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Discussions

Interactive comment on "Considering rating curve uncertainty in water level predictions" *by* A. E. Sikorska et al.

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We thankfully acknowledge the first Referee (Giuliano di Baldassarre) for his revision. We reply to his remarks directly below. To make our reply clearer, we report in italic the original Referee's comments. To avoid any misunderstanding we refer to passages in the Discussion Paper (DP) instead of referring to the revised manuscript, which has a different line numbering.

Referee #1 (Giuliano di Baldassarre): *"Sikorska et al. explored the uncertainty of the rating curve in modelling water levels in a urban catchment. The topic of the study is significant and relevant to HESS, the paper is concise and generally well*

structured while appropriate references to literature are included. Lastly, the research work is overall scientifically sounded, but I have two major comments that should be addressed."

1) *"My first concern is about the fact that (as stated in the paper) "eight recorded rainfall-water level events" were used to calibrate and validate the model. Here, I think that a critical discussion about the assumption of stationary of model parameters is missing. This is rather questionable because this catchment has experienced (as stated by the authors) a "rapid urbanization in the last three decades and today urban areas cover 58.7% of the catchment"."*

We do not see any unusually critical assumptions in this point. Arguably, eight recorded rainfall-water level events cannot cover the entire transition from a rural to urbanized catchment. However, our main contribution is not long-term transitions, but rather on a sound framework to assess the uncertainty contribution of a water level-runoff (LR) model, here: a rating curve. This could also be demonstrated on data from a single event.

In our case study, despite the rapid urbanization of the catchment, it is a reasonable assumption that the catchment properties did not change significantly in the time-span of the eight calibration events because all events were recorded in the same short period (within 2011), as stated in lines 16-17 p. 2966 in DP.

To make this point clearer, we modified our manuscript and included a paragraph in the discussion:

["Flood predictions could generally benefit from gathering more recorded data to calibrate models (both RR and RC). Extending time-span of calibration data must be, however, done carefully. Under changing conditions of the basin (e.g. urbanization), its parameters cannot be considered stationary. This is a general problem of every model applied for long-term predictions. A model calibrated under certain (stationary) conditions cannot forecast the basin response under (unknown) changed conditions but only makes predictions for the given situation (Blöschl and Montanari, 2010). If one is interested in testing the catchment behaviour for different conditions, the parameters of interest have to be accordingly modified."]

2) *"My second concern is the arbitrary use of ranges and distributions for the model parameters (Table 3). I think these assumptions are unavoidable, but they do require justification, and possibly some more numerical experiments."*

This might be a misunderstanding of the Referee. As stated in the paper, the prior distribution for the parameters of the hydrological model (RR) and the error model was not chosen arbitrarily. Instead, it was elicited via parameterization technique developed in a previous study (Sikorska et al., 2012). In contrast, the prior on rating curve parameters (RC) was based on careful field measurements, as described in Sect. 3.4 in DP. As the case study and parameters estimation are not the scope of this paper, we prefer to refer to the previous paper, in which these issues were discussed in details. Nevertheless, we improved this description in Sect. 3.5.1 (Results of the statistical inference, l.21, p. 2968).

3) *"The area of the catchment, for instance, was set as a free parameter (RR1). However, I presume that the area of the catchment can be measured with a proper accuracy. So, why not to fix the catchment area to the measured value, and use only 3 free parameters? This can be a numerical experiment worth trying to also explore the changes in the posterior distributions of the other parameters in view of e.g. possible compensation of errors. Indeed, I understand that the catchment area cannot be perfectly measured, but modelers typically allow parameters between ranges of observation uncertainty. In the current study, if I understand well Fig. 3, the posterior*

distribution of RR1 has a peak around 7 km2, which is almost a quarter of the actual catchment area (28.7 km2). I wonder whether this requires some more justification. Also, what are the consequences in terms of water balance?"

The posterior mode, as truly noticed by the Referee, for this parameter is about 7 km2, which is about 25% of the measured catchment area (28.7 km2). The small area can be explained by the fact that only the water which is directly discharged by the stream is modelled. Other discharges that are drained by the canalization network are not modelled. Furthermore, we modelled rainfall-runoff events. Event-based modelling is generally limited to model only the direct runoff which occurs during the first phase while omitting slower runoff due to the catchment retention, which is typically much delayed in time. Hence although the RR1 parameter might be interpreted as the (total) catchment area, it is 'seen' by the RR model as the 'effective' area that contributes to the direct surface runoff.

Another reason is that we use a hydrological model with a very simple model structure (only four parameters). Therefore, the model structure error is presumably large what was proved by the Results (see also Discussion lines 5-11 p. 2972, DP). This will be compensated by parameters adjusting their values so that the observed data can be better reproduced. The price is, however, that the parameters lose some degree of their physical interpretation (Wagener and Gupta, 2005; Sikorska et al., 2012), as stated in DP in lines 16-19 p. 2971.

We thank the reviewer for the comment regarding the water balance. Indeed, the applied event-based model is not based on the water balance of the whole catchment as groundwater, base flow and evapotranspiration are not modelled. Thus, the direct assessment of water balance is not possible. To avoid misunderstanding, we will augment the discussion with the following paragraph (l. 24 p. 2972):

["It must be also stressed that the applied RR model as every event-based model is limited to model only the direct surface runoff while neglecting other water balance components as groundwater, base flow and evapotranspiration. Thus, a slower runoff which occurs due to the catchment retention and in an urbanized basin is typically much delayed in time is usually omitted."]

Reference:

Blöschl, G. and Montanari, A.: Climate change impacts - throwing the dice?, Hydrol. Process., 24, 374–381. doi: 10.1002/hyp.7574, 2010.

Sikorska, A. E., Scheidegger, A., Banasik, K., and Rieckermann, J.: Bayesian uncertainty assessment of flood predictions in ungauged urban basins for conceptual rainfall-runoff models, Hydrol. Earth Syst. Sci., 16, 1221-1236, doi:10.5194/hess-16-1221-2012, 2012.

Wagener, T. and Gupta, H. V.: Model identification for hydrological forecasting under uncertainty, Stoch. Env. Res. Risk A., 19, 378–387, doi:10.1007/s00477-005-0006-5, 2005.

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