Hydrology and Earth System Sciences Discussions

Interactive comment on "Bayesian uncertainty assessment of flood predictions in ungauged urban basins for conceptual rainfall-runoff models" *by* A. E. Sikorska et al.
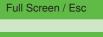
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General comments

The authors assess the uncertainty of flow prediction in an un-gauged urban basin related to structural, parametric and input uncertainties. They apply Bayesian statistics with a continuous-time autoregressive error model and Box-Cox transformation of data, following the work of Yang et al. (2007). It is worth noting that similar methods were applied by Romanowicz et al (1994), where a discrete autoregressive model was used for the errors, together with logarithmic transformation of observations, as

a special case of the Generalised Uncertainty Estimation (GLUE) methodology. The main novelty of the paper is the development of a concise procedure to derive prior parameter distributions based on an external source of data. The approach developed was successfully applied to an urban catchment in Warsaw, Poland. The authors obtained a 150% improvement on model predictions in comparison with a non-calibrated model. The paper is well written and interesting. It follows modern trends in modeling uncertainty in hydrology, has a comprehensive literature review, and applies the most up-to-date numerical tools. The authors clearly state what are the main novel issues of their paper and they fulfill their promises on the point. In the paper an event-based modeling approach was applied. It is well justified, as the paper deals with un-gauged catchments, where there are no continuous observations available. However, this particular point shows some inconsistency in the approach. Bayesian methods are useful only when observations are available. How can this requirement be met in an un-gauged catchment? There are a number of statements that need more explanation. Namely, the Bayesian methods allow for the updating of prior distributions based on the available data, but they do not allow for the separation of the influence of different sources of uncertainties on the output. The separation of sources of uncertainty is possible when the structure of the model is linear. The statement that input uncertainty and model structure simplicity are the main causes of the wide uncertainty limits of the predictions is difficult to justify scientifically, even though it seems to be logical that model parameters are better defined than external forcing. Unfortunately, the scenario analysis presented does not allow us to judge which source of uncertainty is more important The hydrological tools applied are rather dated. The instantaneous unit hydrograph (IUH) is applied for runoff generation combined with five different methods for the derivation of IUH characteristics and Nash model parameter estimates. The methods are based on empirical formulae involving a large number of parameters. The resulting model predictions are not good (the best estimated maximum peak values have 100% error and the uncertainty of the predictions is unacceptably large (quote from the paper: "5 times larger than observed values"). Moreover, the authors do not

present any validation of the model predictions, which is a major drawback of the paper. The authors should apply split sample test to show the model performance. In conclusion, the scientific level of the paper is very high regarding the methods applied, but the question is, were the tools justified by the amount of observations available? In other words, were the tools fit for purpose?

Specific comments

The prior distributions for all parameters (input, autoregressive model and model structure parameters) were assumed to be independent. I would expect some comment on the possible issues emerging when this assumption is violated. The choice of prior distributions of error model is a big challenge, as it has no physical justification after the Box –Cox transformation of data is applied. For the A and CN parameters of the SCN-CS part of the model, a normal distributed error was chosen with 10% standard deviation of the mean. Therefore it was assumed that bias is equal to zero. These errors are related not only to map inadequacy but also to parametric uncertainty of the model. Please comment on that. The choice of rainfall multipliers is not clearly described. In the first instance the authors claim that all 14 events have their own different multipliers, but in the numerical analysis only one distribution is chosen for all the events (Figure 3). Then in the discussion (page 15), the authors write that "one rainfall multiplier per rain event" was inferred. The supplementary material gives the results of posterior analysis for all the events. It is not clear what likelihood function was used for this purpose. Was each event updated separately or in combination with all other events? Assuming that all the events were treated jointly, that would give 20 parameters. How was the computational burden overcome? The likelihood function is usually flat and does not allow for too many parameters to be optimized when a time domain is used. Inferring all 20 parameters based on one likelihood function would be a difficult task. Please give some more explanation of that point. Regarding the likelihood function presented in Appendix A, not all the notation is explained. What is t_i, n, what is the observation equation, how is the fact that the observations are discrete

dealt with? I cannot see any detail in fig. 5. The authors should present a maximum of 4 events at a larger scale; what are the dashed lines showing?

On a pedantic note, please use Poland instead of PL and "flow" or "runoff" instead of "flooding".

References

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Discussion Paper