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Hydrology and Earth System Sciences Discussions

Interactive comment on "Assimilation of MODIS snow cover area data in a distributed hydrological model" *by* G. Thirel et al.

G. Thirel et al.

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We would like to thank both reviewers for their useful comments. Many remarks were made about adding justification and description of the methods used so we worked on improving these points.

During the correction of the manuscript according to the reviewers' comments, we found a major problem in the results. This is why it took us such a long time to produce this revised version of the manuscript. Indeed, we found out that the improvement on discharges seen on the particle filter experiments were not due, at least to its most part, to the particle filter itself. In fact the biggest part of the improvement was coming from the perturbations added to the precipitation, to the temperature and to the snow melts coefficient maps. We demonstrated this fact by running the model with the perturbed maps but removing the assimilation steps (i.e. the particles were just freely run, no resampling was realized in order to get them closer to observations). We observed that the scores computed on the average of the particles were very close to the ones shown in the first version of the article. It means that the way the particle filter was implemented was not efficient. It also means that the improvement was coming from the perturbations, not from the SCA assimilation. By consequence, we decided to add in the new version of the article the scores for the average of the particles run without the particle filter assimilation (or without the Ensemble Kalman Filter). In this way, we are able to really demonstrate what the actual impact of the assimilation is.

Second, we modified the way the particle filter assimilation was implemented. In the first version of the paper, the MODIS SCA was converted into SWE with a Snow Depletion Curve equation. However, this converted SWE was only usable if the SCA was lower than 0.8, due to the approximation of the reversed equation we made. This limited a lot the impact of the assimilation because the observed dataset was much reduced by this way of proceeding. This is why we decided to turn around this issue by converting the LISFLOOD model SWE into SCA (with the same SDC equation) and perform the assimilation on SCA. By proceeding like this, all the MODIS SCA values were usable, which lead us not to lose observed information. Then the observed and simulated SCA maps (summed over areas as defined in the paper) were compared and the particles duplicated / erased according to that. Finally, synthetic experiments were added to the paper. It permits to first show that the particle filter and EnKF work. Then real experiments permit to show that real observed MODIS SCA data can bring useful information.

Here we respond to the specific comments of the reviewer:

Review of Assimilation of MODIS snow cover area data in a distributed hydrological model by G. Thirel, P. Salamon, P. Burek, and M. Kalas I enjoyed reading this paper. Yet, the paper is excessively difficult to follow, and would tremendously benefit from more explanations, and detail, both in the mathematical development, data preprocessing and postprocessing, and model – data analysis. In principle, the authors consider an important topic in hydrologic modeling, however the entire work (from setup to findings) is very difficult to follow, and only reserved for a few experts, or those that have been closely involved in the work. Even then, many questions remain about the choice of the measurement error for various attributes. These are simply assigned values without discussing in too much detail why they are given certain values. For instance: 1. How reasonable is the use of random errors to precipitation, temperature and the snow melt coefficient?

RESPONSE: Of course an improved definition of errors is desirable. Unfortunately, almost never any quantification of the error in the observation is given when a hydrological variable is measured. Hence, we approximate our errors using values given in literature.

2. Why are only these error sources considered explicitly? Why not the equations used to model the state evolution?

RESPONSE: Structural model errors can be an important source of uncertainty. However, research is currently ongoing on how to quantify this type of error (see Salamon and Feyen, 2010). Hence, we did not consider this error source Reference: Salamon,P., Feyen,L., Disentangling uncertainties in distributed hydrological modeling using multiplicative error models and sequential data assimilation, Water Resources Research 46 (12), 2010.

3. Why are the scores calculated for the mean of the 20 or 50 members, or for 50 or 200 particles? What is the underlying reason to do it this way?

RESPONSE: We used the mean because it is the easiest way to capture he behavior of both methods and a classical way to do it.

4. What is the model error used to perturb the simulated SWE states? How was this

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error selected, and how reasonable is this selection.

RESPONSE: We do not perturb the simulated SWE states.

5. What is being estimated, and what is considered to be known?

RESPONSE: Clearly this work does not claim to consider all different error sources contributing to uncertainty in hydrologic modeling. This is a very complex task and to the authors knowledge quantifying all different sources of uncertainties in hydrologic modeling has not been achieved yet and is still a topic of ongoing research. Here, we consider only the major sources of error relevant for snow data assimilation.

6. What are the exact settings of the PF and EnKF? They use different algorithmic variables, and this has not been explained very well.

RESPONSE: The mathematical formulation of both methods has been explained briefly in the corresponding sections. However, in order to keep this article short, we do not explain both methods in detail as this has been done in a variety of previous papers which have been cited and can be consulted if further details on the mathematical description of the approaches are needed.

7. There is a plethora of model parameters that potentially needs to be adjusted in the face of the current analysis. Which parameters are being held fixed, which ones are being estimated? This is all very unclear, at least to me.

RESPONSE: Only the snow melt coefficient parameter of the model is perturbed. All the others remain fixed.

Given some of these very profound questions, I believe that the paper is not ready for detailed review yet. If the authors and other reviewers like to proceed with publication I would not object, but would predict that the paper will not receive much attention. It is simply too difficult to follow and ignores important questions to be answered regarding the setup of each method with respect to the measurement and model error, how the scores are being computed, the reasonableness of the initial assumptions of the various errors, and are the state evolution equations corrupted or not? The model is imperfect. I don't understand that the particle filter works ok, but the EnKF does not? I personally need more information, and more detail to support this main conclusion.

RESPONSE: We improved the explanations about the EnKF not improving discharges.

A few comments from the first few pages: Page 1331, Line 5 – 10: Misses important work by Martyn Clark et al. on snow data assimilation. This includes papers in Geophysical Research Letters in 2006, Journal of Hydrometeorology, 2006, Advances in Water Resources in 2006. Please study these papers and discuss them in the text.

RESPONSE: Page 1331, line 5-10 represented a few examples of data assimilation works with no discussion; it was not made for being exhaustive. The Clark's papers in Journal of Hydrometeorology and in Advances in Water Resources were already present in the manuscript later in the introduction. We added the third paper and described it. The discussion of the two other papers has also been improved.

Page 1334, Line 5: Bad practice to start sentence immediately with a mathematical variable. Better to write. The vector : : :

RESPONSE: We modified the sentence as suggested.

Page 1334, Line 8: where E_0(t) is its error at time t: Again poor writing: I assume that this vector of error terms is the error of y: Bu

RESPONSE: In fact E_0(t) is the estimated error made on the observations. The writing of this sentence has been improved to make it clearer.

Page 1334, Line 12 - General comment: I would suggest to use the writing Y(1:t), rather than Y(t). It is much easier to read, and understand that all the data is used.

RESPONSE: We modified the manuscript according to the comment of the reviewer.

Page 1334, Eq. (2): I appreciate being to the point, yet the authors have to realize that this development will be difficult to understand for many readers.

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RESPONSE: This way of presenting this equation is already used in the literature (see Salamon and Feyen, 2010 for example). P. Salamon and L. Feyen, Disentangling uncertainties in distributed hydrological modeling using multiplicative error models and sequential data assimilation, Water Resources Research, 46, 2010.

Page 1334, Line 17: Rather strange reference. But does not mean it is wrong or so. Just never seen a reference like this. Would say that EnKF provides an approximation of the standard KF but then without requiring analytical equations for the covariance propagation, etc..

RESPONSE: We added the Evensen (2003) reference in the first sentence and we rephrased the second one.

Page 1334, Line 21: The use of the work background. Is this common language, I am more used to forecasted state, and analyzed state (after updating)

RESPONSE: Even if "background state" can be found in the literature, we replaced it by "forecasted state".

Page 1335, Line 15: References should be in order

RESPONSE: The references have been put in order now.

Page 1335, Line 19 – 20: Need to connect the weights to the likelihood. Nothing is mentioned in text, so people that are not familiar with PF will not understand the various steps.

RESPONSE: We modified this part giving the link between weight and likelihood and explained what the likelihood represents: "The weight of a particle is obtained by normalizing the likelihood of this particle with the sum of all the likelihood of all the particles. The likelihood of each particle (a(n)(t)), which represents the probability of a particle given some parameters, was computed as follows"

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 1329, 2011.

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