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Hydrology and Earth System Sciences Discussions

Interactive comment on "Data assimilation in integrated hydrological modelling in the presence of observation bias" *by* J. Rasmussen et al.

J. Rasmussen et al.

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Thank you for your comments. We have here tried to answer these point-by-point.

-Comment: The first issue is that, despite the authors' claim that they "discuss the challenges associated with coupling two processes (groundwater and stream flow) in a single filter", the discussion is strongly biased towards the performance of the filters, little attempt is made to relate the results to the physical processes occur-ring within the catchment, and model results are never showed in terms of stream discharge. The scarcity of information given about the model itself does not help in this respect. I understand the model is not new and has been used in many studies before, but I think a more detailed description is warranted, especially concerning the coupling between

the various hydrological components.

Reply: We agree that the wording cited in the comment is not representative of the paper's content and focus. However, the focus on the filter performance rather than the specific physical processes modelled in this study, which is reflected in the content and structure of the paper, is intended. The previous paper, Rasmussen et al. (2015), has a broader focus in which the two processes are evaluated separately and compared.

Changes: The sentences cited have been replaced by the following: "We discuss the challenges associated with observational bias in hydrological data assimilation for both state updating and parameter estimation. Two existing methods of estimating observation bias, the separate bias Kalman Filter and the augmented state vector approach, are tested and the results compared. The novelty of the study lies in the focus on data assimilation bias estimation in a complex, integrated hydrological model as well as the impact of bias on parameter estimation using both synthetic tests and real world observations." Furthermore, more information has been added on the model coupling in section 2.2.2.

-Comment: Another main issue of this paper is that it refers extensively to another (companion?) paper from the same authors, also submitted (and very recently published) to HESS. As far as I understand, the only difference between this paper and the other is that here bias correction is considered, while all the remaining contents of the present manuscript (methods, model, study area) have been already included in the previous paper. In other words, the results of this study provide only a marginal contribution to the literature. It is not for me to decide whether the contents of this manuscript alone warrant publication in HESS, but I honestly think that the results presented here could (and should) have been included in the previously published paper as additional sections.

Reply: It is true that the model, study area and part of the method are similar to that of the previous paper, which is why it is referenced so extensively. The added information of this paper is indeed bias estimation, but also the assimilation of real observations, which is still not common at this scale – particularly in conjunction with bias estimation. As such, we believe that the new findings presented in this paper can stand alone and that the findings are novel and thus warrant a separate paper. However, as stated in the comment above, we agree that the objective of the paper, as described in the introduction, does not convey this, and it has therefore been changed.

Changes: Same as in the above comment.

-Comment: Page 8136: most of Section 2.2.2 is a model description, not setup. I suggest merging it with Section 2.1.

Changes: The section has been merged into section 2.1.

-Comment: Also, more details about the model are needed, especially as regards the coupling. E.g., what are the parameters "drain level" and "drain time constant" and what is their physical meaning? Are they relevant for the coupling of surface and subsurface flow?

Changes: A description of the parameters has been added to section 2.2.3 (Model parameterization): "The drain level and drain time constant parameters control the amount of groundwater drained to the nearest stream once the groundwater table exceeds the drain level, and are as such linking the groundwater module and the stream flow module of the model. The drain simulation represents the subsurface tile drain systems installed under most farmlands as well as the natural lateral flow processes that often occur in the topsoil. The leakage coefficient is another coupling parameter, which represents the hydraulic properties of the thin layer of the sediments at the bottom of the stream. This parameter is of particular importance with regard to river base flow.". Also, the following has been added regarding the coupling between the processes: "The stream network model is set up using an alternating calculation scheme in which discharge and water level is calculated respectively in alternating points, and is independent from the groundwater model in discretization, but exchanges of water

between the two processes is made available in model grids of the two processes that physically overlap." .

-Comment: Regarding the setup, what are the initial and boundary conditions used in this study?

Changes: The following is added to section 2.1 (Model): "The groundwater model initial conditions is based on an extended warm-up of the model, in which a quasi-steady state develops, while the stream flow initial conditions was calculated assuming a steady state condition."

-Comment: Page 8137, lines 5-7: as far as I know, even a standard EnKF does not require the full covariance matrix, as the product HP(PH)T can be assembled directly. Please rephrase the sentence.

Changes: The sentence has been changed to: "It is computationally more efficient than the EnKF and is furthermore deterministic, meaning that the observations in the filter are not perturbed, which reduces the impacts introduced by sampling uncertainty".

-Comment: Page 8141: Eqs (12), (13), and (14) can be merged into a single equation. Changes: The equations have been merged into a single equation.

-Comment: Page 8142, lines 9-13: in my experience, discharge observations in natural rivers can be as biased (if not more) than groundwater head observations, due to the need of a rating curve that is often accurate only for low flow rates and extrapolated for high flow rates. This statement should be relaxed, or at least appropriate references should be provided to justify it.

Reply: We agree that bias on discharge observations can be as significant as groundwater head bias and that the sentence should be relaxed.

Changes: The sentence has been changed to: "This study assumes no bias in discharge observations, meaning that the only biased observations are the groundwater head observations. In real world observations, discharge observations would usually also be biased, but this bias is not considered in this paper in order to simplify the problem."

-Comment: Page 8142, lines 14-18: this is not clear. Either the initial bias is zero in all the locations or it is generated from a distribution with 0.6 m standard deviation and 0 mean. Please clarify. Reply: We agree that the sentence is unclear. Changes: The sentence has been changed to "In this study, the initial estimate of bias in all observation points is generated by sampling from a normal distribution with a standard deviation of 0.6 m and a mean of 0."

-Comment: Page 8148, line 16: I can see seven scenarios in Table 1, not five.

Changes: Corrected.

-Comment: Page 8152, lines 2-5: this explanation for the reduction of NS coefficient when passing from an ensemble size of 50 to 100 and increase when passing from 100 to 200 is not convincing. Definitely more details are needed here to explain the model behavior. For instance, I suggest adding to Figure 3 (in another panel) some comparison between the true discharge and the discharge in the assimilation scenarios. Also, why don't you show in Figure 3 the results of the other scenarios (SepFil, SepFil NoQEst, and NoBias)? Finally, the open loop results (simulations without data assimilation) should be added as well, to evaluate the real benefits of data assimilation in this series of simulations.

Reply: The same issue with stream discharge (i.e. the peaks caused by spurious correlation) was observed and discussed repeatedly in Rasmussen (2015). The peaks in stream discharge from spurious correlation dominate the performance indicator, which makes comparison between scenarios difficult due to the random nature of spurious correlations. We do however agree that this point is not sufficiently well described in the paper.

Changes: The following is inserted into the section: "As shown by Rasmussen et al.

(2015), these spurious correlations are likely to result in increased drainage to the stream model, resulting in large errors in stream flow. The errors from spurious correlations in the stream flow model dominate the performance indicator and are, due to the nature of spurious correlation, random. [As a result, the Nash-Sutcliffe coefficient is reduced when using an ensemble size of 100, but increased with an ensemble size of 200.]"

-Comment: Page 8152, line 26: the reference to Figure 5 is given before any reference to Figure 4.

Changes: The sequence of figures has been corrected (i.e. figures 4 and 5 have been switched).

-Comment: Also, I would expect more detailed comments about Fig. 5 other than "little drifting behavior is observed in the model".

Reply: The figure is referenced for the drifting behavior for each scenario (three) and finally referenced, described and discussed in depth in the section in which the scenarios are compared.

Changes: The following is added to the discussion of the figure: "Figure 5 shows that the drifting behavior is generally most pronounced in the NoBiasEst scenario and least pronounced in the ColFilEns200 scenario, with the drifting behavior of the SepFil scenario falling in between these two scenarios."

-Comment: Page 8153, lines 4-7: I don't see many differences in the drain level bias between SepFil and SepFil NoQEst, only in the drain time constant. Also, if these parameters are so important, they must be defined and discussed in more detail in the model description.

Reply: Correct; the bias in estimated drain level is not significant.

Changes: The sentence has been changed to: "The reduction in NS is explained by a bias in the estimated drain constant (Figure 4) and by a poorer description of the groundwater head level as indicated by the head RMSE". Furthermore the parameters have been discussed in more detail (se previous comment)

-Comment: Page 8153, line 26 to page 8154, line 5: is this comment based on results showed in some figures?

Reply: The statement is based on experience with the model and the data assimilation framework. But we agree that the sentence does not convey this, and should be changed.

Changes: The sentence is changed to: "It is observed that updating the groundwater head to a biased observation level causes the head to return to an unbiased level when model propagation is resumed (i.e. it is drifting as seen in Figure 5). The model behavior becomes unnatural in the sense that it is not controlled primarily by the input forcings, but rather by the model trying to retain equilibrium. This can result in deteriorated estimation of parameters and updates of model states not only in the observation points but in the entire model domain."

-Comment: Page 8155, Section 4.2: is the "base" model an open loop simulation? Please clarify.

Reply: The base model is described in section 3.1.1 (Synthetic test observations) as a deterministic model. However, we agree that this needs clarification.

Changes: Added to section 3.1.1: "Note that both the true model and the base model are deterministic models, that is, single, propagated models without any noise added."

-Comment: Also, as for the synthetic tests, I suggest adding and discussing a figure showing the model results in terms of stream discharge. In my opinion, as the subject is an integrated hydrological model, it is important to investigate the model behavior with respect to all its hydrological components.

Reply: As the model is integrated, we agree that it is important to evaluate both groundwater head and stream discharge, which is why we use the stream discharge Nash-

Sutcliffe coefficient as one of the three indicators of performance. It is our belief that this indicator serves the purpose of evaluating the modelled stream flow, and that graphical representation of the stream flow would not add significant information to the paper.

Changes: None.

Technical corrections

-Comment: Page 8156, lines 5-12: this paragraph is repeated twice, please delete.

Changes: Deleted

-Comment: Figures 4 and 10: please add units to the parameters.

Changes: Units added.

-Comment: Figure 9: correct the caption. This figure does not refer to the synthetic tests.

Changes: Corrected.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 8131, 2015.