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Discussions

Interactive comment on "Improving operational flood ensemble prediction by the assimilation of satellite soil moisture: comparison between lumped and semi-distributed schemes" *by* C. Alvarez-Garreton et al.

Anonymous Referee #1

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Overview

The study investigates the assimilation of satellite soil moisture data into rainfall-runoff modelling with the purpose of improving streamflow prediction. Specifically, the comparison between a lumped and a semi-distributed version of the PDM model is carried out in terms of model performance with and without the assimilation of satellite soil moisture data.

General Comments

The paper investigates a very important topic related to the assimilation of satellite soil moisture data for improving flood prediction. Being highly interested to this topic, I quickly and carefully read the paper that I found well written and well structured. I fully agree with the authors that there is a strong "...need for further studies focusing on SM–DA for the purposes of improving streamflow prediction from rainfall-runoff models". Indeed, besides the satellite observation that is considered, the assimilation of soil moisture into rainfall-runoff modelling involves several critical aspects (e.g. model and observation error, rainfall-runoff model structure, data assimilation technique, ensemble generation . . .) that significantly affect the final result and, hence, needs to be addressed carefully.

This manuscript addresses some important new aspects related to: 1) the generation of the ensemble, 2) the characterization of the temporal variability of the observation error, 3) the evaluation of the ensemble reliability through the rank histograms, and 4) the spatial discretization of the rainfall-runoff model.

However, I have also some major comments that need clarification.

1) The most important aspect is related to the analysis of the results. Overall, the assimilation of satellite soil moisture data improves the discharge simulation with respect to the open-loop ensemble prediction, but NOT against the model run in validation, without the assimilation. For instance, for the semi-distributed scheme, the NS-value is equal to 0.77, 0.28, and -1.89 for N7, N1, and N3 catchment, respectively, in the evaluation period. The corresponding NS-values after the assimilation are 0.73, 0.18, and -2.47, always worse. The improvements highlighted in the paper are very much related to the significantly lower performance of the open-loop ensemble prediction (NS=0.53, -0.02, and -5.36). This point needs to be addressed, especially if the methodology is to be applied from operational purpose. Actually, in our analyses we didn't find this large deterioration of the model performance when the open-loop ensemble prediction

is considered. What are the reasons for that? Is it due to a bias of the open-loop ensemble prediction with respect to the model prediction (the bias is not reported in the paper)? Could it be due to the procedure adopted for producing unbiased ensemble dealing with the upper and lower soil moisture limits? I am well aware on the difficulties of obtaining a robust ensemble with the Ensemble Kalman Filter applied to rainfall-runoff modelling and to real cases (not synthetic). However, this represents a very important aspect that needs to be discussed in details. At least, it should be clarified in the paper that the assimilation deteriorates the model performance with respect to the model run in validation without the assimilation.

2) I believe that the PDM model is not the most suitable one for discharge prediction in the semiarid catchment considered in this study. Indeed, PDM was developed for simulation of discharge in humid climates, and its application in arid areas can be problematic. I was wondering if this could be the reason for the large deterioration open-loop ensemble prediction. Could the authors add some comments on this point?

3) The seasonal rescaling approach used in the paper allows the observations to be very close to the modelled data. It is evident looking at the correlation values reported in Table 2. Even though this is feasible, I believe that by doing this the impact of data assimilation will be very limited and I would like to know what the impact of this rescaling step is. For instance, what are the differences in the results if the rescaling is done for the whole period (instead of doing it separately for each season)?

4) Another important point is related to the observation error. This is the first paper that considers, in the context of rainfall-runoff modelling, the temporal variability of the observation error, usually assumed as constant in time. However, it should be shown the values of the observation error used in the assimilation. How it varies in time? Could the authors show some plots of the observation error in time? What soil moisture product has the higher/lower error? Finally, what are the differences in the results if the observation error is considered as constant? I am well aware that a single paper can't analyse all the aspects of data assimilation, but some comments

and suggestions should be provided (as it is done for the application of the SWI).

5) The description of the approach employed for the data assimilation in the semi-distributed scheme is not well described. Is the assimilation carried out separately for each sub-catchment? Is it considered the spatial cross-correlation of measurements? Could the authors specify better these aspects?

6) The results in the calibration period are not reported. What is the model performance at the outlet, and for the inner catchments? Is the model able to capture flood peaks satisfactorily (from Figure 4a it seems that PDM always underestimate the highest peaks)? What are the differences in the performance between the calibration and the validation period? I also suggest including all the performance scores (e.g. POD and FAR) used for the assessment of the results before and after the assimilation as reported in Table 3. The addition of the errors on peak discharge and volume would be useful for the evaluation of the results in a context of flood prediction.

In the Specific Comments I reported a number of corrections/explanations that are required.

On this basis, I feel that the paper deserve to be published on HESS after major revision.

Specific Comments/ Technical Corrections (P: page, L: line or lines)

P10636, L5: "...we assimilate active and passive satellite soil moisture ...". The name of the products should be given in the abstract.

P10644, L6ss: The satellite soil moisture observations are assimilated sequentially in this study. Is there an impact in the order of the products that are assimilated? I.e., first AMSR-E, then ASCAT and finally SMOS. If you change the order, do the results remain the same?

P10650, L9: The use of only one year for the calibration of T parameter is likely not sufficient.

P10655, L11: It is very good to show that negative NS-values are obtained, usually this is not done. However, I believe that some investigations on the reasons for these bad performance should be given. Is it due to the PDM model (see General Comments), or to the input data, or to the model parameterization?

P10656, L20: The rank histogram of the soil moisture ensemble might be also analysed here.

P10657, L21-26: A T-value equal to 40 days is obtained for SMOS. This is not expected, as the SMOS soil moisture product should be the one with the higher penetration depth and, hence, the lower T value. It is the opposite. Could the authors add some explanations for that? Could this value be attributed to the noise of satellite data (e.g. due to RFI)?

P106568, L27: "…seems to be less sensitive to these violated assumptions…". What is the proof of this sentence? The reference to another study performing data assimilation is, at least for me, not enough.

Figure 2: SMOS should start in 2010. From the figure it seems starting in 2009. Please check.

Figure 9: The reference Brocca et al. (2010) should be the paper on Remote Sensing of Environment (Brocca, L., Melone, F., Moramarco, T., Wagner, W., Hasenauer, S. (2010). ASCAT Soil Wetness Index validation through in-situ and modeled soil moisture data in central Italy. Remote Sensing of Environment, 114 (11), 2745-2755.), not the one on HESS.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 10635, 2014.