hess-2015-54: Authors' response (in *blue italics*) to comments by Anonymous Referee #3 (in black)

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.

Authors' response: We appreciate that Referee #3 finds that the presented analysis warrants publication. We discuss and answer her/his comments in detail below.

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.

Authors' response: The amount of observations being assimilated into the model depends on the magnitude of W. We tried to avoid statements that we did a comparison of EnKF and AEnKF, because we rather evaluated AEnKF for different lengths of W. We will state more clearly in the revised manuscript that the amount of information differs for different values of W.

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 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.

Authors' response: We will acknowledge these specific limitations (as further explained below) more clearly in the abstract and the conclusions.

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.

Authors' response: We agree with the reviewer that input uncertainty is a very important issue. In previous contributions we investigated the effect of more complex ways of perturbing the rainfall and its effect on forecast accuracy (see Rakovec et al., 2012a,b, Noh et al., 2014). The noise on the soil moisture used in this study more or less resulted in similar open loop simulations of the discharge as in the studies by Rakovec et al mentioned above. The focus of this manuscript is on the filter itself rather than on the effects of the applied noise. We will make this clearer in the revised manuscript.

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

Authors' response: We agree with the Referee that there are other ways of interpolating rain gauge observations, such as kriging, which might be more appropriate. However, evaluating the benefits of different rainfall interpolation techniques was beyond the scope of the study. We used a method used in operational practice as this study is also oriented towards operational benefits of AEnKF over EnKF.

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.

Authors' response: We don't 100% understand the remark made by the Referee. Most probably the reviewer feels that we should make a statement about the non-normality of the discharge pdf. If that is the case, we already mention on P 3172 L 15-18 that although "the Kalman-type of assimilation methods was developed for an idealized modeling framework with perfect linear problems with Gaussian statistics, it has been demonstrated to work well for a large number of different nonlinear dynamical models (Evensen, 2009)".

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.

Authors' response: As mentioned above, in previous contributions we investigated the effect of more complex ways of perturbing the rainfall and its effect on forecast accuracy (see Rakovec et al., 2012a,b, Noh et al., 2014). For this study it was important that the error model produced reasonable results in the open-loop and did not lead to any numerical instability. In the end perturbing the soil moisture has a similar effect to perturbing the precipitation.

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

Authors' response: We did not include a figure on the effect of the ensemble size, because really negligible differences were observed. Nevertheless, we will include a note that such a small ensemble size as presented in the manuscript would not be possible if *parameter estimation would be involved or if more complex error models would be employed. More detailed analysis is left for further research beyond the scope of this study.*

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.

Authors' response: We agree that the magnitude of the observation error could be even larger, but we followed the approach of Clark et al (2008) and Weerts and El Serafy, (2006). The latter explicitly state that a standard deviation of 0.1 represents a large error. However, if an even larger observation error would be employed, we expect marginal differences in the model performance among individual scenarios (in a relative sense). Of course the forecast performance would deteriorate for all, as the weight of the observations would become smaller.

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.

Authors' response: The amount of observations being assimilated into the model depends on the magnitude of W. We tried to avoid statements that we did a comparison of EnKF and AEnKF, because we rather evaluated AEnKF for different lengths of W. We will state more clearly in the revised manuscript that the amount of information differs for different values of W.

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.

Authors' response: This is done through the Kalman gain. When the correlation is lower the update will be smaller. AEnKF exploits the correlation between the present state and the state not only at the previous time step, but also further in the past. Therefore knowing the present state is sufficient to determine forecast. It may be possible to use the correlation between discharge at the present time and UZ/SM in the past for data assimilation, however, this is beyond the scope of this study. Nevertheless, we speculate that this will only be useful in a smoothing context (i.e. present discharge may bring information on UZ/SM in the past), not in a filtering context as in the present study.

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

Authors' response: We thank the Referee for suggestion and we will include this suggestion.

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

Authors' response: We will extend the caption of Figure 6 such that it becomes self-explanatory.

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

Authors' response: We agree that the current figure is rather small, which is due to limited margins of the HESSD layout. We will make sure that the Figure 7 is large enough in the final HESS layout, where more space for Figures is provided.

Editorial:

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

Authors' response: We thank the Referee for the suggestion and we will include this suggestion in the revised manuscript.

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

Authors' response: We will change this to "(..) the model states of the i^{th} ensemble member are updated as follows:".

References:

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