Dear Anonymous Referee #5

I would like to thank you very much for your helpful comments.

Comment 1: p. 1787, line 6: reference for use of optical imagery.

A reference will be added.

Comment 2: p. 1789, line 29: why doesn't the Hostache et al. approach seem to be a valid approach for operational situations?

The method of Hostache et al. (in press) is based on an offline re-calibration of model parameters. This approach was shown to give good results. However, the approach is very time consuming and difficult to apply in an operational (i.e. near real-time) context. We will modify the text accordingly.

Comment 3: p. 1791: What are the characteristics of the SAR observations and how do they relate to the synthetic observation generation (e.g. spatial and temporal resolution)? This is quite important as the synthetic experiment needs to be made as realistic as possible, in order to provide insights for a future real-data application.

In fact, there is almost no publication to be found on this subject. In one of the few studies, Schumann et al. (2008) show empirical probability distribution functions of remote sensing-derived water stage data. They show that for some cross sections the pdfs are Gaussian whereas for others they are non-gaussian and skewed. Andreadis et al. (2007) assume a Gaussian distribution of water stages in their preparatory study to the proposed SWOT mission.

Based on these results we decided to assume a Gaussian distribution (we don't know about any other more likely/realistic distribution function). We further believe, that the particle filter can easily adapt to any for of probability density functions.

Comment 4: p. 1795, lines 1-3: this sentence creates more questions than answers. How is the model structure transparent and optimized? I suggest re-wording r removing the sentence altogether.

We will remove this sentence as it is irrelevant.

Comment 5: Fig. 2: it might be worth showing the deterministic CLM simulation along with the generated ensemble.

We will add the deterministic model run to Fig. 2

Comment 5: *p. 1798, line 18: the standard deviation of 2 m is missing from the text.*

This will be added.

Comment 6: p. 1803, lines 17-20: the authors state "We advocate the use of a particle filter as part of the proposed assimilation scheme because it provides flexibility regarding the form of the probability densities of both model simulations and remote sensing observations", but this wasn't really shown in the results. If the authors do want to explore this, very interesting, question they could perform the same analysis using an ensemble Kalman filter for example.

We agree that we don't provide any data to back-up this statement. We will rephrase it. The weighting procedure that is part of the PF can be adapted to any form of (local) probability density function (Equation 1). We will explain the flexibility of the Particle Filter in this respect in more detail in the re-submitted manuscript. We are currently working on a manuscript that is dealing with a comparison between PF and EnKF. We believe that the PF has some advantages over the EnKF because the updates conserve the mass for each particle. The Kalman filter calculates an innovation (difference between observation and simulation), and maps this onto the state space. Consequently, the value of the state variables changes in the analysis (assimilation) step. Moreover, one of the assumptions of the Kalman filter is Gaussianity of the observation and model errors, which is frequently not met in practice. This can lead to a suboptimal functioning of the algorithm. In the particle filter, the assumption of Gaussianity is relaxed.

Comment 7: p. 1804, lines 8-11: "The error forecast model regresses the future error value against the current value"; I believe this is slightly misleading, because the error model essentially uses a constant value between update times from what I understand. Using this error model in an ungauged basin, could be necessary due to lack of alternatives, but it would likely be quite inaccurate.

Yes, you are right. We will not use the term "regression" here, as it is misleading. We propose assuming that the relative error term is constant. This is based on the assumption that the river basin is acting like a linear reservoir. We know that this approach has some shortcomings. We will clarify the limitation in the re-submitted manuscript.

Thank you very much for your helpful comments. I hope that I was able give you a satisfying answer to all your comments. Don't hesitate to contact me if you need any further clarification. I also would like to refer to our replies to the other reviewers for additional information and clarification.

Sincerely,

Patrick Matgen