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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.

Anonymous Referee #2

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In this work, authors have attempted to carry out assimilation of SMAP soil moisture to correct rainfall using SMART algorithm and soil moisture state of VIC model using Ensemble Smoother over Arkansas-Red sub-basins.

The topic is of interest to hydrological community and the some of the conclusions made are important. However, the quality of writing not up the standard of HESS. As the authors have acknowledged that the methods used in this paper have already been implemented elsewhere in the literature, and the only "new" contribution is in terms of using new datasets, there should have been deeper discussion and analysis regarding

the outcome of this experiment. I agree that authors have used Ensemble Smoother as an extension to EnKF in this work. However, the results suggest improvements only when the updates are made at coarse temporal scale (SMAP scale). So, apart from minor differences, there may not be statistically significant difference in terms of performance between the two techniques. I will be glad if I am proven otherwise. Also, most importantly, there was only a speculative attribution of lack of improvement in performance to the better quality of IMERG precipitation. The results lack appropriate robust quantitative analysis in this regard. Further comments are listed below. In summary, the manuscript may have to be revised thoroughly in a wat that highlights the major contributions, and also show how these contributions are helping us to extend our understanding in this domain of research. In this process, please also consider addressing the following specific and minor comments:

Specific Comments:

1) SMAP soil moisture estimates have a maximum sensing depth upto 6 cm in vegetated areas (Babaeian et al., 2019, Reviews of Geophysics). The deeper soil moisture has stable temporal dynamics compared with that of surface soil moisture. Further, the VIC model executed at 10, 40 and 93 cm. In the process of assimilation, the SMAP soil moisture are rescaled to VIC soil moisture dynamics. So, essentially the noisier timeseries (surface soil moisture) is being rescaled using the temporal dynamics of smoother timeseries (VIC soil moisture). Can authors assess the implications of this mismatch on the final outcome?

2) How is the soil moisture state in the deeper layers being updated? Is there a correction factor implemented here, as carried out by Lievens et al. (2015, 2016)? Although authors have mentioned in Line 221, an equation will bring clarity to their statement.

3) Equations will help to understand the mathematically involved procedure like data assimilation.

4) Authors may have to discuss the sensitivity of choosing gamma parameter in Eq.

(1).

5) L 210: There is also a need for authors to explain why the error variance of 0.3 mm2 is chosen and its sensitivity.

6) L: 228: When only top two layers are being updated, why is it that all the three layers are perturbed?

7) L: 230-233: I find that this statement in qualitative in nature. So, it cannot be considered as a finding.

8) Figure 3 is not explained properly. What is the meaning of improvement in correlation? Is it correlation(NLDAS, IMERG_Corrected) – correlation(NLDAS, IMERG_Original)? There is no detail about it in the manuscript.

9) L: 302: If delta and P are aggregated to 3-day windows prior to correction in the case of EnKF, why are there minor changes in the spatial maps in Fig. 3 (d-f)? Will it not be sensible to just have a 3-day window map?

10) Interestingly, there seems to be an overlap in the spatial patterns of Figs. 2 and 3. It appears that there is a correlation improvement in the western part, which received lower rainfall compared to the eastern region. Is there such dependence of rainfall amount on the performance of correction?

11) I think it will be better if bias and error maps are also plotted to comprehensively characterize the errors.

12) L: 333-334: This is one of the most important statements made by authors. I think it is important to support this statement with rigorous analysis. I think it may not be fair to compare these results with that of Table 2. This is because of a) the experimental setup has changed, b) case study has changed, and c) the reference dataset has changed.

13) Figure 3: Since there is a median correlation improvement difference of only 0.01, can't we just use EnKF, which is much simpler compared to Ensemble Smoother?

14) Figure 4: It is understandable that in the case of correcting rainfall at all timesteps, SMART can misinterpret SM retrieval noise as small rainfall corrections. Can this issue be alleviated by considering a threshold of, say 2 mm to classify rain/no-rain and continuously correct the rainfall. This way the SM retrieval noise can still be pushed to zero, and there may some reduction of uncertainty due to rain/no-rain classification.

15) L: 318 is a speculative statement with no strong analysis.

16) Section 3.1.2: (in alignment with my Comment 12) I think correlation may not be sufficient to conclude on the quality of rainfall product. There can be other forms of error (such as bias), which are not being considered in this analysis.

17) Authors should provide some insights into the spatial patterns in Fig. 5. If median value is all that is needed in the discussion, then what is the need to have such spatial maps?

18) Section 3.2.1: I think there is a need to compare the rainfall products with a third product to get a complete picture of relative errors between the products.

19) There is no discussion regarding Figs. 6 and 7 in the manuscript.

20) Fig. 7 Deep Site: Between June and July although there are spikes in the ensemble, why isn't there a peak in dual corrected time series (which is ensemble-mean)? Also, since these are unregulated catchments, any peak can be attributed to rainfall event. So, if there are spikes in the ensemble during this period, does it mean a) there is an anomalous rainfall or b) the assimilation technique erroneously updated the rainfall during this period? I think these streamflow timeseries should also contain rainfall timeseries to look at where the update is being carried out.

21) The discussion section is speculative not very convincing. Authors may have to carryout robust analysis to substantiate their findings.

Minor Comments:

22) Figure 4: the x-axis is not explained properly.

23) Abstract opens with statement that soil moisture is necessary for accurate streamflow simulations. However, the conclusions are slightly contradictory. Please consider revising the abstract appropriately.

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