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Interactive comment

Interactive comment on "Assimilation of SMOS soil moisture into a distributed hydrological model and impacts on the water cycle variables over the Ouémé catchment in Benin" *by* D. J. Leroux et al.

Anonymous Referee #2

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First of all, I would like to apologize for my very late review.

The study of Leroux et al. investigates the impact of satellite soil moisture data assimilation on simulated streamflow, soil moisture and water table depth in the Ouémé catchment in Benin. Using in situ measurements from this densely equipped river basin as a reference, the results demonstrate that the SMOS soil moisture data assimilation improves the simulations of all three variables regardless of the precipitation data product that is used to run the model. The results are in line with those obtained in a number of similar studies reported in the recent literature. The authors do not introduce any significant methodological developments, but rely on a rather standard data processing and assimilation procedure to do their analysis. This in itself is not a problem given

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Discussion paper

the fact that the number of such studies is still rather limited. Experiments with different soil moisture data sets, different models and in different experimental catchments are clearly welcome to get a better understanding of the advantages and limitations of satellite soil moisture data assimilation for hydrological predictions. The single most interesting result for me was the possibility to significantly improve streamflow and water table depth simulations of the model that uses in situ measured precipitation data as input data. This is the most challenging setup and it is an important result that improvements were obtained for such a scenario. It is more difficult to evaluate the meaning and merit of the results obtained when satellite rainfall products were used as forcing data. My main concern relates to the design of the related experiments. The hydrological model is first calibrated with in situ measured streamflow and soil moisture. The same optimal parameter set is then used when highly biased satellite precipitation data sets are used as forcing data. In fact the satellite soil moisture data is bias corrected before the assimilation is carried out. The reference seems to be the open loop of the calibrated models using in situ measured precipitation as input. I found this experimental setup questionable for two main reasons. First of all, it is rather obvious that the (unbiased) satellite soil moisture data assimilation will improve the (biased) soil moisture simulations that are obtained when using the different (biased) precipitation data sets as inputs. Second, I would argue that it is a very unrealistic scenario to assume that a model that has been calibrated with in situ measured discharge and soil moisture uses as input satellite precipitation products. The more likely scenario is that satellite precipitation is the only available data source for calibrating the model. Therefore, in my opinion, it would have been preferable to re-calibrate the model for each satellite precipitation product before carrying out the soil moisture data assimilation. The authors explain that they did not proceed like this because of the compensation effects that would impact the model parameters when biased forcing data are used. This is true, but in this experiment the compensation takes place whenever a soil moisture data set is assimilated. I have further some concerns regarding the methodology. It is not clear to me how the model error covariance matrix is defined. The authors mention that fixed values are used but do not explain how this was done. Moreover, is seems as if the same values were used regardless of the input precipitation data. This would be a rather gross simplification as obviously the uncertainties of the state variables very much depend on the quality of the forcing data. The covariance matrix should reflect the fact that the satellite precipitation data are much less reliable than the in situ data. An approach based on the generation of ensemble members would be more adequate in my opinion. It is also not clear how the R matrix was setup. Is this just the variance obtained from each pixel's time series? I don't agree that this can be used to estimate the uncertainty of the observations. The analysis of the residuals with respect to the in situ data (where available) would provide a better estimate. Other than that there is triple collocation and its numerous variants. I found it also surprising that the variance attributed to the SMOS observations is that low. Finally, I noticed on Figure 6 that the SMOS observations are well distributed around the in situ measured soil moisture data but less so around the "open loop + precip in situ". This would suggest that a CDF matching was carried out with respect to the in situ data and not the open loop. Or are these the SMOS data points before a CDF matching was applied? In fact I am wondering how the SMOS data assimilation e.g. in May 2012 was able to lead to an almost perfect match with the in situ data when the "open loop + precip in situ" and the SMOS data points were both slightly biased in that time period.

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