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

A. E. Sikorska et al.

anna_sikorska@sggw.pl

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AUTHORS’ FINAL RESPONSE

Dear Associate Editor and Referees,

We would like to thank both referees and editor for their valuable comments, which improved the presentation of our study. Based on their feedback, as well as additional internal reviews, we made major changes to the manuscript. Our replies to the first Referee’s (GDB) and Editor’s comments, which were generally very positive, may be found at http://www.hydrol-earth-syst-sci-discuss.net/10/C1648/2013/ and http://www.hydrol-earth-syst-sci-discuss.net/10/C1653/2013/ respectively. In the following, we answer the comments of the second Referee (*in italic*). In our answers we refer to the original manuscript (OM) or the revised manuscript (RM) where appropriate. For better visibility, the essential reviewer's comments are highlighted in orange. Thank you very much for your time and considerations.

On behalf of the authors, sincerely,
Anna E. Sikorska
Swiss Federal Institute of Aquatic Science and Technology, Eawag
Warsaw University of Life Sciences, WULS-SGGW

Referee 2 (Manfred Ostrowski)

General comments

The manuscript describes the investigation of the uncertainty of parameters of hydrological model and the effect of errors contained in stage discharge relationships, called rating curve model by means of a case study in Poland. The case study deals with a densely urbanised catchment, i.e. with an urban drainage problem. The case study has been published previously (Sikorska et al.(2012)). Such segmentation of articles is neither helpful for their analysis nor generally recommended in peer reviewed publications.

We disagree with the reviewer on two points. First, the problem of stage-discharge relationship in a receiving water is not an urban drainage problem (see below for a detailed justification). Second, our two manuscripts are independent methodological papers that tackle different research questions with different methods. Although the developed methods were indeed demonstrated on data from the same catchment, Sluzew Creek in Poland, they contain enough new knowledge and insight to warrant publication.

The uncertainty analysis uses approaches transfered from mathematics which have been most freqently applied in hydrological modelling during the last two decades, in fact without any visible consequence for modelling in practice. Overall, the research *study is an interdisciplinary approach covering environmental science (mixed rural and urban hydrology and hydrodynamics), and mathematics (models based on linear differential and non linear empirical equations and statistics). Although not in the center of hydrology, the manuscript is still in the scope of HESS.*

We agree that "uncertainty analysis", which is a very broad term, has been applied in various context in hydrology. We also agree that transferring scientific results into practise is certainly challenging. However, this is neither the aim of this paper, nor does it devaluate attempts to improve the rigour of practical analysis, e.g. by means of investigating uncertainty.

The reviewer has considerable concerns against the publication of the manuscript, which will be explained below according to the recommendations for reviewers of HESS.

We are very surprised by the negative perception of Reviewer 2, most of all because the other referees did not raise fundamental criticism. We are thankful for some very constructive arguments, which helped us better present the study. Furthermore, we will show that, while some arguments can also be attributed to a misunderstanding, many miss the original point.

1. Does the paper address relevant scientific questions within the scope of HESS?

The paper adresses relevant scientific questions, but is of very limited practical relevance. The paper deals with a highly urbanised small river basin, which has been dealt with by urban drainage modelers so far. Existing knowledge from this scientific area is hardly addressed.

We disagree with the Referee that stage-level relationships are in the focus of the urban drainage community. First, the main focus of our paper is not to develop an optimal model, e.g. solving the Saint-Venant equations for flow in open and pressurized channels, for the urban drainage system and receiving waters. Instead, our focus lies on providing a method to quantitatively assess uncertainty of water level predictions and the relevance of rating curve uncertainty on the uncertainty of the predicted variable. In this, the paper is in our opinion relevant because it allows one for systematically incorporating the uncertainty of calibration data i.e. streamflow to quantify the total uncertainty in water level predictions. Furthermore, although the test catchment here applied is urbanized, the method is also applicable (and potentially even more important) for rural catchments where usually less data are available and the uncertainty in the rating curve may be of a higher importance due to more significant changes in the cross-section characteristics, e.g. seasonal, vegetation and alluvial changes.

2. Does the paper present novel concepts, ideas, tools, or data?

2a. *The paper uses uncertainty analysis approaches which have been most frequently presented in literature; there is no need to cite them (Baysian statistics, Marcov Chain Monte Carlo, Box-Cox), it would count to several tens. The paper contains no innovative issues except for the analysis of the stage-discharge relationship. However, a very similar paper has been published by McMillan (2010), which is cited but not discussed at adequate depth.*

We certainly do not claim any innovation in the areas of probability calculus nor numerical techniques for Bayesian inference, such as Markov Chain Monte Carlo. However, as far as we know, a formal statistical analysis of how the uncertainty in the rating curve calibration data propagates through hydrological predictions has not been covered in literature yet.

In contrast, McMillan et al. (2010) investigated the impact of errors in streamflow measurements on streamflow predictions informally. Specifically, they used a resampling approach to construct uncertainty intervals of the rating curve (RC). However, they only give an algorithmic description of their approach - a fundamental mathematical deriva-

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tion is missing. First, in contrast to McMillan (2010), we do not see the need to develop new approaches for the calibration of the RC's, because this is a classical non-linear regression problem. We propose a statistical regression framework, because regression problems have been extensively studied in the statistical literature of the last 50 years and many - well founded - methods are available to estimate the function and its uncertainty (e.g. maximum likelihood estimation, Bayesian inference, quantile regression, non-parametric curve fitting).

Second, in contrast to, McMillan et al. (2010) we suggest to use a formal likelihood approach to calibrate the RR model. McMillan et al. (2010) calibrated the RR model with an informal "likelihood", somewhat in the spirit of the "Generalized Likelihood Uncertainty Estimation" (GLUE) (Beven and Freer, 2001). This is conceptually unsatisfactory and has several shortcomings, because it i) maps all uncertainties onto the parameters, ii) prevents learning from additional data, and iii) does not allow for quantifying the contribution of different uncertainty sources to the predictive uncertainty (e.g. Vrugt et al., 2008; Kavetski et al., 2006; Sikorska et al., 2012). A discussion of formal and informal likelihoods (e.g. Vrugt et al., 2008; Mantovan and Todini, 2006) is not the scope of this paper. However, we agree with the reviewer that McMillan et al. (2010) should be discussed in greater depth in the paper and revised the manuscript accordingly, l. 22-28 p. 4 in RM: "For the applied resampling approach to construct uncertainty intervals of the RC model is no justification provided. The subsequent calibration of the RR model is based on an informal likelihood with the help of an Monte Carlo Markov chain algorithms. A consequence of the application of an informal likelihood function is that the resulting prediction uncertainty is mapped on the RR model parameters entirely. This prohibits an assessment of the importance of the different sources of uncertainty, such as rating curve and rainfall-runoff model parameters."

2b. *Also the paper largely ignores heuristic knowledge about the determination of stage-discharge relationships which are in the focus of hundreds of operational hydrologic institutions. Sikroska et al (2012) say that usually modellers do not give sufficient* *attention to errors contained in stage discharge relationships which is a hypothesis without any prove. In contrast responsible modellers in practice will analyse the plausibility of the stage discharge relationship in the first step of a study. Building up an adequate data base in applied modelling often requires about 60% of the project duration.*

It must be a misunderstanding on behalf of the reviewer that we hypothesize that “modellers do not give sufficient attention to errors in stage discharge relationships”. We never make this statement, neither in the submitted manuscript nor in our previous publication (Sikorska et al., 2012). Instead, we most definitively agree with the reviewer that data pre-processing has a significant impact on results further obtained. Actually, we write that the measurement errors of the calibration data for rainfall-runoff models i.e. streamflow “are often considered to be relatively small” (Sikorska et al., 2012) and that the impact of the uncertainty in calibration data, typically streamflow derived from measured water level by means of a rating curve, is hardly considered quantitatively by hydrologists (l. 6-15 p. 2957 in OM). These two aspects are not equal since the first investigates the measurement errors in stage-discharge relationships alone, whereas the second one considers the uncertainty in calibration data derived with stage-discharge relation and its propagation on the predicted water levels or streamflows. To clarify this point, the sentence in l. 22-23 p. 2 reads now: “Typically, the influence of the uncertainty in streamflow data for RR models on the predictive uncertainty is hardly assessed quantitatively by hydrologists in scientific literature.”

2c. *It is obvious that the model applied is inadequate (A). First of all, linear model have been frequently critised as inappropriate and such a model is definitely not suitable in this case. (See Ostrowski, (2003)). Also, the manuscript lacks an assessment of the simulation quality (B) e.g. by showing the Nash and Sutcliffe criterion. [. . .] For the simulation of highly urbanised catchments in science and practice much more appropriate models such as the open source Stormwater Management Model (SWMM) by the US Environmental Protection Agency (2013) is readily available which has a history of*

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some fourty years and is frequently updated and improved. For this model quite well funded prior parameter distributions are available. The study is based on a partial series of rainfall- runoff events. As an analytic simulation algorithm is used and the record period is only three years it is hard to understand, why this limitation was chosen. A continuous simulation over the full observation period (A) would have been appropriate and would provide the opportunity for detailed plausibility assessment. It must be concluded that the modelling approach is definitely behind the standards applied in most industrial states and regions which is a continuous, non-linear modular model structure at high spatial (few hectares) and temporal (5 minutes) resolution. Increasingly, the setup of urban drainage models is supported by specific measurement campaigns (C), when high resolution rainfall and runoff is measured for optimum parameter estimation.

Point A: Inadequacy of the chosen model:
We agree with the reviewer that the choice of the proper model structure is challenging. Clearly, it does not only depend on the complexity of the model structure, but mostly on the purpose of its application and availability of required input and output data (Dotto et al., 2012; Dunn et al., 2008). Specifically, more complex models not always provide more accurate predictions because the number of model parameters which need to be inferred increases (Beck, 1991) and thus usually additional recorded data are required i.e. evaporation or digital terrain model of the catchment. For small and/or poorly gauged catchments, as our catchment, such data are usually not available. In this regards, we added in l. 17-19 p. 18 in RM: “Although the model accuracy usually increases, this must not necessarily lead to reducing the predictive uncertainty because of increasing parameter uncertainty (Sikorska, 2013).”

We furthermore agree with the Referee that the applied hydrological model is a simple conceptual model of linear reservoirs and his limitations must be acknowledged, as it was done in the discussion (see also Sikorska, 2013). However, our approach is independent of any hydrological model. Therefore, the chosen type of model is less

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important, because it only serves as a vehicle to illustrate the presented uncertainty analysis approach. To better explain our choice, we must emphasize that it is still a common practise to apply a simple lumped event-based rainfall-runoff model to predict catchment response from a design or predicted storm. As in our case study, such lumped models are considered appropriate when the modelling focus lies in the catchment outlet (Coutu et al., 2012). Abundant examples may be found in recent scientific literature (e.g. Croke, 2006; Goñi et al., 2013; Grimaldi et al., 2012; Li et al., 2008; Mohan and Vijayalakshmi, 2008; Sahoo et al., 2006; Seibert, 1999; see also Sikorska, 2012).In addition, such models are also still popular in practical design for hydraulic infrastructure e.g. in UK (Kellagher, 2012) or Poland (KIW, 2002). Last but not least, such models due to their simplicity and transparency are frequently applied to investigate prediction uncertainty (e.g. Blöschl and Montanari, 2010; Dotto et al., 2012; Sadegh and Vrugt, 2013; Seibert and McDonnell, 2013; Uhlenbrook et al., 1999).In general, prior distributions must be specified for each case study independently. This required adequate information on case catchment properties and required information which usually increases with the complexity of a model; the number of model parameters to be inferred increases.

Point B: Lack of assessment of the simulation quality:
In regard to the assessment of the simulation quality, we modified the Sect. 2.6. which now reads “Cross-validation for predictions and prediction measures” by adding the following paragraph in l. 17-22 p. 9: “The efficiency of model predictions is measured by the Nash-Sutcliffe (NS) index (Nash and Sutcliffe, 1970) estimated for the best model prediction (mode of the posterior). The uncertainty bands are assessed by the data coverage DC(α) (also known as reliability, Del Giudice et al., 2013; Montanari and Koutsoyiannis, 2012; Sikorska, 2013) defined as the fraction of the data covered by the prediction interval given by the α/2-quantile and the (1-α/2)-quantile. Theoretically the DC(α) should be larger or equal 1-α.” Following that, we provided the Nash-Sutcliffe (NS) criterion in RM in the result section. In this regard, we added in l. 20-22 p.

14: “The obtained average Nash-Sutcliffe index for the best model prediction (mode) during the validation mode was 0.61 over all events and 0.69 when excluding events No. 2 and 8. This can be considered as satisfying as for the simple hydrological model here applied.” We also added the following sentences in l. 24-28 p. 11 in RM: “Lumped models are justified when the modelling focus lies in the basin outlet only (Coutu et al., 2012). Such models, due to their simplicity and transparency of modelled patterns, are frequently applied to investigate prediction uncertainty and the value of data (e.g. Blöschl and Montanari, 2010; Dotto et al., 2012; Sadegh and Vrugt, 2013; Seibert and McDonnell, 2013; Uhlenbrook et al., 1999).”

Point C: The setup of urban drainage models is supported by specific measurement campaigns:
This comment of the reviewer is a bit unclear. We suppose that he suggests that using a distributed model should be feasible when sufficient data are available. Although this is probably the case, the central message of our approach targets the regression framework of the RC and not the type of model.

2d. *The catchment investigated is a highly complex system, as it is very flat and directly connected to the Vistula River flood plain. It contains many hydrologically dominating elements of civil infrastructure (a large airport, several highways, industrial and domestic buildings, etc) as well as hydraulic infrastructure such as reservoirs, culverts, pipes, canals etc. The catchment properties are hardly explained neither in this manuscript nor in the underlying paper from 2012. The reviewer had to use Internet resources (Google Earth) to identify the particular structure of the catchment. Also the hydrograph in Figure 5a shows that the flow is highly influenced by in-system storage effects.*

We very much appreciate the Referee’s efforts to deliver a high-quality review, going to great lengths to identify specific properties of the Sluzew Creek catchment. We change our manuscript and concisely summarized the necessary information from lit-

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erature (e.g. Banasik et al., 2007; 2008; Barszcz, 2009; Sikorska and Banasik, 2010), which we added to the reference list in RM (see below). However, reporting the tiniest details of the case study is not the focus of our study. For example, regarding the in-system storage, Sluzew Creek is a flat partly urbanized catchment. The urbanization ratio for the catchment till the analysed stream profile equals 58.7%. As for an urbanized stream, few culverts are located along the main stream, which may disturb the streamflow during extremely high events which were not observed over analysed period of 8 storm events in 2011 year. The in-system storage effects, which can be seen in the Figure 5a, are rather successfully modelled by the applied model for most events. Thus, remaining systematic errors can be rather explained by the rainfall variability and not by effects of the infrastructure of the drainage system.

It is also worth noting that the airport located within the catchment is provided with retention ponds which control the outflow from the airport surface and which is limited to a certain flow rate (Barszcz, 2009). Thus, the influence of the airport on supplying Sluzew Creek in surface water is greatly reduced.

In summary, we followed the suggestions of the reviewer and improved the description of the case study by adding the following paragraph in l. 23-27 p. 10: “Although the catchment is partly urbanized and a few anthropogenic hydraulic infrastructures are located along the stream, the streamflow is not disturbed during ordinary to middle-high flow conditions (Barszcz, 2009). At the analysed gauging profile an undisturbed streamflow is observed till the water level exceeds 180 cm (see Fig. 1). For more details on the case study, the reader is referred to Banasik et al. (2008) and Sikorska and Banasik (2010).”

2e. *The most critical point, however, is the stage-discharge relationship used, which is in the centre of the paper. This relationship has been established by the authors themselves. Internationally (ISO (2007)) and normally nationally, e.g. USGS (see Buchanan et al (1969)) standards have been set up to minimise error impacts. For the gauge discribed only a first estimate for the relationship has been set up with respect* *to the number of required stage- velocity measurements. Also, the hydraulic regime requires a deeper discussion of the unsteady flow impacts and nonstationarity of the relationship. As the relevant information is missing, the author estimated e.g. the stream bed slope which might be well below the threshold defined by Dottori et al. (2009) to apply more sophisticated approaches.*

Unfortunately, we must disagree with the Referee on this point. First, the suggested regression framework is independent of the type of rating curve. Second, our choice of a very simple power law rating curve in our application example is justified (see also below). It has been established for the analysed catchment of the Sluzew Creek and profile following international guidelines (WMO, 2008). This means that all field measurements of stage-velocity records were taken following these rules, especially the number of each stage-velocity measures per profile per each stage-discharge observation. As the Sluzew Creek is a poorly gauged catchment, no historical hydrometric data are available. Thus, the rating curve was established based on the stage-discharge observations taken during our own monitoring campaign in 2011. Clearly, it is not possible to take numerous measurements over all flow rate conditions i.e. flood flows and drought flows during such a short period of observation (one spring-summer season in 2011). A limited range of stage-discharge observations is an often case for any cross-section and case study.

Obviously, the stage-discharge relationship described by the rating curve is subject to errors due to diverse factors such as non-stationarity due to unsteady flow, vegetation changes, sedimentation and bed erosion. This has been extensively discussed in the Introduction (l. 5-19 p. 2958 in OM). Such errors were the main motivation to develop the presented approach. All these errors are modelled by the introduced error term (E_t^{RL} in Eq. 1 in OM/RM).

We agree that applying more sophisticated approaches as proposed by Dottori et al. (2007) could greatly reduce the uncertainty in rating curve in unregular or changing cross-sections when sufficient data are available. Unfortunately, from reviewer's comment, it is not entirely clear which other relevant information is lacking in our study. In our case, because all stage-discharge measurements were taken during spring-summer seasons, the variation of the rating curve due to seasonal and alluvial changes within the channel, which usually could be significant, may be assumed to be irrelevant. Thus, it is justified to use a single power law equation and this was proved by the Fig. 1 in OM. In other cases, it can be recommended either to use a rating curve with a few power law equations or to model the non-stationarity explicitly by e.g. calibrate a rating curve under different conditions or providing a non-stationary rating curve (Dottori et al., 2007; Westerberg et al., 2011). In this regard, we provided additional information in Sect. 3.2 in RM, l. 11-14 p. 11: “Alternatively, if a single power law equation does not fit the observed data sufficiently well, more sophisticated structure of a RC (Dottori et al., 2010) or a non-stationary RC (Westerberg et al., 2011) could be applied.” In regard to these comments we also corrected the manuscript by adding the following sentences: “[. . .] and the recommended range for its extrapolation was set at 180 cm (see Fig. 1). This water level was not exceeded for any of the analysed storm events.” in l. 5-6 p. 11 into the sentence: “An empirical RC was constructed based on 11 water level-streamflow records using a power-law model.”

2f. *Also the vicinity of the Vistula Rives gives rise to the hypothesis that backwater effects are relevant here.*

Backwater effects from the Vistula river are not observed in the Sluzew Creek, especially at the analysed profile. First, the Sluzew Creek is not a direct tributary of the Vistula river but of the Wilanowka river with which it is connected through the Wilanowskie Lake. Only the Wilanowka river flows into the Vistula river (see Fig. A1 in Supplement). As can be seen from the Fig. A1, the analysed profile is located much upper in respect to the Vistula river (ca 5.4 km upstream and ca 14.5 m higher from the Sluzew Creek outlet into the Wilanowskie Lake). Also, mean water level in the Wilanowskie Lake is about 2.0 meters higher than the average water level in the Vistula river (KIW, 2002). Second, the Vistula river floodplains are protected from

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flooding by levees, and by a lock culvert valve located at the outlet of Wilanowka River protecting from discharging water from the Vistula River during her flood flows. To clarify this point, we included the following sentence: “Sluzew Creek is a third-degree watercourse and a tributary of the Wilanowka river, which flows into the Vistula River.” in l. 15-16 p. 10 in RM.

3. Are substantial conclusions reached?
There are very limited general conclusions reached. The results can hardly be transfered to other applications (models, measurements and catchments.)
Regarding the conclusions, the reviewer’s comment is too general. Therefore, it remains unclear for us which of our conclusions are not reached specifically. Unfortunately, he does not support his very strong critique at all.

Furthermore, he does not substantiate why exactly our results are hardly transferable. As this is a methodological paper, the developed methodology can be readily transferred to other catchments and is applicable for other hydrological model as well. This is clearly stated in the paper (p. 5, l. 7-9 in RM). However, It was not our aim to provide results in terms of quantitative values that are globally transferrable to other applications.

4. Are the results sufficient to support the interpretations and conclusions?
The results led to quite vague and partly contradictive conclusions
Unfortunately, this comment is itself quite vague and not supported by evidence. We carefully checked he conclusions again, but could not find any, even partly, contradictive statements.

5. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

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As the data collection methods, the model and uncertainty analysis applied have been known for long it is easy to follow the theory and its application. No reference is made where the data could be received to do experiments by other scientists with the same data

It is a very valuable comment to indicate how the data and code could be made available for further analysis or archived. The code for the uncertainty analysis developed in R programing language is available at request via corresponding author. Unfortunately, due to legality issue, we cannot make the data directly available for general use. However, request for interesting scientific purposes might be considered. In the future, journals should provide service to upload the code (and data, where possible). Other platforms could be Github (https://github.com/) or data google (www.google.com/publicdata/). In this regard, we added a following sentence in l. 10 p. 10 in RM: “The R script is available on request from the corresponding author.”

6. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

The credit being given to former work is insufficient. This manuscript is a representative example for the research depths being applied at present. More information is given below. It is common understanding that a convincing paper must be built upon a solid literature review. Describing the historical development of theory and methods including the most important milestones and the inclusion of relevant non commercial literature are compulsory requirements for successful research. It is the observation of the author that rules of scientific conduct are increasingly violated. In this manuscript, e.g. all relevant publications stemming from the urban drainage community have been ignored (see below), but this seems to occur vice versa. Such processes obstruct the urgently required integration of water modelling disciplines and would be harmful for successful scientific development.

The credit being given to former work is sufficient. It is common understanding that a convincing paper must be built upon a solid literature review and not include too much detail to obfuscate the lack of knowledge to appraise the suggested knowledge gain. Describing the historical development of theory and methods including the achievements and milestones much better fit into a review paper and is not appropriate for manuscripts with methodological advancements. The innovation of the paper is explicitly stated in l. 19-28 p. 2959 in OM. It is carefully introduced after an extensive description of the current state of research in the related field i.e. uncertainty in hydrological modelling and, specifically, investigating rating curve uncertainty.

Unfortunately, it is not entirely clear to what the reviewer refers with “relevant non commercial literature”, because examples are lacking. Regarding literature and methods, such as certain models, from the field of urban drainage, we do not see it relevant to extensively focus on urban drainage modelling since this lies beyond the scope of this paper (see above/below). Instead, the focus lies rather in receiving water from small and poorly gauged catchments. To increase the clarity of the presentation of our study, we therefore removed all links to urbanized catchments from manuscript as they falsely put the focus on urban catchment modelling (keywords l. 13-14 p. 1; abstract l. 27 p. 1; introduction l. 15 p. 5; Sect. 3. l. 1328 p. 10, l. 23 p. 11).

To improve the readability of our contribution, we also added additional sentences into the Introduction section in RM; in l. 16-18 p. 2: “Typically, the accuracy of model predictions and uncertainty estimates need to be assessed against calibration or validation data, which both may be uncertain due to measurement errors.”; in l. 9-10 p. 4: “The main challenge lies in investigating the relevance of individual uncertainty sources.”. In l. 1-5 p. 5 we modified the description as: “This enables the derivation of the predictive distribution of water levels and to simultaneously assess the uncertainty contribution of the RC to the total predictive uncertainty. For the first time, we compare the contribution of the RC to those of the parameters of the RR model formally. The ability to predict water level for given rainfall is important for design studies and risk assessments.” In l. 5-7 p. 5 we added: “Other methods that rely water level forecast on the known water level or discharge (e.g. Coccia and Todini, 2011) are dedicated for operational forecasting and are not considered in this paper."

7. Are the number and quality of references appropriate?
The reference list seems to be quite incomplete. As for general uncertainty issues the manuscript fails to give an overview of the temporal development of uncertainty analysis. In this context, the early contributions by M.B. Beck (1983) should be listed as well as the white paper by the same author(2009) should be given appropriate credit. They allow an assessment of the progress in environmental modelling and related uncertainty. Also the contributions of authors from the field of urban drainage modelling such as Lei Schilling (1996) should be briefly discussed. The recent work by Deletic et al (2012) and Dotto et al (2012) need consideration. As stated above, several authors have investigated the uncertainty related to the SWMM model such as Muleta (2012). These are only a few examples.
We do not find it necessary to refer to all available uncertainty analysis publications. Uncertainty analysis is a very wide field so we prefer to cite the most recent publications that have a direct relationship to our work.

With regard to the publications suggested by the reviewer, we are thankful to pointing us to the manuscripts of Deltic et al. (2012) and Beck (1983, 2009). The work of Deltic et al. (2012) provides an interesting insights into general guidelines for uncertainty estimation with considering different sources, Global Assessment of Modelling Uncertainties (GAMU). Although a qualitative description is useful, practically it does not allow for quantification of uncertainty predictions and the impact of diverse uncertainty sources. We included the work of Deltic et al. (2012) into the reference list. Alternatively, Beck (1983, 2009) focuses on perspectives in environmental modelling, which are mostly seen in the integration of science, society and policy. We agree with the Referee that modern hydrological modelling should cover all these issues. This has been already noticed in hydrological community in IUHS (Montanari et al., 2013). Indeed, we have discussed the practical value of our work and the potential of integration with society in the discussion section point iii in OM. In contrast, we find interesting another work of the same author (Beck, 1991) which presents a theoretical background of uncertainty in hydrological modelling. Thus, we added this paper into the reference list.

In contrast, we find the work of Dotto et al. (2012) not relevant in the focus of the uncertainty analysis approach because, first, it compares the pseudo-Bayesian methods with a Bayesian approach assuming normality (and most likely also independence) of residuals, which was shown to be clearly violated for hydrological models (e.g. Del Giudice et al., 2013, Sikorska et al., 2012). Second, it considers only uncertainty due to model parameters while neglecting other uncertainty sources. In a similar fashion, Lei and Schilling (1996) present an informal approach only for estimation and propagation of model parameter uncertainty on predicted variable without statistically sound backgrounds i.e. under the assumption of a model correctness and certain input data.

Finally, papers analysing uncertainty related to a particular model as e.g. SWMM do not lie in our focus because we do not limit our analysis to a single hydrological model. Also, detailed literature review from urban hydrology is not relevant to this paper, as the presented approach is not limited to urban catchments. Nevertheless we improve the introduction by adding a following sentence in l. 12-16 p. 2 in RM: “Most recently, a few frameworks have been proposed to assess the total predictive uncertainty (e.g. Ajami et al., 2007; Beck, 1991; Del Giudice et al. 2013; Deltic et al., 2012; Kavetski et al., 2006; Montanari and Koutsoyiannis, 2012; Reichert and Mieleitner, 2009; Renard et al., 2011; Yang et al., 2007; Vrugt et al., 2008).”

8. Is the amount and quality of supplementary material appropriate?
The supplementary material does not contain the required information on the case study to understand the particular characteristics of the catchment investigated.
Since our focus does not lie in investigating the case study of Sluzew Creek, we do not see it relevant for our analysis to provide more detailed analysis on the catchment

properties. In contrast supplementary material provides indeed additional information to the results section.

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Other reviewers' comments

Following comments from the Associate Editor, both Referees and our internal review, in order to improve the transparency of our manuscript, we also made the following changes in the RM;

1. In l. 3-5 p. 13 in RM we added: "Note, however, that the estimation of the RR model parameters is not the scope of this paper. For more detailed discussions of the case study results, the reader is referred to Sikorska et al., 2012."

2. We added the following sentences in l. 23-26 p. 15 in RM: "This may be explained by the fact that, as in every event-based model, only the direct runoff which occurs during the first phase is modelled while a slower runoff due to the catchment retention is omitted. Also other discharges that are drained by the canalization network are not modelled."

3. We added: "i.e. in natural catchments or when the streamflow is not disturbed due to external factors (e.g. hydraulic infrastructure)." into the sentence: "As our case shows, simple models can predict flood events satisfactorily as long as the rainfall-runoff in

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the catchment follows conventional rainfall-runoff processes." in l.22-23 p. 16 in RM.
4. We added the following paragraph in l. 29-31 p.16 and l. 1-2 p.17 in RM: "It must be also stressed that the applied RR model, as an event-based model, is limited to model only the rainfall-runoff process within the catchment while omitting other water balance components as groundwater, base flow and evapotranspiration. Such simplified lumped models are especially useful when only the output of the catchment, but not the processes within the catchment, is of interest."
5. We added the following paragraph in l. 7-15 p. 18 in RM: "In this regard, flood predictions and the uncertainty due to model parameters could generally benefit from gathering more calibration data. However, under changing conditions of the catchment (e.g. urbanization), its characteristics 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 catchment response under (unknown) changed conditions but only makes predictions for the given situation (Blöschl and Montanari, 2010). If one is interested in modelling the catchment behaviour for different conditions, the parameters have to be modified accordingly."
6. In the sentence "However, such uncertainty contributions are strongly case-related and greatly depend on the available monitoring data." in l. 28 p. 19 in RM we added "[. . .] chosen submodel structures, and catchment and cross-section properties".
7. In the Table 2 p. 27 in RM we corrected the unit of the ERL1 into cm.
8. In l. 13 p. 8 in RM we added the reference Beck (1991).
9. In l. 14 p. 9 in RM we added the reference Wang and Robertson (2011).

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