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Interactive comment on "From runoff to rainfall: inverse rainfall–runoff modelling in a high temporal resolution" *by* M. Herrnegger et al.

M. Herrnegger et al.

mathew.herrnegger@boku.ac.at

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General remarks

We thank the anonymous referees and the other contributors for the mostly constructive and very helpful comments and the highlighting of issues, which need to be clarified and discussed in more detail. Judging from the comprehensive interactions and comments, the topic of the manuscript is obviously of interest. We therefore hope that this also comprehensive answer and suggestions of changes can clarify the issues.

Our motivation for the inverse model presented in the manuscript comes from practical hydrological problems we encountered. Some years back we set up rainfall-runoff

models for different alpine rivers (e.g. Stanzel et al., 2008; Nachtnebel et al., 2009a; 2009b; 2010a or 2010b). In the course of these projects, we were confronted with massive errors in the precipitation input fields. This is a known problem, especially in alpine environments. Although the temporal dynamics were captured quite well, we observed significant mass balance errors comparing observed and simulated runoff. This was the case not only for single events, but also for longer periods. The main reasons (measurement, sample and interpolation errors) for these input problems are discussed in the manuscript (We excluded, that erroneous evapotranspiration calculation were the reason (Herrnegger et al., 2012)). In the HYDROCAST project (Bica et al., 2011; Herrnegger et al., 2010) we tested different precipitation interpolation and parameterisation schemes by using the ensemble of generated inputs for driving a rainfall-runoff model and comparing the simulated runoff time series with observations. In essence, the results showed, that no significant improvements could be made and that the information on the precipitation fields is strongly determined and limited by the available station time series. The only additional information available concerning the precipitation of a catchment is the runoff observation. The main aim of the manuscript is therefore to present a proof-of-concept for the inversion of a conceptual rainfall runoff model. That is to show, that it is possible to use a widely applied model concept to calculate mean areal rainfall from runoff observations. We however see that changes and improvements are necessary to show our point. In a first step we will highlight and respond to some general concerns, which were mentioned by more than one referee and which we think are fundamental. The answers to the comments of the reviewers follow this section.

General authors statements

(1) Limited scope of the catchment data set

The inverse rainfall results depend on the ability of the forward model to capture rainfall responses of the catchment leading to runoff. If this is not the case, also the calculation of rainfall from runoff will fail. This is indirectly stated in the manuscript (P13271, L21-36), but will be highlighted as a limitation of the inverse model in a revised version. Although HBV-type conceptual models can be applied to catchments with a wide range of runoff characteristics (P13263, L27-30), it is clear, that catchments exist, where the application of this particular model structure will fail (e.g. flatland catchments dominated by groundwater). If hydro-meteorological conditions of the catchment change or are different from the calibration period and the forward model (e.g. due to poor parameter estimation, inadequate model structure, wrong representation of the real world prototype etc.) is not able to capture these changes, then again the calculation of rainfall from runoff will fail (as they do for the forward case).

However, integrating an additional catchment in the analysis, as noted by the referees, will improve the validity of the presented concept. Additionally, we propose a more detailed presentation of the validation results, as noted by referee #3. We further propose to add an experimental setup, in which we only use 2 years of data to calibrate the forward model and to evaluate the inverse rainfall for the remaining 2 years of data, in order to see the influence of the shorter calibration period.

(2) Inversibility of the model

To calculate the inverse rainfall rate the forward model is embedded in the Van Wijngaarden-Dekker-Brent search algorithm (P13266, L15-21), to find, hour by hour, the rainfall rate that best fits the observed runoff. In every time step the forward state space formulated model is therefore applied. For every time step, an inverse rainfall value, the resulting runoff and system states are calculated. The state space approach of the model is a first order Markov process: The system states S(t) and outputs O(t) of the calculation time step depend only on the preceding states S(t-1) and some inputs I(t) and not on the sequences of system states, that preceded it (e.g. S(t-2),S(t-3),..,S(t-

n) (see equation (2) and (3) in the manuscript). No hysteretic effects are considered. All information of the sequence of the preceding inputs (I(t-1), I(t-2),. . . ,I(t-n)) is implicitly included in the last relevant system state S(t-1). Given the model structure, parameters and potential ET as input, the inverse rainfall and resulting runoff are solely a function of the initial cold system states. Section 4.3.4 of the manuscript shows, that after an adequate spin-up time, all model runs, independent of the initial conditions, lead to the identical rainfall estimates.

Referee #2 states correctly, that the equations in the appendix include min/max operators, which, by introducing discontinuities, may lead to non-inversibility. Eq. A1 to A5 in the manuscript are partly too simplified representations of the model algorithm and need to be revised. Eq. A3 and A5 (concerning the linear reservoirs) do not include a threshold function in the actual model code. The differential equations of the linear reservoirs are solved analytically. An internal time step discretization is included in the code, to guarantee, that the transition between system states above and below the threshold value are solved exactly. A2, representing the soil layer, does include a min() operator for estimating the ratio between actual and potential evapotranspiration as a function of soil water content. This is however not a limiting factor for the inversion, since this factor is a function of the preceding soil state BW0(t-1), which is known. Only 50% of rainfall is used as input into the interception storage BWI. This is unfortunately not shown correctly in the manuscript. By assuming that the other 50% are always throughfall, equation A1 also does not limit the inversion, since a continuous signal through the whole model cascade is guaranteed.

The application limitations of the inverse model are stated in the manuscript (P13267, L1-14). It is, e.g. not possible to inversely calculate solid rainfall and the application is limited to catchment areas, which can still be reasonably represented by a lumped model setup.latin1 The inversibility is however not limited by the model structure. latin9

(3) Virtual experiments

We used observed rainfall input to generate runoff time series with the forward model. These time series were then used as input into the inverse model with the aim to calculate the observed rainfall. The comments of the referees (see also general statement (2)) highlight the skepticism, that this is possible in the first place. We refer to Herrnegger (2013) for the results, for a simple reason: A scatter plot, in which, e.g. the observed rainfall is plotted against the inverse rainfall from the virtual experiments and in which all points are on the 1:1 line and R^2=1, is not relevant to be shown. The manuscript however contains a detailed description of the virtual experiments (see 2.3; Figure 4), which shows the comprehensive setup of virtual experiments performed.

(4) Model structure and parameterisation uncertainty

The presented method heavily relies on a single rainfall-runoff model, as stated by referee #2. This is correct. Adding an additional model structure to the analysis would be interesting, but is clearly beyond the scope of this manuscript.

Referee #3 speaks of "rampant equifinality" for a 12 parameter model, since the parameters space is defined by a hypercube with 4096 corners. For our model 10 parameter where optimized, 2 parameter values where assigned a priori (1024 corners). Equifinality is not a problem, since different parameter combinations, which result in the same runoff, will also result in the same inverse rainfall results. Compared to the number of parameters that are calibrated in very widely used distributed hydrological models the term "rampant" in the context of a 10-dimensional parameter space is somehow a crude remark. Setting up a distributed 10-parameter model with e.g. 1x1 km grid-spacing for the approximately 40 km² large Krems catchments would result in a parameter space defined by more than astronomical 10^{120}(!) corners. This is "rampant" compared to 10^3 in our setup. This fact would however not stop hydrologists to apply a distributed model in catchment modeling.

In the manuscript, inverse rainfall results based on 2 different parameter sets are presented (see section 4.3.1 and 4.3.3). The differences in the results are however not discussed in detail. In the conclusions, we highlight, that the influences of model parameters must be analysed systematically in a further step. These are generally interesting topics of research but are not in the scope of this manuscript and must be dealt with in future research.

Responses to the single referee comments

Anonymous Referee #1

The article proposes a method to inverse the rainfall-runoff relationship at the catchment scale to estimate precipitation from runoff. This is an interesting study and the approach may be useful in areas where the estimation of precipitation is difficult, e.g. due to sparse none*raingauge networks. I think the article could make a valuable contribution to HESS. However, I have a number of concerns: - the general organization of the paper could be improved, especially the results section - some explanations and justifications are sometimes too short to fully understand the choices made by the authors in the study,*

See general authors statements (1)-(4) above and answers to the following detailed comments.

*- the validation of the approach should be strengthened, using other test catchments,*latin1 latin9

See general authors statement (1), where we propose to add an additional test catchment.

- there are too many illustrations (24 in total), not all of them appear necessary.

We are aware of this and intend to reduce the number of figures, also summarizing some results in tables as proposed by L. Brocca in the comments.

Given these limitations (and other aspects detailed below), I think major revision is needed before the article can be reconsidered for publication.

Detailed comments:

1. P13263,L6: The hypothesis of a closed catchment is quite strong, since there are many catchments where underground water exchanges with deep aquifers or surrounding basins are significant. One implication is that this underground exchange term is neglected later in the study (e.g. in the water balance equation). Does it mean that the approach would not be suitable for basins where there are such underground water losses/gains? This should be further discussed somewhere in the article as a possible limitation of the proposed approach.

It does not make sense to apply the inverse model to leaky catchments or catchments, where a significant part of the runoff is not observed at the gauging site. Even with a given quantification of the leakage process, the application of a hydrological model would lead to an additional uncertainty difficult to quantify. This is not necessarily a limitation of the inverse model. Also the application of a forward hydrological model, which needs to be calibrated against runoff observations, will fail or will result in wrong estimates of water balances or wrong evapotranspiration.

2. P13263,L24-26: I found this sentence unclear. Could the authors further explain why this approach has limitations that justify the introduction of the new method they purpose?

Kirchner (2009) presented a method to infer catchment rainfall from streamflow fluctuations. The approach is (as stated by Kirchner) limited to catchments, where discharge is determined by the volume of water in storage and which can be characterized as simple first-order nonlinear dynamical systems. The Kirchner (2009) model (when deriving the storage-discharge relationship directly

from runoff data) only has a single parameter. In contrast the presented model uses 10 parameters, which offer more degrees of freedom and flexibility in describing more complex catchment responses (P13263, L27-30).

3. P13265,L19: The equation may give the impression that time steps are considered independently in the method. However, S(t-1) is a function of antecedent rainfalls R(t-1), R(t-2), etc. Maybe this should be stated more clearly.

See general authors statement (2) for our detailed answer.

4. P13266,L6: How the upper bound of 50 mm/h was chosen. Is that a general value that can be applied everywhere or is it specific to the study catchment?

50 mm/h is an arbitrary (but reasonable) value. Any reasonable bounds can be applied. For the study catchment, this value corresponds to an hourly rainfall rate with a return period of about 3 to 50 years, depending on the tabulated value chosen (design rainfall events taken from http://ehyd.gv.at/). The spread in the return period highlights the uncertainties concerning rainfall.

5. P13266,L12-14: Could the authors detail this a bit more? Which disadvantages are they?

The model mentioned, but not used, does not include interception and routing. Additionally the inversion is limited due to threshold values, which cannot be resolved in the analytical inversion. The resulting inverse rainfall shows oscillations and instabilities. These facts are based on the findings of the virtual experiments, highlighting their importance (see also general authors statement (3)).

6. P13269,L3-9: I found the choice of the study catchment quite strange. The authors mention at the end of section 2.2 that the method cannot be applied in snow influenced catchment. . . and then they select a test catchment heavily influenced by snow! Con- sequently, the test of their method can only be done on a short part of the test period where there is no snow influence, which limits the depth of the evaluation of the proposed method (e.g. can it handle seasonal variations in precipitation?). I found this choice unfortunate. Besides, I found that testing the method on a single case study is also limited, since it does not give any information on the transposability of the method elsewhere. Therefore, I think it would be useful to have at least two study catchments with contrasted climate (and possibly hydrological) characteristics and without snow influence to provide a more comprehensive evaluation. If the authors have a specific interest in the Krems catchment, then it could be kept as an additional case study to show how the method can partly be applied in snow influenced catchments.

With the inverse model, it is not possible to calculate solid rainfall. The application to calculate rainfall is therefore only possible in snow-free periods. Consequently only summer months, when no snow melt influences runoff, are simulated. In rainless periods, where it is clear, that snow melt is dominating runoff (e.g. in spring), the inverse model can however be used to quantify the snow melt contribution. This is however outside the scope of this manuscript and may be addressed another time. Concerning the other issues addressed in the above comment we refer to the general authors statement (1).

7. P13270,L4-12: The authors should explain why such a short period is sufficient to test the efficiency of the method, given the known variability of hydroclimatic conditions.

See general authors statement (1) as an answer to this comment.

8. Section 4: I found this section not well organized. It mixes the presentation of testing methods, criteria and results. I think that all methodological aspects should be presented before the results section, to provide a clearer overview of the testing approach, and then the results section should only detail and discuss the results.

In a revised manuscript, the presentation of the testing methods (e.g. first part of section 4.3.2. and 4.3.4) will be reorganised before the results section.

9. P13272,L11-13: I found this is not so clear for the year 2007.

That is correct and will be changed.

10. P13273,L17: R2 is known to be very sensitive to outlier data. Therefore is it really a well-chosen criterion? MSE is used later. Why these two criteria are necessary?

R2 is included, because it is the standard criteria to assess a model to capture the variance in observations. Additional criteria are presented, since they highlight different aspects of a comparison.

11. P13273,L19-22: How can this be interpreted? Can the low-pass filter role (smoothing effect) of the catchment be partly responsible for this?

Temporal integration leads to the elimination of noise in the inverse rainfall signal.

12. P13273,L27 to P13274,L8: I found this part not really essential.

We find this part essential, since the correlation coefficient is also a standard criteria frequently used and measures the models ability to reproduce time and

shape of observed values (Gupta et al., 2009).

13. P13275: What can be learnt from this?

It is not clear, to what part of P13275 the question refers to.

14. P13279,L29: In real time flood forecasting, the target is the future flow, not the past rainfall. So I did not understand why the application of the method would be useful for this application.

Two different applications of the inverse model in the context of real-time flood forecasting models are conceivable:

1. **A frequent problem observed in real-time flood forecasting models with state space formulations is that the system states in the models are biased in such a way that the simulated and observed runoff differ systematically. Methods exist to cope with this problem and to update the system states (e.g. Liu et al, 2012; McLaughlin, 2002). The system states in the inverse model will, at least during driven periods (see P13275, L8-16), always guarantee, that the simulated runoff is identical to observations. This fact may be used as a basis for updating system states in the flood forecasting models.**
2. **At least in Austria, 2 different types of precipitation forecasts are used as input in flood or runoff forecasting models: Nowcasting fields (used for forecasts of t=+1h to t=+6h) and fields from numerical weather forecasting models (used for t>+6h). The nowcasting fields strongly depend on the quality of station observations (t=0h), as they are the basis for extrapolation into the future (Haiden et al., 2011). By assimilating the inverse**

rainfall into the nowcasting system, it is conceivable that the rainfall estimates of t=0h can be improved. An extrapolation of the improved rainfall fields could therefore improve the nowcasting fields and in consequence the runoff forecasts.

There are however methodological issues to be solved, before an application is possible. These include the spatial disaggregation of the inverse rainfall and system states in case the flood forecasting models are setup as distributed models or the limitation of the inverse model, when used to calculate rainfall, to snow-free periods.

15. P13281,L10-11: I did not understand this sentence.

Percolation from the soil layer was initially calculated using a linear reservoir with a time-invariant parameter KBF. With the additional parameter PEX2, KBF becomes time-variant, calculated as a function of soil moisture.

16. Table 1: What is the source of these values? Any reference?

The values originate from Sevruk (1981) and the other references mentioned in the text. We will add these references to the table.

17. Table 3: Should fluxes not be expressed as depth per time step?

The fluxes represent sums over the time-step. This information will be added.

18. Figure 1: BWI should appear in the first store

We agree.

19. Figure 2: This is quite classical. Is it really useful here?

We agree, that it is quite classical. We however think, that it may help to understand the inversion procedure (see general authors comment (2)).

20. Figures 4 and 5: Could these two figures not be merged?

This is in our opinion not feasible because they are too complex to be merged.

21. Figure 7. Remind in the caption that only a few months are used each year.

Will be added.

22. Figure 8: It is always nice to have hydrographs shown, but I found the added value of this figure is rather limited.

We will reformat the figure, e.g. using log-scale axis.

23. Figure A1: Is this figure useful, given it is not really commented in the text?

We agree, that the information content is limited. The figure will be removed.

Anonymous Referee #2

General comments:

The paper by Herrnegger et al. presents a method to derive estimates of rainfall from runoff and potential evapotranspiration by inversing a rainfall-runoff model. The paper is clearly written and well presented, it deals with a very interesting topic and the authors have covered several important issues related to the method presented including the impact of model initialisation, model calibration and validation. However, we believe that the method requires significant improvements before the paper can be accepted for publication. Three points require the author's attention:

- *Inversibility of the state-space equation: the method presented by the author is based on the inversion of the state-space equation presented in Equation 4. The authors have not fully explored the fact that the relationship may not be inversible in certain conditions. They have mentioned inversibility problems related to snow pack and distributed modelling in section 2.2.1. However, we believe that this problem is far more common that suggested by the authors, due to the two following issues:*

  - *Thresholds: many rainfall-runoff model structures use threshold functions to process input data. For example the COSERO model relies on three min/max operators in equation A1, A2, A3 and A5. These functions introduce discontinuities in the relationship between rainfall and runoff preventing an inversion algorithm to be applied. A simple example can be given with the interception reservoir of the COSERO model: in equation A1, $BWIt = 0$ whatever value of rainfall Rt is chosen such as $Rt < ETPt - BWIt{-}1$, as a result the state space equation related to the particular state BWIt is not inversible for low rainfall values. This example illustrates the difficulty of inversing rainfall-runoff models during low rainfall periods or in high evapo-*

    ration catchments.

    See authors statement (2), where we deal with the inversibility of the model.

  - *Delayed responses: Equation 1 assumes that the runoff at time step t is a function of inputs during the same time step (i.e. Rt). However, many rainfall-runoff model structures (e.g. HBV with the MAXBAS parameter) introduce a lag effect between inputs and outputs. As a result, Equation 1 could be rewritten $Qt = f(Rt, Rt{-}1, Rt{-}2, ..., Rt{-}n, ETPt, St{-}1, ji)$. In this case, it is not clear how the inversion algorithm works because it has to deal with rainfall values at several time steps simultaneously.*

    The applied model does not include a lag parameter. See authors statement (2), were we explain, that the model outputs are solely a function of the preceding system states.

  Overall, we believe that the inversion method as presented by the authors is possible in many situations, but requires a clear definition of feasibility conditions. This point could be explored in the synthetic experiments. In addition, it is clear that inversion is impossible in certain conditions (e.g. dry spells, high evaporation catchments,...), it would be useful for the authors to provide more context on the intended use of the method to judge if these limitations pose a real challenge to be explored further.

  We refer to the authors statement (2) and (3) as an answer to the above comment.

- *Impact of rainfall-runoff model structure: The method presented by the authors heavily relies on a single rainfall-runoff model (COSERO). As a result, it is not* *possible to differentiate the impact of the inversion method itself from the one of the rainfall-runoff model. We suggest adding at least another rainfall-runoff model and check the link between the performance of the forward model and results obtained by the inversion method.*

Please see the first part of authors statement (4).

- *Limited scope of the catchment dataset: all results presented cover a single catchment of 38 km2 for a period of 3 years. It is extremely difficult to generalize this setup to other hydrological conditions and we urge the authors to consider a larger number of catchments with longer simulation periods. Problems like non-stationarity, poor data quality, prolonged spin-up periods and model biases need to be factored in the results for the method to become relevant to the hydroligical community. We are aware that getting hourly data is not simple for large catchment samples, but the study could be performed at the daily timestep with equally interesting outcome.*

  4 years of data are available (see section 3.3 of the manuscript). Please refer to the authors statement (1) for a detailed answer to the above comment.

Detailed comments are provided in the following section.

Specific comments

1. Page3 Line9, "Errors are considerably lower compared to rainfall": This is certainly true, however the authors process the streamflow data via a non-linear inverse model that could easily magnify streamflow errors by several order of magnitude. I suggest a comment on this point.

This is a good point, which we have not addressed. A possible approach to

evaluate the influence of streamflow errors would be to run the inverse model with runoff observations, which are changed systematically, e.g. by adding a constant offset. Adding this experimental setup to the already comprehensive manuscript would be outside the scope of this paper. We therefore propose to perform these analyses for a follow-up publication, in which we would also look at the parameter uncertainties (also see authors statement comment (4)).

2. Page6 Eq 5 : Please add the objective function that was used in the root-finding algorithm (e.g. squared error). I presume that equation 5 is essentially a stop criteria for the algorithm.

Equation 5 is the objective function used in the root-finding algorithm. This must obviously be stated more clearly in the text.

3. Page7 L17, "Reservoir without memory" : I don't understand this statement. Please clarify and give examples.

The statement is misleading and the paragraph will be changed.

4. Page7 L22, "small errors ... can be amplified" : This statement confirms that small errors in streamflow data can back propagate within the inverse model and heavily influence the rainfall estimates. We suggest adding an experiment testing this assumption.

Please refer to the answer to the first specific comment of referee #2.

5. Page13 L10, "model performance expressed by the correlation coefficient": Please add also bias of model simulations. Bias is sometimes difficult to re- produce, especially if the model is calibrated with NSE objective function.

Thank you for the suggestion. We will add the bias.

6. Page20 L4 : What is the effect of BWIt to the rest of the model? I can't see it mentioned in subsequent equations. Please clarify.

See authors statement (2), where we state that the equations in the manuscript must be revised.

7. Page20 L6 : Is it really PEX 2 or f (PEX 2) that should be mentioned in equation 2 with f the function displayed in Figure A1? Please clarify.

f(PEX2) is correct and will be changed.

8. Page26 Tab 2 : I presume that the upper and lower values of the TAB and TVS parameters vary with the time step. Otherwise, we could get a and b parameters greater than 1, which could lead to negative values of BWi. Please clarify.

The values of a and b vary with modelling time step and represent smoothing functions of the linear reservoirs. As mentioned before and in the authors statement (2) at the beginning, the equations in the appendix will be revised.

9. Page31 Figure 9 : Scatter plots are extremely misleading for flow data because of the high concentration of point in the lower left corner. Please change both axis to log scale to get a better distribution of values along the axis.

This can be done.

10. Page33 Figure 11: Same comment than #9

This can be done.

Anonymous Referee #3

General comments:

This manuscript claims that one can use a calibrated rainfall-runoff model to "predict" rainfall from runoff time series, by using a search algorithm to find, hour by hour, the rainfall rate that best fits the observed runoff.

We have not used the term "predict*" throughout the whole manuscript in the context of the inverse model, as suggested by the anonymous referee #3 (not only above, but also in the following sections).

This result is somewhat surprising, because mathematically speaking one would expect the inversion of a multi-compartment model to be ill-posed (because different rainfall inputs at different times, and different combinations of storage levels in the different compartments, should lead to the same discharge), and possibly also mathematically unstable. In that respect the results claimed here are intriguing.

Please refer to authors statement (2) for a detailed answer.

However, the manuscript does not make a convincing case for its central claims, in several important respects.

*1. First of all, the manuscript claims to "predict" rainfall from runoff, but it can only do this at sites where it already has extensive rainfall data to calibrate the model with. This leaves the reader wondering: how can we "predict' a rainfall time series if, in order to do so, we must already *have* the rainfall time series so that we can calibrate the model?*

It is actually the same situation when hydrologist forward-predict runoff. In order to do so we need rainfall and runoff time series to calibrate the model.

*2. The manuscript claims that this method will be useful for generating improved estimates of mean areal rainfall. The manuscript presents no convincing evidence to support this claim. Indeed, unless one already *has* good estimates of mean areal rainfall, one will be calibrating the model with incorrect rainfall data, with potentially serious consequences for the calibrated parameter values and the resulting "predictions" of rainfall from runoff.*

See above answer to referee #3.

3. This is a 12-parameter model. More than two decades ago, Jakeman and Hornberger (1993) showed that typical rainfall-runoff time series were unable to constrain the parameters of even a two-compartment, four-parameter hydrologic model. A 12-dimensional parameter space is absolutely vast compared to a four-dimensional parameter space (as a simple illustration, a 4-dimensional cube has 16 corners, but a 12-dimensional cube has 4096 corners). Rampant equifinality is virtually guaranteed in such a model. The manuscript gives no indication of any awareness of the equifinality problem, and no indication that anything has been done to deal with it, or even to assess how serious it is.

See authors statement (4) for a detailed answer.

4. The only validation that is presented here consists of calibrating the model for 2006, 2007, and 2008, and then running it without calibration in 2009. It should be recognized that this is an extremely weak test, because the model is being tested against one year that is nearly identical to the three other years that it was already calibrated with (at least I believe this to be the case, but the figures present almost no information about the validation data, whereas the calibration data are presented in much greater detail). This problem was already pointed out over a decade ago by Seibert (Nordic

Hydrology 34, 477-492, 2003) in the context of rainfall-runoff modeling. Seibert observed that models typically give good fits to calibration data, and also to "validation" data that is virtually the same as the calibration data, but then fail spectacularly whenever they are tested against data that are somewhat outside the calibration conditions (that is, whenever the "predictions" are really predictions, rather than fits to existing data or functionally equivalent data sets). This problem becomes more acute as models become more parameterized; thus it is likely to be a particularly serious problem in the present case. The manuscript must demonstrate that the proposed approach can predict rainfall under conditions that are substantially different from the conditions the model has already been calibrated against.

See authors statement (1), to add a second catchment and to add a different experimental setup.

5. The abstract claims that "To verify the existence, uniqueness and stability of the inverse rainfall, numerical experiments with synthetic hydrographs as inputs into the inverse model are carried out successfully." Absolutely no evidence to support this statement is provided; the reader is referred instead to the first author's Ph.D. thesis. What was done, apparently, was to put precipitation numbers into the forward model to generate streamflow, then to put exactly those streamflow numbers into the inverse model to generate precipitation again. This does not demonstrate stability, at least in any sense that really matters. It does not test, for example, whether small errors in the streamflow numbers will generate big errors in the inferred precipitation (which would seem to be likely, particularly at short time scales). It also does not test the even more important question of how structural errors in the model (which must exist, because no model is an exact copy of the real hydrological system) will affect the accuracy of the rainfall "predictions".

Please refer to the authors statement (3).

5. Figures 10 and 16 show impressive-looking cumulative rainfall curves, which appear to show very close correspondence between the model results and two data sets. It is important to recognize that this proves nothing except that the model conserves mass, and thus that the amount of rainfall that is predicted to go into the model is consistent with the runoff that is measured going out of it (plus evapotranspiration, about which surprisingly little is said).

Yes, the model conserves mass (luckily). The mean catchment rainfall in the summer months is around 564 mm, actual evapotranspiration is 263 mm. Almost 50 % of precipitation is lost to evapotranspiration, highlighting the importance of this term of the water balance. If the model would not capture actual ET correctly, the cumulative rainfall curves would look completely different. Therefore, there is more behind the cumulative curves than just mass conservation, as suggested by referee #3. Actual ET from the model results from the interplay and temporal dynamics of the system states of the different parts of the model.

6. At a shorter time scale, the same effect is likely to be behind the apparent increase in goodness-of-fit, shown in Figure 11, as rainfall estimates are averaged over longer intervals, from 1 hour to 6 hours to 24 hours. Because the model must conserve mass, the integrated inputs and outputs must be equal, over timescales than the storm response timescale (that is, the timescale that changes in storage can allow inputs and outputs to diverge substantially). Thus as the averaging interval becomes longer, the inputs and outputs must match more closely.

As stated above, the mechanisms leading to a “correct” mass balance are more complex than suggested by referee #3.

7. In this respect, it is interesting to compare the results presented in Figure 11 with the *much simpler method of Kirchner (2009). In the current manuscript, the inverse model predicts one-hour precipitation with an r-squared of 0.24, whereas Table 4 of Kirchner (2009) shows r-squared values of 0.66 (r=0.81) and 0.77 (r=0.88) for one-hour rainfall predictions at his two sites. Most importantly, in that approach, predictions are really predictions, because they are not calibrated against any precipitation measurements.*

The correlations presented in Kirchner (2009) are impressive. The r-squared of 0.24 between observed and inverse rainfall must however also be seen in comparison with the r-squared between observed and INCA rainfall: The r-squared in this case is 0.18 and even lower, although INCA is based on rainfall observations. The Severn and Wye river catchments differ substantially from the Krems catchment concerning hydrological characteristics, land use, geology etc. The aim of this manuscript is not a model comparison. It would however be of interest to apply the Kirchner model to the Krems catchment. To be fair it must however be mentioned, that for estimating the sensitivity function g(Q), the Kirchner approach also depends on rainfall time series for identifying rainless periods.

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