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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.

Anonymous Referee #3

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General comments

This paper evaluates two alternative modifications of the ensemble Kalman filter to account for observation bias when assimilating groundwater head and stream discharge in an integrated hydrological model. The data assimilation methods are tested for the same catchment in two test cases, one with synthetic observations and another with real measurements. The main conclusions of the study is that accounting for observation biases overall improves the performance of the model predictions, in terms of hydraulic head root mean square error and Nash-Sutcliffe efficiency of stream discharge predictions.

The paper is interesting and explores important aspects of data assimilation appli-

cations to integrated hydrological models. However, there are significant issues that should be addressed. 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 occurring 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.

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.

Specific comments

Page 8136: most of Section 2.2.2 is a model description, not setup. I suggest merging it with Section 2.1. 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? Regarding the setup, what are the initial and boundary conditions used in this study?

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.

Page 8141: Eqs (12), (13), and (14) can be merged into a single equation.

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.

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.

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

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.

Page 8152, line 26: the reference to Figure 5 is given before any reference to Figure 4. Also, I would expect more detailed comments about Fig. 5 other than "little drifting behavior is observed in the model".

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.

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

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

Technical corrections

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

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

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

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