

Interactive comment on “Assimilation of MODIS snow cover area data in a distributed hydrological model” by 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

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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:

The study illustrates an application of two data assimilation techniques for updating snow state of a distributed hydrologic model (LISFLOOD). The main objective was to evaluate the efficiency of particle filter and Ensemble Kalman filter assimilation meth-

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ods for snowmelt runoff simulations. The methodology is tested for the Morava catchment. The results indicate that the particle filter method improved the discharge simulations. The MODIS data assimilation is very interesting and relevant topic, within the scope of HESS. So the study is interesting, however before being considered for publication, it is necessary to clarify and revise/improve several points: 1) Please compare in more detail how SCA was implemented/assimilated into hydrologic models in previous studies (Introduction section) and STATE MORE CLEARLY what is novel here (with respect to previous studies).

RESPONSE: The description of previous studies has been improved, especially regarding the descriptions of the direct insertion and data assimilation methods. The use of a mass conservative method, the particle filter, for satellite SCA data is what is novel with respect to previous studies. This is now described more clearly at the end of the introduction.

2) The methodology is not always clear and/or well justified: a) Please consider to extend the pure mathematical formulation of data assimilation with clear hydrological reasoning and adding more description what/where and why is updated within the model.

RESPONSE: What was updated (or not) has been explained in a better way for each case.

The conversion between SCA and SWE is interesting, but not justified. I would expect that this relationship (function) depends on different settings (e.g. climate region, initial SWE state, season), thus I'm wondering to what extent one functional relationship (as proposed here) is able to describe the spatiotemporal variability between SWE and SCA. It will be very interesting and important to show how it fits with real observations from the study region (or region with similar physiographic characteristics).

RESPONSE: We agree with the reviewer on the fact that the Snow Depletion Curve (SDC) is probably far from perfect. However, the focus of this paper is on the impact

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of SCA data assimilation techniques on discharges simulation. This is why we did not want to focus too much on the SDC. Zaitchik and Rodell (2009) used this equation on the whole Earth, this is why we did not think it was necessary to do a proper study on the availability of this equation for our area. The availability of in situ snow measurements would also be a limiting factor for us. We are however aware that improvements of this equation could lead to improvements of the efficiency of the assimilation in the future and that research on spatiotemporal variability is interesting.

b) Probably I missed something, but I do not understand the reasoning applied for creating ensemble members. It is very difficult (if even possible) to interpret the MODIS assimilation efficiency, if so many factors are combined together. Please also clarify what is expected by using such large intervals for precipitation and air temperature changes (what is the accuracy of model inputs-precipitation and air temperature- when such large changes are applied)?Is it MODIS that improved the streamflow simulations?

RESPONSE: The error ranges applied to create members are common error ranges described in the literature. Regarding the latter question, this is clarified in the new version of the manuscript, since the scores of perturbed particles without assimilation has been added. Where the improvement was coming from (assimilation or perturbations) was indeed unclear in the first version of the paper, as explained at the beginning of this response.

c) The next question is the frequency of updating. In my opinion, seven days are simply too coarse for expecting some real improvement in streamflow simulations. I understand that cloud coverage is an issue, but it is probably not necessary to remove all the clouds before SCA estimation. How will the assimilation efficiency change with the frequency of data assimilation?

RESPONSE: The reviewer is right to raise this interesting question. In the new version of the paper, data assimilation frequency has been tested for 7, 3, 2 and 1 day. This

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allows studying the behavior of the data assimilation in both synthetic and real cases.

3) It is important to add more hydrological interpretations of the results.

RESPONSE: The hydrological interpretations of the results have been extended.

Specific comments 1) p.3: SCA can be derived also from microwave products (where cloud coverage is not a problem), not only from optical sensors.

RESPONSE: The reviewer is right. Microwave products are also available (see AMSR-E or SSM/I) as well as radar data (RADARSAT or ASAR for example). However, AMSR-E and SSM/I have poor frequencies and resolutions, and radar data quality is not yet sufficient for applications like the SCA assimilation we present in this paper. This is why they were not good candidates for this work. The introduction has been completed with the mention of these additional types of SCA data.

2) pixel resolution of MODIS: why it is 420m? The standard product is either 500m or 0.05deg. Please clarify.

RESPONSE: The standard MODIS SCA product resolution is indeed 500m on a sinusoidal map projection. The 420m resolution was the resolution of the data after the process of projection on the LISFLOOD projection. We removed the 420m resolution from the paper in order to avoid confusion and we only kept the 500m original resolution.

3) the sequence (order) used for cloud removal is not justified.

RESPONSE: The cloud removal methods used in this paper have been described and justified in Parajka and Blöschl (2008) and Parajka et al. (2009). Using the snow-line method after the other cloud removal methods can easily be understood by the fact that this method would have difficulties to discriminate snow altitudes and no-snow altitudes when few pixels have data. References: J. Parajka and G. Blöschl, Spatio-temporal combination of MODIS images – potential for snow cover mapping, WRR, 44, 2008. J. Parajka, M. Pepe, A. Rampini, S. Rossi and G. Blöschl, A Regional snow-line method

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for estimating snow cover from MODIS during cloud cover, Journal of Hydrology, 381, 203-212, 2009.

4) conversion SWE-SCA. What is meant by “observed SCA values higher than 0.8 were not used : : :”? Does it mean that for cases when MODIS indicates e.g. full snow coverage and model no snow, no assimilation was applied?

RESPONSE: Yes, it was the case. This was due to the approximation made to invert the Snow-depletion curve (SDC) equation, which was incorrect for SCA values higher than 0.8. It was one of the limitations of the way the assimilation methods were implemented in the first version of the manuscript. For the reasons explained above, the SDC equation is not inverted anymore. It permits to avoid rejecting any available data, since for each observed SCA value we obtain a corresponding simulated SCA value.

5) The period used for analyses is rather unusual (starts on January, 10 and ends on December, 10). Why?

RESPONSE: This period represents in fact the period with available observed discharge data for the study area. The period could not be extended for this reason.

6) I would suggest to merge the sections 4.3 and 4.4 into the section 2, in order to clarify the background for data assimilation.

RESPONSE: The definition of the variable states has been moved in the description of the assimilation algorithms but we left the description of the perturbations in part 4.

7) Please specify in more details (e.g. by equation) the efficiency criteria of ratio-RMSE and ratio-bias.

RESPONSE: The equation of Ratio-RMSE has been added in the new version of the manuscript, together with the equation of the Nash criterion. The Ratio-Bias is not used anymore in the paper.

8) Please correct the reference to Zaitchik (year of publication is 2009).

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RESPONSE: We modified this reference.

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