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Discussions

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 #1

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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.

Major remarks

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.

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

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.

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.

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.

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.

Minor remarks

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)

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

Page2, Line 14 in space should be spatial

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.

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

Table 4 The % are not well explained in the caption of the table, please adjust. TRMM after assimilation, should that not be -6%?

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