don't need frequent updates such as for real time forecasting applications.

L 2-3 p 15: I would be careful here and reformulate this sentence, as you don't give clear evidence to support it.

Table 1: The river flow is calibrated in several stations in the JRC map, as also stated in l 32-33 p8. Or perhaps here (and in the line above) you mean inundation mapping (instead of river flow).

Caption of Fig. 5: I suggest removing the last sentence. In general I don't think it needs such long description.

Caption of Fig. 8: Please provide the Supplement.

Caption of Fig 9-11: What surprises me the most is that only in those 3 captions is the climatic model used as input data mentioned. This needs to be mentioned before in the methods, together with a dedicated description. In addition, given it's only one model, some comments on the uncertainty of the climatic input should be included.