Dear Prof. Moradkhani,

I would like to thank you very much for your comments and kind words. I am sure that your advice will help us to improve the manuscript and to move the discussion forward.

Comment 1: It appears that the authors have used the CLM as the hydrologic model to provide the hydrograph as the input to the hydraulic model. Given the great amount of complexity of such a land surface model that considers both energy and water balances in its conceptual framework, it is not clear to me why the authors have picked such a model knowing that they have set-up a synthetic case study. Minimal illustration was provided for the hydrologic model (CLM) used considering that it could be a main source of uncertainty water stage estimation. If the importance of the hydrologic model is minimal in the study why they have not chosen a simple conceptual model for this which could be sufficient for a proof of concept?

There is a rather pragmatic reason for choosing the CLM model. The results that are presented in this manuscript were obtained in the framework of project that is aiming at the joint assimilation of remote sensing-derived soil moisture and water stages in coupled hydrologic-hydraulic models. The CLM model has been selected as hydrological model as it will allow assimilating spatially distributed soil moisture. Regarding the water stage assimilation part, it is true that the study could be done with more simplistic conceptual hydrological models. The ensemble members would be generated in the same way. It is also worth mentioning that the two models are loosely coupled and that there is no feedback from the hydraulic model towards the hydrologic model. The hydrologic model can thus be replaced by any other model. However, depending on the hydrological model structure, the proposed inflow correction module may need to be changed.

Comment 2: Section 2.1 "Experimental Design", It is mentioned that the adopted experimental design depicted in figure 1, is similar to the presentation by Andreadis et al. (2007), however, it appears that Figure 1, is very similar to the schematic framework presented by Moradkhani (2008) in figure 2 of his paper. It would be more clarifying if the authors explain about this in the paper.

We will refer to Moradkhani (2008) and highlight the similarities and differences between the experimental designs.

Comment 3: Minimal and simplistic illustration has been provided for the data assimilation (Particle filtering) in section 2.2 presuming that this is the major portion of the current paper contribution. I think after reading the paper, still many ambiguities remain about the particle filter implementation for water stage estimation. Therefore, section 2.2 needs to be elaborated further and explained more clearly that a reader interested to the subject can duplicate the work without reading many other articles to understand this.

We got similar comments from other reviewers. Even though we believe that the scope of the paper was not to present an enhanced version of the particle filter and that more detailed information on this subject can be found in other articles, we will clarify the description of the particle filtering algorithm in order to avoid any ambiguity. All information that is needed to duplicate the experiment will be available in the re-submitted version of the manuscript.

Comment 4: Page 1794, L 5-13, it is not clear to me if the authors have applied the SIR particle filter or they just mention the features of SIR algorithm. Further illustration would clarify the issue.

We applied the SIR algorithm throughout the study. We will clarify this in the paper.

Comment 5: The procedure for generating a meaningful ensemble in hydrologic modeling and data assimilation as explained through equations 5-9 was also explained by Moradkhani et al., (2005). This is meant to provide more historical background on the subject.

Thanks for the reference. We were not aware of the fact that Moradkhani et al. (2005) applied the same procedure. We are glad to be able to provide more background on the subject.

Comment 6: Page 1796, L1-10, authors mention that they perturbed the forcings, parameters and the initial condition by adding a Gaussian random number to their deterministic values. Knowing that the forcing data (in particular the precipitation) to hydrologic models have the multiplicative nature, the forcings need to be perturbed lognormally instead, to be more realistic.

This is an interesting point that we did not pay attention to. In this study we perturbed the precipitation and evapo-transpiration rates by the means of a multiplicative factor with mean zero and standard deviation sigma. We realize that a perturbation with a lognormal distribution would have been preferable. Since the adopted approach allowed to satisfy the verification measures for the ensemble generation, we believe that it is not necessary to re-do all the experiments. The magnitude of the noise is as follows:
forcings: 1% of the nominal value.
parameters: 10% of the nominal values.
This variance values gave a satisfying ensemble spread (i.e. uncertainty of observed states, in this case the water stages stage levels).

Comment 7: Page 1797, L13-15, " ..., an artificial positive bias of 25% was introduced to the simulated upstream boundary discharge ... to simulate the bias that is inherent in most model realizations, even after calibration". I am not sure if this is a true statement that after calibration we observe positive bias in models. If there is such a huge bias in the calibrated model, most likely the model calibration is ineffective or the hydrologic model has structural deficiency.

We agree with you that in reality a bias of 25% is a very strong indicator of ineffective model calibration, poor input data and/or model structure deficiency. However, we believe that over any given time period, bias is practically unavoidable and inherent in most model realizations (but of course less than 25 %). By adding 25 % to the simulated discharge values, we enhance the difference between results obtained with and without assimilation. We made the same experiment with smaller amounts of bias and no bias at all. The conclusions remain the same. The only thing that changes is the readability of the plots that is of course better with a 25% bias. We will clarify this in the re-submitted version of the manuscript.

Many thanks 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