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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Global joint assimilation of GRACE and SMOS for improved estimation of root-zone soil moisture and vegetation response" *by* Siyuan Tian et al.

Anonymous Referee #2

Received and published: 20 October 2018

The submitted manuscript by Siyuan Tian et al investigated the impacts of assimilating satellite water content retrievals on the estimation of surface and root-zone soil moisture over the globe and across different land cover types. The authors aimed at improving the accuracy of root-zone soil moisture prediction by jointly assimilating satellite-observed soil moisture from SMOS and total water storage changes from GRACE into a global ecohydrological model. They then evaluated the performance of the joint assimilation by comparing against the open-loop model and alternative assimilation methods with ground-based soil moisture measurements and vegetation index.

This paper is well written, properly structured and presented, with interesting results

being thoroughly interpreted by a good discussion. I believe this manuscript will be interesting to future HESS readers and contribute to the international literature. There are two major concerns that I would like the authors to address before the publication of this manuscript.

1. While GRACE-derived TWSA provides an integrated measurement of water storage changes above and underneath the earth surface, why would near-surface soil moisture derived from SMOS still be required? Don't SMOS and GRACE monitor overlap water content at near-surface? This has not been fully justified and explained in the Introduction or in the ecohydrological modelling method.

2. Following up Reviewer#1's major comment on assessing assimilated soil moisture using NDVI, I do agree Reviewer#1 that extra experiments of correlation analyses based on de-seasonalized times series of all data are required. Although I agree with the authors that the improvements of the modelled root-zone soil moisture over only ET limited regions are likely due to increased seasonality, authors may need to show how the methods proposed in this study could improve root-zone soil moisture in the long run without the effect of seasonality.

My specific comments are as follows:

1). Page1, Line 4:Do you have references to confirm this? Some people believe GRACE-derived TWSA is mainly dominated by soil moisture variation over many places.

2). Page3, Line 9-18: Introduction is well presented, however, this paragraph of objectives could be improved by clearly numbering each objective such as 1)…. 2)….3)…. This will make it easier for future readers to get straight to the points.

3). Page3, Line 27: includes âĂŤ> including, and these .

4). Page3, Line 21-30: More details of the ecohydrological model (W3) is needed to show how exactly it works.

5). Page7, Line 18-19: Please move API to Materials.

6). Page8, Line 9-11:How can these two statements be justified from Fig.3d? What do R0 and Ra stand for? I assumed they represent correlations for open-loop and joint assimilation? You need to indicate it at least in the Figures.

7). Figure 5 : I suggest authors to label these sample sites on Figure 2.

8). Page8, Line 15: "marginally better than SMOS-only results", which is hard to tell from the figure.

9). Page9, Result-4.2: This section needs extra experiments using de-seasonalized data as mentioned in the major concern 2.

10). Page12, Line 26-27: There is a recent study very relevant to this statement that used GRACE-derived TWSA for Australia.

Xie, Z., Huete, A., Restrepo-Coupe, N., Ma, X., Devadas, R., Caprarelli, G., 2016. Spatial partitioning and temporal evolution of Australia's total water storage under extreme hydroclimatic impacts. Remote Sensing of Environment. 183, 43–52.

11). Page12, Line 28-29: This is likely to be attributed to 2015 El Niño impact.

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