Anonymous Referee #1

Received and published: 29 February 2016

This study aims at assimilating SMOS soil moisture observations to correct for errors in precipiation and reduce uncertainties in simulated discharge for a catchment in Benin. The study uses 4 precipitation datasets to capture the uncertainties in the simulated soil moisture. This is a nice detailed study that goes beyond the standard work and should be considered for publications. However, I do feel that the authors can make some improvements and therefore recommend the revisions below. One more general remark is that the goals seems to be to improve streamflow simulations, while the paper reads more like a paper that does precipitation correction using satellite soil moisture and thereby obtain improved simulations.

We would like to thank Anonymous Referee #1 for the constructive remarks, which made this work more thorough. Based on these remarks, the objective of the work has been refined in the abstract and hopefully throughout the whole manuscript.
In the original paper, the real-time rainfall products (PERSIANN, TRMM, and CMORPH) are used in the model, which bring too much water. Soil moisture assimilation can attenuate the effect of bringing too much water over the whole basin by correcting the soil moisture content. This positive bias in the real-time precipitation products has already been identified and corrected in reanalyzed datasets: PERSIANN-CDR, TRMM-v7, and CMORPH-v1. When these bias-corrected rainfall products are used, the simulations are much better and in very good agreement with in situ measurements (soil moisture, water table depth, streamflow). However, these products are only available at least 2 months after the real-time products. The objective of the paper is now focused on a proposition of a fair approach to fill the gap of these few months.

<u>Major remarks</u>

Page 3, Line 8-13 Some recent studies have actually used assimilated SMOS soil moisture at a scale comparable to the catchment in the work (Lievens et al 2015 and Wanders et al 2014). These studies show that the assimilation of SMOS has a positive impact on the streamflow estimations in some scenarios. It is stated in the manuscript that it has no impact, which is contradicted by the above mentioned studies. I think a comparison with these studies would be valuable for the reader.

The last part of the sentence has been removed ("but has little impact on the streamflow estimation") as it not true that all these studies found that SM assimilation did not improve the streamflow simulation. We apologize, it was a mistake. These references have been added in the introduction as proposed by the referee and as it is quite relevant for the reader as well.

Some restructuring of the introduction would help to more clearly state the research gap that this paper would like to fill. I my opinion, the most novel thing done in this study is the use of multiple precipitation forcing product and the impact of SMOS DA on the hydrological simulations with these products. Now it states (Page 4, line 16-17) that SMOS assimilation impact on streamflow is the main goal, while in Page, Lines 13 it was stated that assimilation of SMOS has no impact on the streamflow performance.

Some parts of the introduction have restructured and reorganized. The last two paragraphs of the introductions have been modified so that the main objective of this work is now clearly stated: "Reanalyzed versions of the satellite precipitation products, correcting for their initial inaccuracies, are often available but only after several weeks or months after the observations, *which can be an issue for operational systems."*

"The objective of this study is to constrain the water and energy balances by assimilating surface soil moisture satellite observations using the near-real time satellite rainfall products.Our study focuses on the assimilation of SMOS soil moisture over a West African catchment in Benin and investigates its impact on other hydrological variables. A first part of this article presents the Ouémé catchment, the in situ measurements and the satellite data. Then the hydrological model DHSVM is briefly described along with the assimilation method. The results of the assimilation are presented in the last section before the conclusions."

Page 5 Line 25-26 Using rainfall satellite product doesn't make it challenger. If one would use perfect rainfall data the potential of SMOS for streamflow improvements would be almost zero, while if the rainfall is very imperfect the potential impact is significant since the initial guess is far of and the potential improvement is large. Please remove or correct this incorrect statement.

The referee is absolutely right. This sentence is very confusing as the further you start from the truth, the bigger impact the assimilation will have. So this case should not be called "challenging" but the exact opposite since it is expected to get the best improvement. The whole sentence has been removed.

Page 5 Line 31, Why is the 3B42RT product used instead of the reanalysis product of TRMM, which is gauge corrected and therefore has a higher quality compared to reality.

As other referees suggest, the reanalyzed rainfall products have been added to the study in order to compare their performances with those of the assimilation. And, after bias-correction, the open-loop give much better results than the real-time products.

Table 2, how is it possible that the quality of the SM simulations after assimilation show a decreased performance compared to before assimilation. Does this mean that SMOS and the observations are not well aligned or is the DA procedure sub-optimal? Does result is at least counter intuitive to what one would expected after DA of additional observations.

As explained in the text, only the bias results should be judged. Correlation, sdd and rmse are impacted by the discontinuities introduced by the assimilation when the soil moisture corrections are applied. If the observation is drier than the simulation, then water is removed from the ground and a discontinuity appears, which artificially increases the sdd and the rmse and lowers the correlation.
In the case of in situ precipitation, the assimilation does not improve the performances as it tends to only add noise to simulations that are already good. In the cases of real-time precipitation products, the bias are always reduced after assimilation.
However, the assimilation technique implemented here is quite basic since it is the Optimal Interpolation. With this technique, the B and R matrix are set by the user and do not evolve in time. So the DA might not be as optimal as an Ensemble Kalman Filter could be.

I think some maps of the spatial improvement of the simulations would help the reader to get a better feeling with regard to where the largest potential is for further improvement. Is it the upstream areas or are better results obtained in other regions.

In order to draw maps of improvement of surface parameters such as the soil moisture, a map of the

“true” state is necessary. In situ measurements are only available at several locations which are not enough to interpolate for the whole basin.
Regarding the streamflow simulations, only two points at the outlet of each sub-basin are simulated, so it is difficult to say if the improvements/changes are better upstream or downstream, but it would be interesting to simulate a streamflow at each point of the river and compare them with available in situ measurements.

<u>Minor remarks</u>

With respect to the precipitation corrections that are in a way done I think it would be useful to mention here some studies that focus on this aspect (e.g. Crow & Bolten, 2007; Crow et al. 2011; Pellarin et al 2013; Wanders et al 2015)

Thank you very much for these references. In the introduction, a paragraph has been added explaining that several studies have showed that it was possible to correct real-time precipitation products using satellite observations. And it has been added that the present study is different in the sense that the correction of these inaccurate real-time precipitations is operated within the hydrological model, as opposed to the references given here.

Figure 1, the quality of the image low in my version of the manuscript

It was not supposed to be a low quality image. It is not the case in the author version. But I will check on the revised version.

Page2, Line 14 in space should be spatial

It has been corrected, thank you.

Page 2, Line 21, 0.04 is only the mission requirement of SMOS and not its actual accuracy or provide a reference to confirm this.

The reviewer is correct, this is a target accuracy set by the mission requirements. Modifications have been added to the text (“with a mission requirement accuracy of 0.04 m3/m3”).

Table 3 This table tells me with far from perfect precipitation one can gain a lot from the assimilation of satellite derived SM data, while if the forcing is almost perfect the assimilation of SM is a difficult and potentially low gain approach. Maybe some of this should be mentioned in the dicussion

Some words have been added to the paragraph beginning with “Another representation of these statistics is the Taylor diagram in Fig....”.

Table 4 The % are not well explained in the caption of the table, please adjust. TRMM

after assimilation, should that not be -6%?

The % are actually explained at the bottom of the table ("statistics compared to OL simulations using in situ precipitations"), but this description should be mentioned in the caption instead. It has been moved and changed to "The percentages between parenthesis indicate the comparison with the OL simulations using the in situ precipitations that are used for reference.". Thank you for the suggestion.