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.

Anonymous Referee #2

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The paper presents results from an assimilation experiment of coarse resolution satellite derived soil moisture observation in a rainfall runoff model. The paper makes a significant contribution to hydrologic sciences as it is the first attempt to assimilate coarse resolution satellite derived soil moisture into a hydrologic model reporting a positive impact. So far scientists have been very critical about the value of such observations for hydrologic small scale applications.

Generally, the study is carried out very carefully, the methods are scientifically sound and the discussion of the results is objective. The discussion of the selected methods and the interpretation of results could have been more detailed in some places.

My questions to the authors are: 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. 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? 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. 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. 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? 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. 7. In table 2 it would be helpful to highlight those cases where the improvement in performance numbers is statistically significant. 8. Fig 1 is difficult to read in b/w.