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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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Reply to Referee#2's comments.

We are grateful to Micha Werner (Referee#2) for his thorough and inspiring review. Referee#2 raises some sensible and meaningful comments, and since comments concern fundamental aspects that are not only related to this specific work but also to field of hydraulic applications, we consider this discussion to be very interesting and functional to a significant improvement of the overall quality of the manuscript. Our reply is structured as follows, we report all referee's comments (indicated by RC) together with our reply (denoted by AC, Authors' Comment).

RC: 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

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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.

AR: In this comment Referee#2 raises some major comments that, for the sake of brevity, can be summarized into two points: i) the opportunity to extend the hydraulic model downstream the Cremona section; ii) the suitability of the model structure chosen for the main aim of our work. Concerning the point i), we certainly agree with Referee#2 when he highlights the existence of a good modeling practice which suggests (one should probably add "when possible") to set the downstream boundary condition well beyond the domain of interest to reduce its influence. However, this is the very focus of our manuscript. Being aware of the influence of the downstream boundary condition in subcritical flow conditions, we are exactly interested in better understanding and possibly quantifying the effect of the uncertainty in the downstream boundary condition on the evaluation of dike breaches and flood probabilities, uncertainty that is seldom considered in practical applications. Since the effects of rating-curve uncertainty on flood hazard mapping is in fact the main goal of our investigation, we deliberately referred to a case in which we set the boundary condition at the downstream end of the considered river reach. Also, concerning this point we would like to stress a couple of aspects, which we deem to be not marginal: 1) our analysis is not the outcome of a commissioned research work, to be used by decision-makers of a given public body, quite the opposite, it is an analysis designed and performed specifically to address a particular issue (i.e. effects of rating-curve uncertainty) , which we consider to be still relevant and open and was recently addresses by other Authors in different contexts (e.g. Pappenberger et al., 2006, referring to a 1D-hydraulic model); 2) setting the downstream boundary condition beyond the influence point does not answer to our science question (philosophical issue), and, more importantly, is not always viable or applicable in the real world (practical issue). For example, what if the modeler cannot set the downstream condition far enough due to lack of data? Perhaps, under such circumstances, s/he would like to know if and how the uncertainty in the downstream boundary condition impacts his/her computations. Furthermore, the application of a "back of an envelope calculation" for the estimation of backwater effect could (largely) underestimate the effects of such an uncertainty. Considering our case study and referring for example to Samuels (1989) for a rough estimation of the backwater effect, it is possible to evaluate the order of magnitude of the backwater length, obtaining a value of about ∼13km, which is half of the distance where we experienced significant impact of the uncertainty on the downstream boundary condition (i.e. 25-35 km). As a matter of fact, the upstream influence length of the rating curve uncertainty, 25-35km, is a result of our analysis and it was not expected nor retrievable from "back of an envelope calculations". In conclusion, since the point raised by Referee#2 is, in general, absolutely appropriate, we will revise the manuscript highlighting this point and discussing it in detail. Concerning point ii), the reason why we adopt a bivariate approach for flood estimation is that we do not know, before the application, what could be the main cause of dike breaching. We are not suggesting as a starting hypothesis that all breaches are due to overtopping since this observation comes from the results of the investigation. As a matter of fact in this area some piping phenomena have being experienced in the past and could not be excluded a priori. In this condition the shape of the flood event and the duration time of high water level into the river could strongly influence dike-breaching mechanisms, activating piping or micro-instability phenomenon which may be not observed for high peak and low volume event. Furthermore, even in the case that all dike breaches are due to overtopping the shape of the flood event and its overall flood volume could influence the amount overflowed, and consequently the floodable area. Nevertheless, a further clarification on that point will be added to the manuscript.

RC: 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.

AR: We identify two (very sensible) main points in this comment. Concerning the first one, the reviewer argued that if all return periods (the whole risk curve) are considered, the probability of inundation becomes higher than the what obtained in our study for a 200-year event. This is of course true. But the decisions on the flood protection and precaution are often made for a certain standard of protection related to a certain return period, e.g. a 200-year event is considered by the Po River Basin Authority along the River Po (i.e. 100-year in Germany). Here actually comes the value of the probabilistic representation of the inundation maps considered in the work. Of course, we acknowledge the point of the reviewer, we will further discuss the issue of the marginal probability on the revised version, but still stress the value of the probabilistic representation of the inundation maps for individual return periods. Relative to the use of terms over- and under-prediction (second point), the reason comes from what we defined in the manuscript as the true or reference normal rating-curve (blue thick line on Figure 3), obtained at Cremona river cross-section from the compound of unsteady stage-discharge pairs (grey dots). These grey dots represent all stage-discharge points simulated by means of a quasi-2D model of the River Po, which was first calibrated for a specific flood event and then considered able to reproduce the hydraulic condition at the Cremona cross-section and applied for the simulation of 10 well known historical flood events. Grey dots represent the compound of discharge-level pairs simulated at Cremona gauge by means of the calibrated quasi 2D model which were used to construct synthetic rating-curves (concerning this point and the following specific remark please refer to Domeneghetti et al., 2012 for further details). If the blue curve on Figure 3 could be considered as the reference normal rating-curve, left panel of Figure 3 clearly emphasize the non-negligible overestimation induced by a traditional approach. However, the comment of the reviewer may result from a lack of clarity of the manuscript, therefore we acknowledge his suggestion. The discussion concerning the flood probability will be rephrased following Referee#2 comments.

RC: 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 modeling 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.

AR: Specific comments will be incorporated in the manuscript, while the issue raised about epistemic uncertainty will be further explained.

RC: 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.

AR: The text will be adjusted according to these suggestions.

RC: 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

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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.

AR: The description of methodologies adopted for rating-curve construction cover a fundamental role in this analysis and their understanding is important for the overall meaning of the work. Arguments raised by the reviewer are indubitably associated with a lack of clarity of the original manuscript and we agree with Referee#2 when he asks for a revision of this section. Our study, when it comes to rating-curve estimation methodologies and quantification of rating-curves uncertainty heavily relies on Domeneghetti et al. (2012). Even though it is obviously not possible to include a comprehensive and detailed explanation of this work in our manuscript , we will provide more information, making these elements clearer and the manuscript more self-contained and self-explanatory. Concerning specifics reviewer's comments, grey dots on Figure 3 represent all discharge-stage pairs reproduced by a quasi-2D calibrated model that has Cremona as an internal cross-section (downstream boundary condition in this model is imposed at ∼300 km downstream) simulating 10 historical flood event (see also previous comment). These points (grey dots) are then used in order to mimic several synthetic field-measurements campaigns for rating-curve construction. Each synthetic campaign (made of 15 discharge-stage pairs up to 6000 m3/s) was used in order to fit a rating-curve by adopting two different methodology: traditional and constrained approach. Traditional approach consists on a simple fitting of a power law equation to the set of data, while the Constrained approach try to reduce the extrapolation error by means of only one additional point. This additional stage-discharge pair is estimated by means of a simple 1D steady-state model (different from the quasi-2D model) that also uses Cremona as an internal cross-section. This second model is calibrated referring to the maximum measured pair and is then used to estimate the maximum discharge capacity at the Cremona section. This additional point is then used in order to reduce the extrapolation error by forcing the rating-curve trough it. The reduced bias ensured by the constrained approach is clearly evident in Figure 3, right panel, where the uncertainty range is limited and rating-curves are close to the reference on (blue line).

RC: 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.

AR: The reviewer raises a good point highlighting the dependence between breach width and overtopping magnitude that could be observed in reality. However, the current model version does not included this relation on the breaching dynamic estimation and this imply that every time a dike failure occur, the IHAM model randomly define the final maximum width of the breach, sampling the log-normal distribution fitted over the range of observed breaches. Considering this aspect would be interesting and could represent a further step on flood probability analysis. This aspect will be discussed and clarify on the revised manuscript.

RC: 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.

AR: The reviewer raises a good point. The time window of 30 days around the flood peak was selected as a result of the analysis of the series of annual maximum flood events observed along the Po River at the gauge at Piacenza. This time-span ensures the complete description of every flood wave (major events occurred on 1951 and 2000 are entirely described). Obviously, this time interval should not be considered as a standard duration but have to be defined for every case study in relation to the hydrological characteristics of the river basin. This discussion will be included in the manuscript.

RC: 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.

AR: Reviewer is right, during the Monte Carlo simulation curves are sampled between the 90% bounds of uncertainty reported in Figure 3 (left panel for the Traditional approach, right panel for the Constrained approach). The revised manuscript will better clarify this aspect.

RC: 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.

AR: we agree with the referee and we will add his suggestions.

RC: 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.

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AR: please see Authors' comment at the beginning.

RC: P9825.21: that this result could also be partly associated with; P9825.26: when performing real-time

AC: adjusted

RC: P9826.3: It is suggested that the hydraulic behavior 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.

AC: The series of analyses carried out clearly shows how the constrained curve is better than the traditional one (see also Domeneghetti et al., 2012), ensuring a better estimation of the real hydraulic conditions represented in our case by means of blue line and grey dots on Figure 3. Probably this comment originates from a lack of clarity of the manuscript in the description of the two methodologies (see discussion before). We will better clarify this point.

RC: 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.

AR: The clarity of the figure is questioned also by Reviewer#1. We will revise the figure

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using colors (instead of black and grey) to better highlight differences, and we will use zoom-in panels with independent scale-bars to better show that the differences in terms of floodable area are far from being small. In our opinion the figure is functional to the discussion of effects of rating curve uncertainty and use of deterministic/probabilistic maps. We will revise it as described, but we are also open to drop the figure is the Associate Editor recommend us doing so.

RC: 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.

AR: all corrections will be adopted on the revised version.

RC: 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. AR: May be the Referee's comment is relative to P9827.24-29. The aim of this section was to emphasize one of the possible weaknesses of the analysis, highlighting that even if overtopping appeared to be the only phenomena responsible for dike breaches for the 200 year event, evidences of sandboils experienced during the most recent flood events suggest the presence of retrogressive erosion that imply that other breaching mechanisms should not be excluded a priori.. The section highlights that these results are affected by uncertainty which is due to the limited knowledge of dike properties (geometrical and geotechnical characteristics) and to the uncertainty involved on piping analysis. In particular, concerning the latter aspect, Vorogushyn et al. (2009), highlighted how the probability of failure due to piping is strongly influenced by pipe development velocity, which emphasize the concept that even if sandboils occur they may not results on a piping failure because of the low pipe development velocity (for more details on fragility curves estimation please see Vorogushyn et al., 2009). In order to better clarify this aspect the section will be rephrased.

RC: P9827.23: considered through a bivariata; P9835: Median; P9836: The not-floodable area is unclear. Is this not the floodplain (yellow) ??? Please clarify and/or correct; P9837-caption: italics.

AR: Errors will be fixed as suggested by the Referee, while “not-floodable area” will be replaced with “floodplain”.

RC: P9828.8-12: These sentences will need to be rephrased in the light of comments made earlier on the rating curve.

AR: as mentioned before, this comments in our point of view comes from a lack of clarity on the previous part relative to rating curve.

RC: 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?

AR: The Referee is right, it is referred to the number of pair used for rating-curve estimation. This source of uncertainty will be better described in the revised manuscript.

RC: 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;

AR: Suggestions will be acknowledged.

Additional reference: Samuels, P. G. (1989). Backwater lengths in rivers. Proc. Inst. Civ. Engrs. Pt 2, 87, 571–582.

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Interactive Comment

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 9809, 2012.

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