Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-41-RC1, 2019

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Dual state/rainfall correction via soil moisture assimilation for improved streamflow simulation: Evaluation of a large-scale implementation with SMAP satellite data" *by* Yixin Mao et al.

Christian Massari (Referee)

christian.massari@irpi.cnr.it

Received and published: 19 April 2019

Review of the manuscript:

Dual state/rainfall correction via soil moisture assimilation for improved streamflow simulation: Evaluation of a large-scale implementation with SMAP satellite data.

by Mao et al.

The manuscript deals with the assimilation of satellite soil moisture observations de-

rived from the SMAP mission for improving streamflow simulations in the Arkansas-Red River basin. The ingestion of SMAP satellite soil moisture data into the system is carried via the so called "Dual state/rainfall correction" a method already used by one of the authors in other studies published in recent years. With respect to previous studies the authors used a satellite precipitation estimates derived from the new Global Precipitation Measurement mission (GPM), i.e., the IMERG Early run product which is the near real time version of the three available IMERG products. With respect to the products available in the past like those derived from the TRMM mission this satellite product has increased the accuracy and detection skill thanks to a new Dual Precipitation radar with light rainfall detection capabilities. Therefore one of the research question is whether satellite soil moisture observations are still able to improve the quality of the rainfall time series (and also the model states) in a way that it can be beneficial for streamflow simulations. In particular, satellite soil moisture observations are derived from the recent SMAP mission which has demonstrated to release products with a relatively high signal to noise ratio.

The manuscript is well written and clear. It is also of interest for the journal readership as the use of these new satellite products has been explored very little due to the relatively short observation period. The focus on this "dual correction" scheme is also very interesting as the improvement of streamflow simulations can be obtained either via the correction of states (in this case several an improvement of low flows is obtained with respect to high flows) or via correction of rainfall (which seems to have a benefit on high flows, from previous studies cited by the authors).

I have only one major comment which is related to the rainfall correction and its effect on the streamflow simulations which to me is a bit ambiguous and should be improved. In many parts of the manuscript it is said that the correction of the rainfall has a smaller effect since the rainfall forcing used (IMERG-ER) has a good quality (see lines 331 onward). However, this contradict with the results in Table 3 where the open loop simulations show in some cases of very poor performance of flood simulations (which

are likely due the poor rainfall quality) and with other sentences stating that the IMERG-ER has large errors (line 448) in some basins. In fact, when forced by NLDAS2 there is a significant increase of the model performance up to 80% of PER which however, is still not satisfactory for some basins (see Table 3 Walnut, Chikaskia and Spring).

Then, I think there two possible reasons. Either SMAP adds little in terms of rainfall correction or SMART only corrects for the random error component which is the component the hydrological model is less sensitive to as correctly stated by the authors. Therefore the systematic error can be very important in this respect. However as between the two precipitation products it is difficult to judge which one is really better (at least by looking at the performance in terms of KGE) I suggest to compare them with a gauge-based dataset like CPC or Stage IV both in terms of rainfall (bias, correlation and error) and in terms of streamflow simulations. Indeed it is well known that these two products works really well in US (see for example the last study of Beck et al. 2019 where they used Stage IV as a reference for validating precipitation products over CONUS). Another solution could be to drive VIC model with IMERG final run which is corrected with rain gauge and therefore should have a lower bias with its near real time counterpart and thus would explain if the systematic error is the real problem. To summarize my suggestion is to include in the study a reference precipitation product against to compare IMERG-ER and NLDAS2. That would shed some light on the problems of the poor performance simulations.

Based on that, my suggestion is MODERATE revisions of the manuscript.

Christian Massari

Beck, H. E., Pan, M., Roy, T., Weedon, G. P., Pappenberger, F., van Dijk, A. I., ... & Wood, E. F. (2019). Daily evaluation of 26 precipitation datasets using Stage-IV gauge-radar data for the CONUS. Hydrology and Earth System Sciences, 23(1), 207-224.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-41, 2019.