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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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Dear Associate Editor, Reviewers and Commenter,

We would first like to thank all the reviewers and commenter for their positive feedback. We found their input very valuable and believe that it improved the manuscript. On the following pages we address point-by-point all remarks and describe in detail where we made the corresponding changes in the manuscript.

Based on the reviewers' feedback, we made major changes to the article. These are included in the revised manuscript "Bayesian uncertainty assessment of flood predic-

tions in ungauged urban basins for conceptual rainfall-runoff models". Thank you very much for your time and considerations.

On behalf of the authors, sincerely,
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Associate Editor: (Jasper Vrugt)
" Thank you for this paper. I screened the paper and overall it appears fine for review. On a personal note, I am surprised however that essentially our own work that is crucial to this paper is neglected. This is not meant to advertise my own papers but to point the authors to literature that they are completely missing. – the 2003 paper on automatic sampling of posterior distributions, (SCEM-UA), 2005 paper on treatment of all sources of error using combined MCMC and Ensemble Kalman Filter, 2008 paper on DREAM and input treatment uncertainty – I suggest that you have a look at these papers – they do essentially what is being done herein – for instance 2005 paper uses first order model as well but infers model errors also, 2009 paper in SERRA does also do treatment of all error sources, and 2007 paper on Bayesian Model Averaging. Please check jasper.eng.uci.edu Also, the work of Ajami et al. 2007 is missing on joint BMA and input error treatment, and several other developments along these lines. Please have a look at these papers. "

Id./Comments/Response:

E-1 "...but to point the authors to literature that they are completely missing..."

We are grateful to the Editor for pinpointing us to valuable literature, which we indeed overlooked. The above sources relate to our study with regard to: i) sampling algorithm from posterior distribution, ii) assessment of model structure errors and iii) treatment of individual error sources and additional information has been added in the Introduction

(l. 9-12 and 14-17 p. 3, l. 16-25 p. 4).

Specifically, regarding i), we employed the adaptive sampler of Viohla (2011), because it has some nice properties, including proof of convergence. As we obtained satisfactory results, we did not see the necessity to test different algorithms to sample from the posterior, such as SCEM-UA (Vrugt et al., 2003), Differential Evolution Adaptive Metropolis (DREAM) (Vrugt et al., 2008a, 2008b), or others. Nevertheless, we added general references on sampling techniques in the manuscript (Gilks et al., 1995; Brooks et al., 2011) (see also l. 8 p. 13 in the manuscript).

Second, with regard to ii) there have been suggested many different techniques in recent years, which are now properly referenced in the introduction (l.16-25 p. 4). Only Bayesian model averaging (Vrugt et al., 2008c) we did not find relevant for our study, because we are not interested in assessing which candidate of a set of model structures is the structure that actually gave rise to the observed runoff data. In our study, we are rather bound to the simple IUH model structure, among other things because one of the main innovations is that we exploit the physical meaning of IUH-type models to concisely formulate prior knowledge.

Finally, with regard to iii), Simultaneous Parameter Optimization and Data Assimilation (SODA) (Vrugt et al., 2005), rainfall multipliers (Vrugt et al., 2008b) and Integrated Bayesian Uncertainty Estimator (IBUNE) (Ajami et al., 2007) are uncertainty analysis frameworks which provide similar ways to treat contributing sources of uncertainty. SODA merges the input and model structure uncertainty together into a single forcing term (Vrugt et al., 2005). Vrugt et al. (2008b) introduces storm-specific rainfall multipliers and infer them together with the model parameters. IBUNE applies a set of a priori drawn normally distributed rainfall multipliers which are then shifted and scaled according to the 2 additional input error parameters (the unknown mean and variance) for the estimation of the likelihood (Honti and Reichert, 2012). This has been added to the Introduction (l. 9-12 14-17 p. 3, l. 16-25 p. 4).

Interactive comment 1 (Alberto Montanari)

“I wish to congratulate with the authors of this study which I believe is highly significant. I came across this paper while searching for references on application of event-based hydrological models in urban environment and I really enjoyed reading it. I think it is well written and very much interesting, especially in view of the assessment of the performances with respect to different scenarios. The paper is very well written and organised. It is very clear also. I just have a very minor suggestion to forward to the authors: they rightly point out that the formulation of a likelihood function for hydrological models is a relevant challenge. The approach proposed by the authors is in principle correct. I am only curious to know whether the authors succeeded in getting residuals from the error model that were independent and identically distributed. Was the Box-Cox transformation successful in making the data homoscedastic and was the autoregressive models successful in explaining the correlation structure of the data themselves? It would not be a problem if the reply to the above queries was not fully positive, the approach would be interesting anyway. I just believe that it would be interesting to know the goodness of the fit that the authors could reach. Once again, I am pleased to express my congratulations to the authors.”

Id./Comments/Response

C-1 "If the residuals from the error model were independent and identically distributed?"

In the presented study we applied a continuous autoregressive error model (AR(1)) based on the Ornstein-Uhlenbeck process proposed by Yang et al. (2007, 2008) that assigns the independence and normal distribution not to the error (residuals of the model) but rather to the random distributes, called innovations (Chatfield, 2003; Yang et al., 2007, 2008). For the innovation we did not get 100 precent fulfillment of this assumption, however it was much improved (shown on the fig. 4 in the manuscript) comparing to the traditional assumption of normally distributed residuals (not shown in the manuscript). As this is clearly described in the manuscript (Discussion par.2 p. 17), we did not change the text.

C-2 "Was the Box-Cox transformation successful in making the data homoscedastic?"

With the Box-Cos transformation we obtained a good homoscedasticity (symmetric distribution of the residuals). As this detail is a prerequisite for a meaningful analysis, we also suggest to not explicitly mention it in the text.

C-3 "was the autoregressive model successful in explaining the correlation structure of the data themselves?"

The continuous AR(1) process captured the autocorrelation for the most events well. However, for some events weak autocorrelation remains, which is openly illustrated in Fig. 4 and addressed explicitly in the manuscript (l.8-14 p.17). For a more flexible hydrological model we would expect even better results. In a further study we will test other more complex hydrological models (e.g. HyMod).

Reviewer 1 (Renata Romanowicz)

“General comments The authors assess the uncertainty of flow prediction in an ungauged 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 percent 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 ungauged 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 percent 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 percent 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 ti, 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

Romanowicz R.J., K. J. Beven and J. A. Tawn, 1994, Evaluation of predictive uncer-

tainty in nonlinear hydrological models using a Bayesian Approach, in "Statistics for the Environment (2), Water Related Issues", ed. V. Barnett and F. Turkman, Wiley, Chichester, pp. 297-318.

Yang, J., Reichert, P., Abbaspour, K. C. and Yang, H.: Hydrological modelling of the Chaohe 4 Basin in China: Statistical model formulation and Bayesian inference, J. Hydrol., 340(3-4), 5 167–182, doi:10.1016/j.jhydrol.2007.04.006, 2007."

Id./Comments/Response

R1-1 "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."

As pointed by Reviewer 1, in the study of Romanowicz et al. (1994) an autoregressive error model (AR) with a logarithmic transform (as a special case of the Box-Cox transformation) to stabilize error was also previously applied in Generalized Likelihood Uncertainty Estimation (GLUE). We have updated our citations referring to the use of AR in l. 16 p. 8.

R1-2 "Bayesian methods are useful only when observations are available. How can this requirement be met in an ungauged catchment?"

In a Bayesian framework a parameter distribution must be available from the beginning, typically derived from expert knowledge. This distribution can be updated with data if available. The model's predictive distribution, however, is calculated in the same way, irrespective if the parameter distribution was updated or not (see equation (5) and (7) in the manuscript).

To clarify this point we included more information on the line 20-22 p.7 and l. 11-12 p. 8.

R1-3 "the Bayesian methods do not allow for the separation of the influence of different sources of uncertainties on the output"

Error separation within a Bayesian context is possible and has been applied in to different extends in the last years (Kavetski et al., 2006a, 2006b; Vrugt et al., 2008a; Renard et al., 2011; Honti and Reichert, 2012; Reichert and Schuwirth, 2012). Conceptually linearity is not required but it might allow for simpler analytical solutions. Parameter uncertainty is already separated in the very ‘classic’ application of Bayesian inference. The input uncertainty was separated following the approach of Kavetski et al. (2006a, 2006b), see also appendix B.

R1-4 "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."

This is a valuable comment from Reviewer 1.

Error separation allows to quantify the uncertainty sources. While the approach thereto is conceptually very sound, in practical application the different error models must be simplified to keep the number of parameter reasonable small. The chosen input error model with rainfall multipliers has certainly its limitations, but we see no indication that a more sophisticated model would have altered the conclusions.

As we showed by a comparison of different scenarios, the model was the least sensitive to parameters uncertainty. This, in our view, clearly demonstrates that the input and model structure uncertainties are mostly contributing to the wide predictive uncertainty bounds. This result is similar to the one of (Kuczera et al., 2006) who highlighted the often overlooked fact that poorly determined parameters do not necessarily lead to high predictive uncertainty.

To clarify this point we added additional information l. 27-32 p. 17 and l. 1-6 p. 18 (see also comment R2-2).

R1-5 "The hydrological tools applied are rather dated."

This comment from the Reviewer 1 is not clear for us. Which hydrological tools does the Reviewer refer to exactly?

If the Reviewer means the used run-off model, we agree that the unit hydrograph (UH) model is conceptually simple and more detailed models could be applied. However, the main advantage of this kind of model is that it has parameters that can be inferred from empirical methods with a physical interpretation. As they can be directly transferred to ungauged catchments, they have great practical relevance. We explained the reason behind the model choice on line 26-32 p. 2 and 14-21 p. 6 in the manuscript.

The UH-based models are still frequently used in hydrology especially to predict runoff in ungauged basins (Kumar et al., 2007; Ahmad et al., 2009, 2010; Gironás et al., 2009; Khaleghi et al., 2011), see also l. 28-29 and l. 31-32 p. 2, l. 23-24 p. 6 in the manuscript.

R1-6 "The authors should apply split sample test to show the model performance."

This is a very good point, as cross-validation is excellent to generalize findings to an independent data set, e.g. to assess prediction performance. In the presented study we deliberately chose not to cross-validate the model performance for three distinct reasons. First, cross-validation is important in guarding against overfitting. This, however seems not to be a critical point for this application, as the model is rather inflexible and thus parameter uncertainty has a small influence (l. 21-22 p. 16). Second, cross-validation would have made a direct comparison of different scenarios difficult, because for some no model calibration was performed (scenario A D). Third, and an arguably weak argument, that cross-validation is not necessarily meaningful with a small dataset of only 14 rain events.

R1-7 "were the tools justified by the amount of observations available? In other words, were the tools fit for purpose?"

As mentioned in the above (see comment R1-2), the predictive uncertainties can be calculated even without any data. That means, that, even although prediction uncertainty may be large, the way to quantify the uncertainties does not change.

The uncertainty assessment and its reduction is one of the main research targets of the Prediction in Ungauged Basins (PUB) initiative (Sivapalan et al., 2003; Montanari and Di Baldassarre, 2011). Therefore it is meaningful to recognize main contributors of predictive uncertainties, even for flood assessment in catchments with scarce data.

We consider a simple conceptual model, as those tools are usually a first reasonable attempt if not the only possible to predict flood flows in scarce data catchments. Despite of their simple structure models based on UH are therefore still in common use in current hydrology (see comment R1-5).

Even if our work focused on a lumped conceptual hydrological model (UH), the quantification of the uncertainties could be done in a similar way for more complex, physically based, distributed models.

R1-8 "The prior distributions for all parameters (input, autoregressive model and model parameters) were assumed to be independent. I would expect some comment on the possible issues emerging when this assumption is violated."

This a very valuable comment. The a-priori assumption of an independence between all parameters (input, autoregressive model and model parameters) is common assumption (Yang et al., 2007; Reichert and Schuwirth, 2012) since it is extremely difficult to evaluate and formulate knowledge of their mutual interactions beforehand. We included this point in the manuscript l. 7-10 p. 12.

As the interactions are unknown, an independent prior represents this ignorance/knowledge. As a-prior represents (subjective) knowledge and not the ‘truth’, a violation of the prior is conceptually not possible (although, a good prior is always desirable in praxis).

R1-9 "For the A and CN parameters of the SCN-CS part of the model, a normal distributed error was chosen with 10 precent 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."

With regard to parameters of the SCS-CN method, in our study we informed a parameter of a maximal potential retention of a catchment (S) and not Curve Number (CN). For S we assumed a lognormal distribution with the mean of 55 and standard deviation of 30. The prior S distribution was delivered from the CN parameter. For area of a catchment, how correctly stated by the Reviewer, we used a normal distribution with 10 precent standard deviation of the mean.

Our formulation of the prior knowledge was based on the best estimates. Therefore, since the bias cannot be known a-priori the first choice for it is zero. Going further, that has a valuable meaning since if the bias would be known in advance the model/parameters should be corrected by it. However, we agree with the Reviewer that, although prior parameters of SCS-CN method should represent physical properties of a catchment, the inferred posteriors may also include compensation of model structure deficits. To clarify this point we added an explanation to l. 27 p. 11 and l. 1-3 p. 12.

R1-10 "The choice of rainfall multipliers is not clearly described. 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?"

As stated clearly in the manuscript (Sect. 2.4 Input error model), the uncertainty of the precipitation measurements was tackled with an individual rainfall multiplier for each storm event. The rainfall multipliers are modeled by a lognormal distribution with an expected value of 1 and the unknown standard deviation. That resulted in additional 15 parameters (14 multipliers + standard deviation) that were inferred together with other parameters in the Bayesian inference. This approach requires an event-based modeling approach as for any analyzed storm event a separate rainfall multiplier must be inferred.

Sampling from a 21-dimensional parameter space is computational intense. However, as MCMC sampler scale reasonable well to high dimensions and as the model runs quite fast the computation was manageable. Additional information was added into the manuscript (l. 22-25 p. 12).

R1-11 "App. A, not all the notation is explained. What is ti, n, what is the observation equation?"

Good observation. The missing notations are now explained in the manuscript. See Appendix A l. 15 p. 20 and 2-3 p. 21.

R1-12 "how is the fact that the observations are discrete dealt with?"

The error model is continuous in time. That means that in contrast of a AR model, the measurements can be taken at any point in time and not only at discrete time steps. This is a very helpful property if data are not equally spaced in time (e.g. due to missing values).

R1-13 "use Poland instead of PL and “flow” or “runoff” instead of “flooding”"

We changed the notations throughout the manuscript (l. 26 p. 1, l. 27-28 p. 5, l. 13 p. 6, l. 19 p. 13).

R1-14 "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?"

We updated the missing notation on the figure 5. Since the other Reviewers did not complain about Fig. 5 we would like to keep the figure as it is. In our opinion, a more detailed figure would not bring additional insight into the obtained results.

Reviewer 2 Hilary McMillan

“This paper presents a method for flow prediction and uncertainty assessment suitable for small/poorly-gauged basins, using Bayesian methods including explicit representation of input rainfall errors. Flow measurement and structure errors are represented jointly by an autoregressive error model using a Box-Cox transformation. In general I found the paper to be very well organised and well explained. Although similar methods may have previously been applied, this paper is a worthwhile addition to the literature since it demonstrates clearly how to apply the most recent techniques to a typical urban catchment with poor gauging record. The method devised by the authors for deriving prior distributions on the model parameters using multiple regionalisation algorithms is particularly useful. There are some points which should however be addressed prior to publication: 1. The authors do not separate model structural errors and flow measurement errors, and comment in several places that flow measurement errors are small, and therefore this source is dominantly caused by structural error. Although I agree that it is not necessary to separate all error sources where there is insufficient information to do so, this should be clearly stated and flow measurement error should not be ignored. Recent papers such as McMillan et al (2010), Thyer et al (2009) and Westerberg et al (2011) have all demonstrated that flow measurement errors can be large and have a significant impact on parameter inference. 2. The authors draw strong conclusions about the dominance of input uncertainty. However the increased posterior standard deviation of rainfall multipliers compared to the prior might be partly due to the high degree of freedom allowed when all 14 rainfall multipliers are inferred from limited available data. In effect, some of the output uncertainty might be implicitly included in the input uncertainty. Recent papers by Renard et al (2010, 2011) explore very similar questions and conclude that weak priors on rainfall multipliers can lead to overestimation of the posterior standard deviation. The authors should reference these papers and comment on how the findings relate to their work.

Minor Comments: 1. The abstract states that predicted flows were ’up to 7 times higher' than observations, but then 'this was reduced by 150 precent' by Bayesian updating. The language is unclear - please say how many times higher were the flows after updating. 2. May be better to refer to the catchment as 'poorly gauged' rather than 'ungauged' given that gauges were available for the catchment. 3. Figure 1 was hard to understand and did not add to the discussion, it could be deleted. 4. The priors for the parameters were fitted using lognormal distributions (Page 11086 Line 14). Please comment on why the lognormal was chosen and whether it can be satisfactorily fitted with only 5 data points. 5. The CN parameter was given a prior with standard deviation 10 precent of the mean. That seems rather small for a fitted parameter - please comment. 6. Please update the McMillan et al (2010) reference - citation below.

References:

Renard, B., D. Kavetski, G. Kuczera, M. Thyer, and S.W. Franks (2010), Understanding predictive uncertainty in hydrologic modeling: The challenge of identifying input and structural errors, Water Resour. Res., 46, W05521, doi:10.1029/2009WR008328.

Renard, B., D. Kavetski, E. Leblois, M. Thyer, G. Kuczera, and S. W. Franks (2011), Toward a reliable decomposition of predictive uncertainty in hydrological modeling: Characterizing rainfall errors using conditional simulation, Water Resour. Res., 47, W11516, doi:10.1029/2011WR010643.

McMillan, H., J. Freer, F. Pappenberger, T. Krueger, M. Clark (2010). Impacts of Uncertain River Flow Data on Rainfall-Runoff Model Calibration and Discharge Predictions. Hydrological Processes 24(10): 1270-1284 DOI: 10.1002/hyp.7587

McMillan, H., B. Jackson, M. Clark, D. Kavetski, R. Woods (2011). Input Uncertainty in Hydrological Models: An Evaluation of Error Models for Rainfall . Journal of Hydrology 400(1-2): 83-94

Thyer, MA; Renard, B.; Kavetski, D; Kuczera, G (2009) Impact of runoff measurement

error models on the quantification of predictive uncertainty in rainfall-runoff models. 18TH WORLD IMACS CONGRESS AND MODSIM09 INTERNATIONAL CONGRESS ON MODELLING AND SIMULATION: INTERFACING MODELLING AND SIMULATION WITH MATHEMATICAL AND COMPUTATIONAL SCIENCES Pages: 3414-3420 Published: 2009

Westerberg I.; Guerrero J. -L.; Seibert J.; Beven, KJ; Halldin, S (2011) Stage-discharge uncertainty derived with a non-stationary rating curve in the Choluteca River, Honduras. HYDROLOGICAL PROCESSES 25(4): 603-613 DOI: 10.1002/hyp.7848

Id./Comments/Response

R2-1 " 1. (major) Although I agree that it is not necessary to separate all error sources where there is insufficient information to do so, this should be clearly stated and flow measurement error should not be ignored. . ."

This is a very valuable comment. The measurement error of water levels if directly measured is assumed to be small (Di Baldassarre and Montanari, 2009). In contrast, the error of flow observations might be large, especially when no reference or calibration measurements are available. This is mostly due to error of the rating curve, which is commonly employed to transfer measured water levels into observed flows (Di Baldassarre and Montanari, 2009; Montanari and Di Baldassarre, 2011). Usually, the rating curve error is included within the model structure error and cannot be here distinguished from it. As our results show that the model structure error is significant, we would like to separate those sources in our next study. Additionally, the rating curve error strongly depends on the case study and may be significantly reduced by eliciting the maximal information content from the gauging station (McMillan et al., 2010). Here, for this particular stream gauge and cross section, we possess detailed information of the rating-curve from own field experiments and reference measurements. Therefore we are confident that the flow data are accurate within the specified tolerance levels. To clarify this point we included additional information in Introduction part (l. 31-32 p.

3, l. 1-4 p. 4), error model description (l. 8-10 p. 10) and in the Material (l. 10-14 p. 14) and Discussion (l. 1-6 p. 18).

R2-2 " 2. (major) The authors draw strong conclusions about the dominance of input uncertainty... Recent papers by Renard et al (2010, 2011) explore very similar questions and conclude that weak priors on rainfall multipliers can lead to overestimation of the posterior standard deviation. The authors should reference these papers and comment on how the findings relate to their work."

This is again a valuable comment. We are also greatly thankful for pinpointing to us the relevant current literature on similar aspects (Renard et al., 2010, 2011), where the problem of reliable total predictive uncertainty of rainfall-runoff models and contributions of different uncertainty sources were addressed.

With regard to the dominance of input uncertainty, we agree that the increased posterior of input uncertainty may potentially be caused by mutual interactions of different uncertainty components. However, in our opinion, not the output uncertainty but rather a model structure error may infer with input uncertainty and lead to a wider posterior. We are aware of that problem and will address it in a future study.

Consequently, the inferred rainfalls should be treated rather as estimated inputs than “real” observations (Seibert and Beven, 2009), see also the Discussion in the manuscript (l. 27-32 p. 17 and l 1 p. 18). However, this is only a case when a poor prior knowledge on rainfall multipliers is provided (Renard et al., 2010). Contrary, an informative prior allows avoiding this problem (Renard et al., 2011) and even in ungauged basins, as in our case, a valuable prior knowledge can be still obtained from experts' and engineers' experience or short observations.

Furthermore, we agree that due to the mutual interactions it is problematic to separate contributors of total predictive uncertainty. However, in practice, most of interest is the evaluation of the relative importance of uncertainty components on total uncertainties. From our results the structure error and input uncertainty were found to be the most

significant. That is partly in agreement with findings of Renard et al. (2011) who, however, pinpoint only a structure error as the most critical uncertainty source. However, as stated in the same paper, both sources of uncertainty are strongly dependent on the case study, specifically of the layout of the rain gauge network (input uncertainty) and the simplification of the real system (structure error). It is hard to not agree that a variation of rainfall fields within an urban basin (as in our case) may be large, whereas the model structure error, which depends on both the model abstraction and the uncertainty of calibration data, may be high for a conceptual model as applied here.

With regard to these aspects, we included an additional point in our discussion part “uncertainty contributions and their difficulties” (l. 12 p. 16, l. 27-32 p. 17 and l. 1-6 p. 18).

R2-3 " 1. (minor) The abstract states that predicted flows were 'up to 7 times higher' than observations, but then 'this was reduced by 150 precent' by Bayesian updating. The language is unclear - please say how many times higher were the flows after updating."

The direct verification is presented in table 3 in our manuscript. The uncertainty bounds after Bayesian updating with available data were reduced from 7 times higher than observations to 5 times higher. To clarify this point we changed the notations in the abstract, results and conclusions (l. 1 p. 2, l. 1 p. 16, l. 1 p. 20).

R2-4 " 2. (minor) 'poorly gauged' rather than 'ungauged'"

It is true that in our specific case the catchment may refer to poorly gauged instead of ungauged. However, with our manuscript we wanted to address problems of flood predictions in ungauged catchments and our case study was only an example of a practical application with more data than usual available. Therefore we suggest to keep the terminology unchanged.

R2-5 " 3. (minor) Figure 1 was hard to understand and did not add to the discussion, it could be deleted."

We suggest to keep figure 1 since it provides an overview of our method. This apparently did not raise concern from the other reviewers. However, we improved the annotations on the figure.

R2-6 " 4. (minor) Please comment on why the lognormal was chosen and whether it can be satisfactorily fitted with only 5 data points."

Selecting a parametric form to represent a prior is always difficult. Obviously, five data point do not allow for a very precise density estimation. However, in our practical experience the influence of the choice of the parametric distribution family is minimal as long they have the same support (e.g. Weibull or Gamma distribution).

R2-7 " 5. (minor) The CN parameter was given a prior with standard deviation 10 precent of the mean. That seems rather small for a fitted parameter - please comment."

This comment is misleading. We defined our prior for S (maximal potential retention of a catchment) instead of CN and for S we provide a sufficient wide prior distribution (see table 2 and the comment R1-9)..

R2-8 " 6. (minor) Please update the McMillan et al (2010) reference"

We updated to (McMillan et al., 2011).

Other Changes made by the authors

In addition to the suggestions of the reviewers, we changed the following details which we noticed during our internal revision:

l. p. Changes

l. 29 p. 2 Added reference: Sikorska and Banasik, 2010

l. 11-15 p.4 Added information: A promising approach to treat the introduced input error originally proposed by Kavetski et al. (2006a) an adopted by others (e.g. Vrugt et al., 2008a, 2008b; McMillan et al., 2011) is to tackle the uncertainty of the precipitation measurements with estimating event-specific parameters (rainfall multipliers).

l. 21 p. 6 Added reference: Sikorska and Banasik, 2010

l. 12 p. 9 Added reference: Honti and Reichert, 2012

l. 26 p. 9 Changed: no explicit rules exist (O'Hagan, 1998; Scholten et al., in prep.).

l. 21-22 p. 10 Other methods (Haan et al., 1994; Bhunya et al., 2003; Jain et al., 2006; Singh, 2007) are not suitable as they relate IUH characteristics to discharge properties that are not available for ungauged catchments, however, may be extended to if such data available.

l. 12-15 p. 12 Changed: In the context of floods the predicted peak flow is often the most important model result. For Sluzew creek, it can be readily transferred to stream water levels and flooded areas during a flood event. Specifically, we used the peak flow and its 80 percent-interquantile range to assess the model performance.

l. 22 p. 12 updated with calibration data of 14 rain events

l. 25-27 p. 12 Changed: This is the best option to predict the run-off of a future rain event, for which an appropriate multiplier cannot be known in advance.

l. 4 p. 13 Updated reference: R Development Core Team, 2011.

l. 20 p. 13 The average annual precipitation in this part of city is about 540 mm and. . .

l. 21-22 p. 13 Added reference: Majewski et al., 2010.

l. 22-23 p. 14 and a mean of 1.78 h and standard deviation of 0.86 h for N and k respectively.

l. 27 p. 14 Additionally, all 14 rainfall multipliers were inferred together with model parameters.

l. 5-6 p. 15 This means that by the prior knowledge the input error may have been slightly underestimated. Further explanation is provided in the Discussion (Sect. 5).

l. 12 p. 15 The assumption of the continuous AR process with independent innovations is fulfilled much better (Fig.4, bottom row), even if

l. 15 p. 15 Not surprisingly, the residuals show a strong autocorrelation (Fig. 4, top row).

l. 10-14 p. 16 Changed: Here, we would like to discuss four important aspects, namely i) the obtained results for the prior and posterior parameter distribution, ii) the choice of the likelihood function and the consideration of input uncertainty, iii) uncertainty contributions and their difficulties, and iv) problems with assessing the consequences of urbanization and modelling in ungauged basins with a brief outlook on future challenges.

l. 20-21 p. 16 (shown by a comparison of scenarios B C)

l. 22 p. 16 (shown by scenarios A D).

l. 6-7 p. 17 Interestingly, the posterior mode of SIGMA is more than two times smaller than the prior, which was rather unexpected and is discussed further below.

l. 8 p. 17 With respect to point ii..

l. 10 p. 17 Changed: Here, reasonable uncertainty bands were obtained with the proposed autoregressive error process. . .

l. 18 p. 17 Second it, allows for a better fit to the data (not shown) and an estimation of the uncertainty in the input.

l. 7 p. 18 With regard to point iv), we found. . .

l. 23-24 p. 19 Changed: Our results for the latter might therefore be beneficial for other

studies in similar basins.

l. 6 p. 20 Changed: The second main contribution are model structure deficits, whereas the parameter uncertainties were found to be not so important.

l. 10-12 p. 28 Corrected: Scholten, L., Scheidegger, A., Maurer, M. and Reichert, P.: Combining quantitative expert knowledge and local data for enhanced service life modeling of water pipes under lacking data, in prep..

l. 32-34 p. 29 Corrected: WAU: Hydrologic documentation of the Sluzew Creek at Pszyczolkowa gauge station (in Polish), Internal Report (led by K. Banasik), Warsaw Agricultural University - SGGW, Dept. of Water Engineering and Environmental Restoration, Warsaw., 2002.

Tab. 1 p. 31 Changed: Row 5, column 2: (RB/RA), deleted: Row 3 column 3: (i), Row 5, column 3: (ii).

l. 2 p. 31 Delated: (i) (ii).

l. 4 p. 39 Changed: standardized residuals (top row),

l. 4-8 p. 40 Changed: Solid lines preset predicted runoff corresponding to the median parameters values, dotted lines – observations. Grey areas depict 80

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