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Hydrology and Earth System Sciences Discussions

Interactive comment on "Assessing rating-curve uncertainty and its effects on hydraulic model calibration" *by* A. Domeneghetti et al.

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We thankfully acknowledge T. Moramarco's (Referee#1) very useful comments and for providing meaningful suggestions which enable us to improve the overall quality of the presented work. 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:

I enjoyed reading this paper which conveys a significant message on the rating curve uncertainty assessment, which is a topic of utmost importance in the hydrology field. I recommend that the authors address the questions that I posed as best they can, but regardless of how well they answer them, I still recommend publication. The authors obtained the synthetic sets of pairs (h,Q) from the simulated historical floods at Cremona gauged site, after that the "first quasi-twodimensional" hydraulic model was calibrated for the flood event of October 2000.This means that a single calibrated Manning value was used for identifying all synthetic measurement campaigns and, hence, the stage-discharge power relationships. However, my field experience on streamflow measurements leads to say that the Manning's roughness in the inbank channel varies from higher values for low flow depth and drops to asymptotic values for higher stages (Moramarco and Singh, 2010). This entails that if a single Manning's value is used for the calibration in the main channel, the discharge for low flow is overestimated and this might be the case of this work.

AR:

We agree with Reviewer#1, he raises a fundamental concern (the utilization of a single Manning coefficient) that was not adequately discussed in the original manuscript and that will be addressed in the revised manuscript (se also our replies below), together with the assumption of neglecting seasonal variation of the Manning value (see e.g., Di Baldassarre and Montanari, 2009).

RC:

However, knowing that the Cremona river site is a gauged site, I guess that recorded stages are available there for the historical floods. If so, the authors might calibrate each flood by matching the stages and then to draw out from each one, the 5 synthetic sets of 15 pairs (h,Q). In this way, for the traditional approach, I expect that the "new" empirical stage-discharge power relationships should be more "steep" than the ones shown in the paper and the extrapolation might achieve the "optimal" rating curve or at least approach to it, as instead it occurs for the constrained approach. Therefore, the difference in terms of uncertainty between traditional and constraint approaches should be less evident of that shown in Figs.6 and 7 and perhaps more coherent with the reality. If historical floods data at Cremona sites are not available in terms of recorded stages, the previous insights should be at least highlighted in the manuscript. If historical floods data at Cremona sites are not available in terms of recorded stages, the previous insights should be at least highlighted in the manuscript.

AR:

Again, we agree with Reviewer#1. We carefully analyzed the results reported in Moramarco and Singh (2010) (by the way, this is a relevant and significant paper that we will cite in the revised manuscript) that show empirical evidences of Manning's coefficient asymptotic behavior with river flow-rates. We are currently investigating the potential consequences relative to our study, as suggested by Reviewer#1 himself (i.e., calibration of hydraulic model for a few streamflow and discharge data observed at Cremona in same year of the topographical survey used for the models' implementation). Nevertheless, we can anticipate some considerations relative to Reviewer#1 main concern.

The evaluation of (h,Q) pairs using a quasi-2D model calibrated for high events inevitably introduced uncertainty, which is expected to be higher for low-flow conditions. Moramarco and Singh (2010) analyzed this aspect evaluating the trend of Manning's coefficient for two river sites along the Tiber River and they highlighted that the n value decreases with increasing flow depth, tending asymptotically to a constant value after a certain water level. Furthermore, they showed that, for both rivers sites, the asymptotic Manning value was reached for water levels higher that 1-1.5 m (Moramarco and Singh, 2010, Fig. 3), which corresponds for the river cross-sections of interest (Moramarco and Singh, 2010, Fig. 2) to 25-30% of the maximum water level.

Considering this behavior and assuming a plausible similarity of flow conditions on the main channels of Po and Tiber rivers (they both have comparable roughness coefficients for high flow conditions) we investigated this aspect for historical events simulated in our study. We analyzed the hydraulic conditions at Cremona by considering all simulated (h,Q) pairs with Q< 6000 m3/s and we computed for each pair the ratio between h and maximum water level (dimensionless water level). Figure (a) shows the cdf of all dimensionless water level (Q<6000 m3/s). Also, vertical dashed lines in Figure (a) indicate the range of values that were resampled to generate synthetic measurement campaigns (i.e. (h,Q) pairs used for the rating-curve construction for both traditional and constrained approaches). As one may observe, all used pairs are associated with dimensionless water levels higher than 50%.

On the basis of these considerations we expect that the range of flows considered in our study is not affected by a decrease of manning values with Q (i.e. the asymptote is already reached). We will get a definite answer from the simulation of empirical values at Cremona (beginning of our comment). Nevertheless comprehensive discussion of this insight will be introduced in the future version of the paper.

RC:

I would like to also emphasize that the same analysis proposed in this paper can be directly applied to measurement set of (h,Q) pairs, really observed at a gauged site, and this is a further asset for the hydrological practice.

AC:

Reviewer#1's suggestion is appropriate and sensible. We will point this out and discuss how the methodology could be applied in gauged cross-section in the revised manuscript and we will.

RC:

I don't think that the term "optimal rating curve" is quite appropriate. "Optimal" is used if more solutions are compared and the best one is selected. It doesn't seem this the case. For that, it would be better to use the term "normal rating curve/discharge" rather than "optimal rating curve/discharge".

AC:

The Reviewer#1 is right, “optimal” is not used in this contest with the meaning of best solution between several options, but rather to represent the best shape of the actual steady-state rating-curve for the cross-section of interest. The rating-curve will be called “normal rating curve” as recommended by Reviewer#1.

RC:

The paper by Perumal/Moramarco/Sahoo/Barbetta is quoted in the references, but not in the manuscript. I guess that the reference should be placed at line 22, pag 10505.

AC:

The reviewer is right here, also in the recommendation of the page and line. We will quote the paper as suggested and the additional reference provided by the Reviewer will be added.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 10501, 2011.

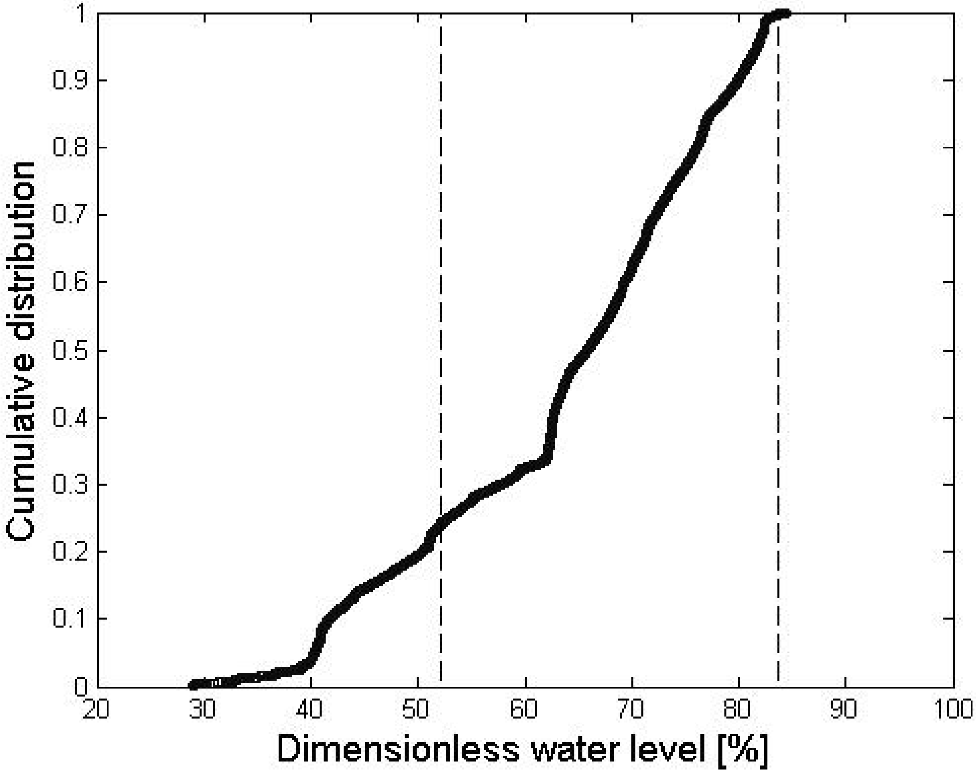

Fig. 1. Figure a - Empirical cumulative distribution of dimensionless water level; vertical dashed lines define the range of (h,Q) pairs used to generate synthetic measurement campaigns.

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