

## ***Interactive comment on “Operational aspects of asynchronous filtering for hydrological forecasting” by O. Rakovec et al.***

### **Anonymous Referee #3**

Received and published: 5 May 2015

General evaluation:

This paper is interesting especially because it tests a methodology for data assimilation, the asynchronous Ensemble Kalman Filter, which was not tested yet in hydrology. The comparison of this method with EnKF warrants publication in my opinion, but one critical point needs to be resolved. It was unclear to me whether in the experiments with EnKF and AEnKF the same amount of observations was assimilated. It is logical that assimilating more data would give a better result. Can the authors clarify this and also stress this more in the paper.

There are other points which make that the assimilation of discharge data in this rainfall-runoff model shows significant deficiencies. These are the considered uncertainty

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sources in the experimental set-up, the magnitude of the observation error and the normality assumption for the observations, and considering a time lag for updating model states with help of discharge data. These limitations should be acknowledged in the paper in the abstract and the conclusions. I agree that the main new point of the paper is the comparison of AEnKF and EnKF, and that we can live with these limitations then, but they should be acknowledged.

Altogether I believe moderate revision is needed.

Main points:

Section2.1: We will see later that rainfall is assumed deterministic, whereas all uncertainty is attributed to soil moisture. A normal procedure would be to assume rainfall uncertain as it is the most uncertain component for predictions with rainfall-runoff simulations. In spite of what the authors say, in several studies uncertainty in rainfall is considered in these studies. I think that this assumption should be more critically evaluated in the manuscript and in the discussion and it would be good if the authors discuss its implications. It would be good that this decision is also directly visible in the abstract and conclusions, as it is important information for the experiment.

Page 3173, line 16-17: Why was inverse distance weighting used and not kriging? Maybe add a short statement.

Eq.10-12: It is unclear how discharge is treated here, this is not discussed. But it seems that although discharge is typically not normally distributed, this is neglected here. At least additional discussion would be important here.

Page 3180, Line 23: This is a simple error model. Why not uncertain precipitation and parameters? This decision warrants more discussion, as already indicated above.

Page 3180, Line 26: I suggest showing results from these calculations as for the moment the paper is not very large and the number of figures not too high. It is interesting to learn how many ensemble members are needed for which type of model. For exam-

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ple, it is typically found that for distributed models the number of ensemble members has to be larger, especially if parameter estimation is also involved.

Page 3180, Line 27: A discussion on the magnitude of the observation error is needed. Literature on observation errors for discharge measurements suggests in general a much higher measurement error.

Page 3182, Line 10-16: Sorry if I missed something, probably did not get it right. You compare EnKF and AEnKF, where AEnKF assimilates the current observation and ten observations for the past. Did you apply EnKF then for this time period at each time step when data became available? This would mean, if you applied AEnKF with  $W=10$ , did you apply EnKF with  $W=0$  eleven times for this period, so that both methods ingested the same amount of data. This is needed for a fair comparison, but it is not clear to me if this has been done. Please clarify.

Page 3185, Line 1-11: It is unclear how discharge is related to past soil moisture or upper zone storage states. If it is used to update current soil moisture the procedure is suboptimal I think as discharge will have a higher correlation with past UZ/SM-conditions. If the time lag is not considered some of the conclusion (i.e., updating soil moisture not important) might be related to this specific set-up. In this case, it would be good to add some relaxing statement in the discussion.

Page 3188, Line 2: “a rainfall-runoff model” instead of “hydrological model in operational settings”.

Figure 6. Extend caption to again mention the different scenarios that are displayed here.

Figure 7. Not so clear to me as there are a bit too many lines. Maybe you can find a better solution.

Editorial:

P3175, L 1: change to: “(..) as for the EnKF”.

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P3176, L9: change to: “(..) the model states of the ensemble member are updated as follows:”

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 3169, 2015.

**HESSD**

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