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

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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 â­Ť> including, and these .

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

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.


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