Hydrology and
Earth System
Sciences
Discussions

Interactive comment on "Satellite soil moisture data assimilation for improved operational continental water balance prediction" *by* Siyuan Tian et al.

Anonymous Referee #2

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The authors present in their manuscript an application of assimilating SMAP and SMOS soil moisture into the AWRA-L hydrological model. The innovation of this manuscript lies in the development of a two-step data assimilation approach. In the first step, model states are updated using a Kalman filter type approach whereby error covariances are obtained through triple collocation. The second step is to mitigate the mass balance error created by the data assimilation through what the authors named the Analysis Increment Redistribution approach.

The topic is relevant for reader of HESS. The manuscript is generally well written and methodology and results are well explained. I believe the manuscript can be consid-

ered for publication after consideration of the following comments .

General comments:

* Even though the manuscript is well written general, I found still a number of grammar mistakes. Several of them I have indicated in the specific comments below, but I would recommend the authors to check the manuscript carefully again. * In their DA approach the author assume that the error (co-)variance are temporally constant, while there is ample evidence that this is reality not the case. For instance, due varying sensing depths as a function of the soil moisture content itself. In the discussion section the author mention this as point of improvement for the future, but I would appreciate if the authors could introduce this assumption early in the manuscript.

Section 4: Results

* When presenting your assimilation results figure 4 and onwards, do you only present the results with assimilation of SMAP observations? It would be interesting to see also the results for the assimilation of SMOS to get an idea about the what the effect of observation uncertainty is on the analysis results.

Section 4.3:

* Differences in root zone soil moisture, ET and streamflow after DA are actually quite small, while in figure 8 there is still as substantial difference between the observed and simulation streamflow. I would expect more discussion here on how this gap in streamflow between model and observation can be closed. Can this be done with soil moisture assimilation?

* How do you explain that the correlation between the AWRA-L root zone soil moisture and NDVI improves, while the correlation with the root zone soil moisture measurements do not improve (see box plots)?

Section 4.4

* The authors evaluate the persistence of data assimilation through comparison of the open loop and DA-TCAIR. Could the authors also include the DA-TC in this analysis? I would be interested to see what AIR in itself does to the persistence of the soil moisture data assimilation. This would potentially also support the use of DA-TCAIR over DA-TC.

Specific comments:

Abstract: I would suggest to specify the following in the abstract

* the name of the soil moisture product assimilated

* the method of state updating

L15: Could the authors provide also correlation coefficients for the comparison of the root zone soil moisture and vegetation time series? Instead of only the increment.

L41: 'As the assimilation .. ' Sentence seems incomplete.

P2L45: check sentence.

L61: replace 'has' by 'have'

L65: Could the authors explain why this limits the operational use?

L85-87: Please add references in support of this statement

L111: change Figure1 to Figure 1

L115: What do the authors mean by 'dynamics' and it is unclear why this would flatten to zero as a result of mean and variance matching.

L116: The coefficients of what are derived? More explanation is needed here.

L139: change 'were' to 'was'

Eq. 1. The letter Q is used for the variance while sigma2 also indicate this. Could the authors explain this? Should the reader interpret this both as variances?

L165: Why do the authors make this statement because they apply mean and variance matching to suppress the systematic differences between the observations and simulations.

L182: Why do they authors refer to Crow and Van den Berg (2010) here? If they have used TC as method to derive uncertainty levels I would have expected the reference earlier in the manuscript.

L195-205: How do the authors obtain dM/dx? Is this a fixed value or a quantity that is updated every time step?

Figure 3: Could the authors add a time series of the measured soil moisture to this figure.

L226: Could the author indicate where the Murray-Darling Basin is? Readers not familiar to the continent may not know where it is.