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Hydrology and Earth System Sciences Discussions

Interactive comment on "Effects of rating-curve uncertainty on probabilistic flood mapping" *by* A. Domeneghetti et al.

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General Comments This paper presents an assessment of probabilistic flood mapping in a reach of the Po River in Italy. The analysis focuses on the influence of three sources of uncertainty in developing probabilistic flood maps of the area outside the main river dikes of the Po River. The sources of uncertainty considered are primarily (i) the upstream (inflow hydrograph) boundary conditions, (ii) the downstream boundary condition represented by a rating curve, and (iii) the parameterization of the dike stability model.

The paper is generally well structured and well written, and the results and discussion provide interesting insight into the issues addressed. However, there are also sev-

eral quite fundamental questions that are in my view left unanswered and need to be addressed before publication.

My main concern relates to the aspect of the downstream boundary and its influence on the probability of flooding, particularly in the lower part of the reach. This influence is clearly highlighted by Figure 6, where the marked difference between the two rating curves as established using the two different methods presented influences mainly the lower 20-25 km of the reach studies. The authors also discuss this influence in several parts of the paper. That this is the case is of course quite obvious, with the length of this reach of influence being also dependent on the discharge (for a higher discharge it would be expected to be shorter). This raises the question of good modelling practice. In my experience of modelling, it is customary to establish a model domain such that the uncertainty of the downstream boundary lies (well) beyond the domain of interest. In this reach this would imply setting that boundary some 20-25 km further downstream of Cremona. Indeed there may be a constriction in the river at Cremona which would mean that locating the downstream boundary beyond that constriction would mean its influence on the reach under study would be less still. I am, however, unfamiliar with this reach of the Po, so this is only a suggestion. In the analysis the authors choose to ignore parametric uncertainty as its contribution to the overall uncertainty is small. If this is indeed warranted, then there are only two sources of uncertainty left in the analysis if the boundary condition is moved sufficiently downstream. This will greatly simplify the problem. Indeed later it is suggested that the main cause of dike breaching is the overtopping mechanism, rather than piping, thus suggesting that the now complex approach of selecting equi-probable volume/discharge pairs for the 200 year return period event could also be simplified. I do think these issues are very relevant for further discussion. While I value the work presented by the authors, it would seem to me that a large source of the uncertainty as presented is due to model structure, and the choices made in setting up that structure. In my view it could be argued that the structure chosen is not appropriate as many of the results presented are very much dependent on that structure. Indeed if the downstream boundary had been located halfway the current reach, the results for the upstream part would have been quite different. I would ask the authors to reflect carefully on the choices made and add these to the discussion. While I agree that the discussion with the decision maker is responsible for making the actual decisions, it is the responsibility of the hydrologist to present results based on an appropriate model structure.

Another question that should be addressed is that the probabilistic flood maps presented are in my mind marginal probabilities, as these present the probabilities of inundation, given uncertainty, for the 200 year return period event. The real probability of flooding would need to be derived by assessing the different return periods, each with their uncertainty. This would reveal that the probability of inundation of in particular the area on the right bank in the lower part of the study is quite a bit higher than 0.005, as found from the joint probability of return periods and uncertainty. This raises the question on how this information is then communicated to the decision maker, who as suggested will need to make the decision (rather than the hydrologist). Currently the authors suggest that results such as those presented will provide adequate information to the decision maker in making an informed decision, and thereby reduce as suggested the danger posed by making decisions on a deterministic map only. It would be a benefit to the paper if this discussion could be explored further – in particular the question on how "realistic" the uncertainties presented are (i.e. do these represent the true uncertainty, or could these be biased depending on the decisions made by the modeller).

Related to this last point, the authors often mention that there is an over-prediction or an under-prediction of the uncertainties. I am not sure how this conclusion is reached, I agree with the statements that calibration of such inundation models is difficult (if not impossible as suggested), but would argue that this holds also for the estimation/calibration of the uncertainty. Over prediction and under prediction suggests that this can be evaluated against some "observation", which clearly here is not the case. I would suggest rephrasing these discussions, and instead use terms such as lower or

higher estimation.

Detailed Comments p9810.4: in the case
p9811.17: this concept, highlighting
p9812.22: especially if used for p9812.25-28: In naming the sources of uncertainty epistemic uncertainties are considered as being due to an imperfect knowledge of the system. In literature model uncertainty (structural/parametric) are sometimes defined as separate from epistemic. The reasoning is that these are not necessarily due to a lack of understanding of the phenomenon, but are a result of choices in modelling approach (e.g. 1D instead of 2D), model structure and parameters. Here these are included (as defined in table 2) as a part of epistemic. I am fine with leaving the definition as it is here – but would suggest to be more explicit in the text that in this paper epistemic uncertainty includes model structure and parameters. This will also improve the link also to the next paragraph.
P9813.24 of uncertainty
P9814.10: Chains of models that describe
P9814.12: uncertainties that are summarized
P9814:13: Table 1, starting
P9814.16: uncertainty reduction that can be achieved by adopting additional information or a different procedure.
P9814.17: remove "several"
P9814.18: change bold to italic (there is no bold text in the table).
P9815.4: into the flood-prone area
P9815.5: The IHAM model
P9816.14 by adopting
P9816.23: termed the traditional and the constrained approach respectively, and quantifying

P9816.24-27: The approach taken in fitting the rating curves seems to raise several questions, which are to my mind quite fundamental. I think this section needs to be elaborated and made clearer. As I understand it, the maximum rated discharge at Cremona is in the order of 6000 m3/s. The rating curve for higher discharges is then based on extrapolation. This is in part done through the use of a 1D model of the reach as represented by the grey dots. The first question then is how representative such a 1D model is in extrapolating the rating curve. There are several examples in literature of 1D models underestimating stages due to an underestimation of the turbulent losses for overbank flows (see e.g. Werner and Lambert, 2007). Also there is a clear hysteresis effect in the results of the 1D model that is not considered in the rating curve formulation that is used (such a hysteresis is clearly expected in this quite flat reach). When looking at the fit of the rating curve (in particular that of the constrained approach, it would seem that more than 10% of the grey dots fall outside the 90% uncertainty bounds – which would suggest an underestimation of the uncertainty. I guess the actual ratings are not available, as this would shed a little more light on the true uncertainty in the curve. To my mind what we see here is the uncertainty due to the fit of stage-discharge pairs generated by a 1D model, which itself is an extrapolation. Does that model consider parametric uncertainty? Where is the downstream boundary? If this boundary is well downstream of Cremona, then would it not make sense to continue the model on to there – and not induce uncertainty by fitting a rating curve to the simulation results at Cremona? What is the bank-full discharge, and has the rating curve as commonly done been divided into at least two sections to reflect in bank and out of bank conditions? In short there are many questions that need to be addressed to my mind on the representation of uncertainty in the rating curve, which as discussed later is of key importance to the authors.

P9817.7: By means of a one-dimensional
P9818.19: may appreciably alter the
P9819.25-27: I would like the authors to be more explicit on how the distributions of the breach parameters were sampled. It would seem logical (but perhaps data shows

this to be otherwise) that the parameters such as the breach width depend on the magnitude of the overtopping (which is stated as being the primary cause of failure). This in turn depends on the event magnitude. Has this been taken into account in the sampling strategy? Also, if there is such dependence, then does it make sense to truncate the distribution, given that the observed events in 1994 and 2000 are as stated to be in the order of 50 year return period, quite a bit lower than the 200 year return period in the estimation.
P9820.3: The breach
P9820.4: follow a normal
P9821.1-4: The volume considered here is in the 30 days around the flood peak. It may be good to reflect on the typical duration of an event in the Po in this reach. Is this sufficient? For some rivers this may not be the case. It may be useful to the reader to provide just a little insight into how the assessment was made that 30 days would suffice.
P9822.5: red dots
P9822.11: the gauge at Cremona – or – the Cremona gauge.
P9822.16: the median
P9822.18: the constrained
P9822.20-22: More insight in the sampling of the RandomT rating curves may be useful. It is suggested that curves are sampled only within the 90% bounds. I am not sure I understand this. I would assume that the distribution was sampled, which would imply that some 90% of the curves sampled would fall within the 90% bounds. I would like the authors to clarify this, or if required rephrase.
P9824.10-12: That the area is flooded in the traditional approach is a direct result of the difference of the (extrapolated) rating curve – stages in the traditional rating curve are quite a bit higher – thus exacerbating overtopping. It may be good to add that this could have been expected.
P9825.2: I would suggest removing the word “remarkable” – as this distance is exactly what would be expected. A simple analysis of the backwater length using a “back of

an envelope calculation" would likely suggest the order of magnitude.
P9825.21: that this result could also be partly associated with
P9825.26: when performing real-time
P9826.3: It is suggested that the hydraulic behaviour of the constrained rating curve is better than that of the traditional. To be honest, I am not entirely sure I agreed with that – as it depends on how well the extrapolated 1D model represents the true rating curve which is unknown (see also general discussion). Please rephrase in the light of the discussion that is to be added on the rating curve in response to comments in the general section.
P9826.13-20: The differences between the two maps in figure-10 are to my mind quite limited. The downstream end sees little flooding – except for the lower right bank area that is flooded in all scenarios. Differences in the upstream end are limited to some small areas on the upper left bank. This is again logical given that the difference between the two is mainly the downstream boundary. The variability does not overtop the lower left bank dikes (in 100% of the runs), it overtops the right bank (in 100% of the runs), and the upstream is mainly influenced by the scenarios – which are the same in both cases. In short – I am not so sure this figure and discussion contribute all that much.
P9826.21: upstream of the downstream end
P9827.3: the decision-maker
P9827:21-23: suggest the initiation of retrogressive erosion and the presence of a non-negligible danger of piping (Coratza, 2005). This aspect . . .
P9826.24: the piping
P9286.24 - 29: I am not so sure this section contributes and as is currently phrased actually to my mind leads to confusion. First it is accepted that overtopping is the main cause of breach. Then it is stated that the geological controls are of importance and need to be studied more. Bearing in mind the fact that historical evidence is from a 1:50 year event - and that the analysis here is for a 1:200 year event, the question then arises how representative the parameterisation of the breach model used is, which

leads to the result that the main process leading to breaching for the 200 year event is indeed overtopping. It would be good to elaborate more on this in the discussion.
P9827.23: considered through a bivariate
P9828.8-12: These sentences will need to be rephrased in the light of comments made earlier on the rating curve.
P9835: Median
P9836: The not-floodable area is unclear. Is this not the floodplain (yellow) ??? Please clarify and/or correct.
P9837-caption: italics
P9837: What is meant by time series length in contributing to the uncertainty of the rating curve? Should the number of rating pairs used to construct the curve be included as a source of uncertainty in the rating curve?
P9838: Add to the caption what the grey dots are (i.e. 1D model results)
P9841: The legend and caption of this figure mention levees- while the rest of the text the word dikes is used – please be consistent.
P9843: MedianC

Werner M. Lambert M. 2007. Comparison of modelling approaches used in practical flood extent modeling. Journal of Hydraulic Research. 45(2):202-215.

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