

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

### **Anonymous Referee #2**

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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.

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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? 2. Why are only these error sources considered explicitly? Why not the equations used to model the state evolution? 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? 4. What is the model error used to perturb the simulated SWE states? How was this error selected, and how reasonable is this selection. 5. What is being estimated, and what is considered to be known? 6. What are the exact settings of the PF and EnKF? They use different algorithmic variables, and this has not been explained very well. 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.

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.

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.

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

Page 1334, Line 8: where  $\epsilon^0(t)$  is its error at time  $t$ . Again poor writing. I assume that this vector of error terms is the error of  $y$ . But

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.

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.

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..

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)

Page 1335, Line 15: References should be in order

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.

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

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