



Interactive comment on “Maximizing the usefulness of flood risk assessment for the River Vistula in Warsaw” by A. Kiczko et al.

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Dear Anonymous Referee # 1,

Thank you for your comments and suggestions. Our responses will be placed after quotations from your text.

Major comments

1. *“The topic addressed by the paper is an “evergreen” in Discussions the field of flood risk evaluation. The comparison between deterministic and stochastic approaches has been studied by different authors and presented in several papers in the scientific literature. As matter of fact some references are missing so I*

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suggest the authors to include it in the list.”

We did a search of the available literature on the subject and we found an additional paper by Aronica et al. (2012), which will be referred to in the corrected version of the paper.

However we would like to point out that our approach to the problem differs significantly from previous studies of flood risk. For the first time we derived an uncertainty of inundation extent conditioned on the design flood wave, taking into account the dependence of roughness coefficient on flow. When that relationship is not taken into account, the input flood wave uncertainty and flow model parameter uncertainty are implicitly assumed to be independent. As a result we obtained the total probability distribution of an inundation. The comparison between the probabilistic and deterministic formulation of the problem made on such a basis allows us to raise the question of how the risk related to the river discharge is affected by the model uncertainty. To our knowledge, in recent times only Aronica et al. (2012) deal with a similar problem, although assuming the independence of roughness coefficients on flows.

2. *“Another important point to be commented is, in my opinion, the choice of the hydraulic model. The area interested by flooding is on the left-bank of the river and show typical peculiarities of a 2-D domain (large and flat). Further this area is hydraulically connected to the river when levee breaches occurred. In my opinion, the authors should use a 2-D model to analyse this domain capable to include also a levee breach scheme. I cannot understand how the HEC-RAS has been used to model these breaches. Please supply this information. As HEC-RAS considers weir coefficients to connect the river with storage areas the authors should have been considered also these parameters during the calibration phase. The authors stated they used a 1-D model for speed-up the calibration and uncertainty analysis as the use of a 2-D model is time consuming. Actually, I don't agree with this as the flood risk maps are essentially prepared for planning and risk man-*

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agement purposes, so you can have plenty of time to prepare them. They are not used for real-time flood control.”

Our strategy was to identify areas that are under the threat of flooding and in our analysis we neglected the probabilistic nature of embankment breaching. Therefore for each site it was assumed that a breach will occur (with certainty). The hydraulic computations were performed with a 1-D steady flow model. The resulting water levels were projected to the 2D inundation extent using DTM. In such an approach we do not consider a weir type flow between the river channel and floodplains (it is equal to 0). Of course, this is a coarse assumption, but in subsection 5.4 we presented detailed but maybe not sufficiently clear explanations.

To be brief, we conduct a series of numerical experiments involving a 1D unsteady flow routing model coupled with storage cells (HEC-RAS, UNET). As you have mentioned, the link between the channel and flooding area is described with the weir formula. To validate our stead-state assumption we investigate how the breaching at three different flooding sites will affect the water levels along the river and inundated areas themselves. The analysis revealed that only breaching for the largest cell would significantly affect the maximal water levels. Two others would become filled up enough quickly, due to their small capacity, that computed water elevations follow these in the channel (with reasonable accuracy).

In the case of the weir coefficients, they were absent in the steady flow mode – because of the zero-flow assumption between channel and flooded areas. Experiments with the unsteady flow model were performed for standard values, as direct identification of their values was impossible due to a lack of necessary observations. However, please note that our analysis is performed for sites (the two smallest), whose size and shape allow us to assume steady flow conditions. This is also a justification for the 1D model, as under such conditions we can expect that 2D solution would lead us to similar conclusions. Here we can refer to studies of of e.g. Horritt and Bates (2002) and Chatterjee et al. (2008), where 1D

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and 2D models were applied to flood inundation mapping, giving (under specific conditions) similar results.

Regarding the justification of the use of the 1-D model, we would like to explain that the main aim of our paper was "maximizing the usefulness of flood risk assessment", and not a comparison of deterministic and stochastic approaches "per se". We wanted to present a simplified methodology that makes the probabilistic flood risk assessment feasible on a larger scale than a single case study. That was the reason for applying a 1-D rather than a 2-D model and comparison with the commonly used 1-D solution that does not take parametric uncertainty into account. As far as we know, only 1-D models are used for the routine derivation of flood risk maps. We do not agree with the reviewer's opinion that the time of computation is not relevant. The stochastic approach requires hundred of thousands of computations of a flow model, which must take months of computer time for a single case study when a 2-D model is used. Moreover, the introduced dependence of roughness coefficients on flow can be also applied in a 2-D model and further work is planned in that direction. We shall improve the introduction to make the purpose and the innovation of the approach presented more clear.

3. *"Last but not least, the paper is not clear in some parts so it needs a careful language check before re-submission to fix up some problems with English"*

The paper was checked by a native English speaker working in science. However, we agree that we should improve on the clarity of the text.

Specific comments

1. *"Page 2699, line 6-7, What do you exactly intend with the expression "parameterisation of the flood frequency curve...."? This lines are not clear, please rewrite and explain."*

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We are grateful to the reviewer for noting that point. The sentence will be changed to: "Three different types of uncertainty related to inundation mapping are discussed: flow model parametric uncertainty, uncertainty related to flood quantiles of 1 in N year flood and uncertainty of maximum annual flow."

2. *"Page 2702, line 3, Why here you consider the design flood normally distributed as in the paragraph 4 you instead consider other different distributions? Please clarify. 2702, line 4, "...and a variance..". May be a letter or a symbol is missing"*

The sentence should be: Further on we assume that the predicted 1-in-N year design flood flow Q has a log-normal distribution with a mean value Q^* and a variance σ^* derived from the observed maximum annual flow records (see Sect. 4).

3. *"Page 2703, equation 5, Deterministic means you have only a single realization of the model if you regard as "the realization" Not necessarily, this is the expected value of the possible distribution. Consequently I cannot understand this equation and, also, the statement in the lines from 12 to 17 in this page. Please clarify."*

The sentence from p. 2702 line 26-p. 2703 line 1 will be changed to:

If the flow routing model, used to transform the input flow into inundation extent, was linear, both in regard to flow and model parameters, the inundation extent obtained using a 1-in-N year input flow (so-called design flood) would give a 1-in-N year inundation extent, which is the aim of the flood risk mapping. In mathematical terms, only for a linear model can the operator of expectation move places from in front of the operation into the variable. This requirement might be written in the following, general form:

$$E(G(\mathbf{m}, Q)) = G(E(\mathbf{m}), E(Q)) \quad (1)$$

where $G(\mathbf{m}, Q)$ is an operator (flow routing model) acting on the stochastic variables (\mathbf{m}, Q) and $E()$ denotes the expectation, but other operators can also be

used (i.e. max).

As mentioned at the beginning of this section, flood extent is derived by an interpolation from the maximum water levels predicted for the assumed design flood wave. Thus the transformation of the input flow into inundation extent is nonlinear in respect of both flow and model parameters. It depends on the channel and floodplain geometry, roughness coefficients and on the flood wave amplitude, which additionally undermines the linearity of the flood wave transformation problem.

4. *“Page 2703, line 18-23. This choice will increase significantly the uncertainty of flood predictions! As I said before why have you not used a 2-D model in a MC analysis to derive the probability?”*

As explained above, the aim of the paper is to decrease the gap between theory and practice. The proposed approach described briefly in lines 18-23, was further described in the "comparison and discussion of the results" section (see also Fig. 9). The approach seems to be simple and it can be applied to either 1-D or 2-D flow routing models. It does not increase the uncertainty but makes it more visible. Figure 9 shows that the uncertainty of the relationship between inundation extent and discharge is very large and grows with the discharge value. It indicates that further work towards decreasing that uncertainty is required. A sentence will be added in the discussion to stress that problem.

5. *“Page 2704, line 19, change in “..that also takes into account..” We shall modify the sentence.*
6. *“Page 2707, line 8, what does the word “amplitude” mean regard to a flood wave?”*

We meant the amplitude in the discharge domain. We shall change this into “the maximal discharge” to make it more clear.

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7. “Page 2708, line 1, Please add the reference for the WRC recommendations.”

The reference will be added.

8. “Page 2709, line 20-23. This explanation for the roughness coefficient behavior in relation to the vegetation cover is too simple. Have you considered previous studies on this area which refer to this problem? Please supply a clearer explanation.”

We presented a very short discussion referring to the fact that estimated values of Manning coefficients depend on flow as we wanted to avoid any speculation. The subject is complex and requires specific studies (Yen (2002) and Mugler et al. (2011)). Unfortunately we do not know of any research on that problem for the study area and we plan to pursue that subject in the future.

9. “Page 2710, equation 6. How the two parameters a and b have been calculated? Please supply this information - Page 2710, line 12, change “possibly” in “possible”.”

The parameters presented in Figure 3 were computed by minimizing the model residua using a simplex algorithm. A description of this process is missing and of course will be provided: The relationship between discharge and roughness coefficients, presented in Figure 3, was elaborated on the basis of optimal parameters computed for maximal annual inflow rates. The data period includes 27 events in the years 1984-2010. For each set of an inflow and water levels at a river gauge, a minimization problem, in a form of a square residua of the model, was formulated. The dependency between roughness parameter, computed in this way, and discharge revealed a linear character. Therefore we decided to parametrize this relationship with a function defined in formula 6. The limits at Q_1 and Q_2 , respectively minimal and maximal discharge rate in the data set, were introduced to avoid the extrapolation beyond observed data.

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In the uncertainty analysis it was assumed that a mean of a posterior distribution for a and b , is equal to values estimated for the optimal, in deterministic sense, roughness coefficients.

10. *Page 2713, line 6, quantiles of exceedance? This expression is strange!*

True, the word “exceedance” will be removed.

11. *“Page 2714, line 3. It is not completely true the deterministic map has no probability assigned. The probability of the risk is the same of the probability of the flood peak (iso-frequency assumption). It is true, instead, the deterministic map has no uncertainty assigned.”*

In order to clarify the sentence in question we have changed it into: As this map results from a single realisation of a flow model for a 1-in-100 year flow, it does not have uncertainty assigned.

12. *“Page 2714, line 23. How have you decided about this probability of exceedance? - ”*

In that sentence we meant that the probability of the map will be 0.5. The sentence will be changed to make this more clear.

13. *Page 2715, line 4. Do you intend the flood peak quantile estimate?”*

Certainly yes, thank you for pointing it out.

References

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