Hydrol. Earth Syst. Sci. Discuss., 7, C2214–C2217, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C2214/2010/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Improving runoff prediction through the assimilation of the ASCAT soil moisture product" *by* L. Brocca et al.

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Received and published: 9 September 2010

We really appreciate that the reviewer agrees with the authors about the point that the assimilation of coarse resolution satellite soil moisture observations (retrieved from ASCAT) can be effectively used for hydrological applications.

Concerning the reviewer questions:

1. Some more details about the MISDc model would be helpful. A short paragraph about the model structure and the parameters that are used in the model would help to understand how the observations link with the model physics.

In the revised manuscript more details about the model structure and parameters will be included.

2. The authors shall explain the set up for perturbing the data for the synthetic experiments. Why is the multiplicative scaling error sampled from a log-normal distribution with SD of 0.4. What effects are simulated with such a perturbation, and what is the justification for using a log normal distribution and the SD of 0.4?

The question raised by the reviewer is of paramount importance. In fact, the selection of the probability distribution and, mainly, of its standard deviation is fairly arbitrary. In our study, we followed the indication of *Crow and Ryu (2009, HESS)* for the selection of the probability distribution even though different standard deviation values were used. We observed that low values of the standard deviation produced results very similar to the real case (without added errors). For high values, results were unrealistic because model performance become very poor. Therefore, a value between the two extreme conditions was selected for this study. This point will be clarified in the revised manuscript by using also different values for the standard deviation.

3. Is there a physical explanation why the satellite retrieval conforms best with the model for the NIC catchment. NIC has the largest coverage of forest (65%) which I assume makes the retrieval of soil moisture at C-band more uncertain.

Overall, the agreement between the model simulated soil moisture and the satellite derived SWI might be due, besides to the physical characteristics of the catchments, to the T value and the reliability of the input data (rainfall and temperature) used to obtain the simulated soil moisture temporal pattern. Indeed, for NIC catchment a quite high T value was estimated (T=60) and a dense and reliable hydrometeorological network has been operational for several years. This catchment was set up as "experimental catchment" to study runoff generation at different scales and its relation with soil moisture conditions. These two aspects might explain the best performance obtained for NIC catchment; however, further investigations involving a larger number of catchments and different study areas are needed for getting more general conclusions.

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4. On page 4127, line 9 the authors state that an improvement on total runoff estimation was expected. Why do the authors expect an improvement? The results of the assimilation could also be neutral or worse.

The sentence will be modified in the revised manuscript pointing out that the effects of the assimilation of the SWI index are more significant for the total runoff than the flood peak. Indeed, it is well known that the initial soil moisture conditions have a direct relation with runoff volume and not with the hydrograph shape which is influenced by other parameters.

5. In the synthetic experiment the authors degrade the model performance by simulating different noise sources. Consequently the authors should also modify the gain parameter G which to my understanding was not done. What is the affect of using a "wrong" G?

This is another good point raised by the reviewer. In the revised manuscript the effect of the G parameter value will be detailed also for synthetic experiments. Results corresponding to the ones already shown in Figure 3 will be added.

6. For the synthetic experiments strictly speaking a new SWI* has to be calculated. Specifically for the second (bias) experiment. Currently SWI* is scaled to the optimum model state (using the best available data). Not surprisingly this SWI* is very effective in correcting an artificially introduced bias. In practice this optimum model state will not be available and the SWI has always to be scaled to then imperfect (i.e. biased) model which will degrade the ability of the observations to correct for model errors.

We agree with the reviewer about the re-calculation of SWI* for the synthetic experiments. Strictly speaking also the rainfall-runoff model parameters should be recalibrated. However, in the study, we supposed that the parameters used for the SWI* computation were obtained by a previous analysis (in "non perturbed" conditions) and that the errors occur after the calibration period when the SWI* was assimilated. For the same reason, we do not optimize the G parameter value for the synthetic experi-

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ments and we keep the one estimated for the "real case". We would like to point out that the synthetic experiments aimed at analyzing the SWI* potential if rainfall errors may occur, for instance, for a temporary malfunctioning of the monitoring network or if the model parameterization is based on a calibration period not representative of the full range of possible conditions, e.g. dry period. In the revised manuscript we will better specify all these aspects.

7. In table 2 it would be helpful to highlight those cases where the improvement in performance numbers is statistically significant.

Further to the reviewer suggestion the statistical significance of the efficiency index and of the differences in the performances with and without assimilation will be estimated by the bootstrap method and shown in Table 2.

8. Fig 1 is difficult to read in b/w.

Accordingly, we will modify the figure.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 4113, 2010.