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Interactive comment on "Assimilation of SMOS Brightness Temperatures or Soil Moisture Retrievals into a Land Surface Model" *by* Gabriëlle J. M. De Lannoy and Rolf H. Reichle

Anonymous Referee #2

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General comments

Thank the authors for this interesting work. Based on a catchment land surface model and the advanced EnKF data assimilation technique, this study employed several experiments trying to answer the question on "how to make the best use of L-band microwave satellite observations through DA". Prior to the assimilation, a sophisticated data quality control, model perturbation, and bias mitigation in both TB and SM retrievals are applied. Finally, DA outputs are carefully evaluated by comparing with in-situ measurements and with special attention on DA innovation and increments. The findings provide important inspirations for further SMAP DA and the manuscript is overall well organized. However, some statements within this manuscript remains unclear

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Discussion paper

to me and more details are needed. I therefore recommend this manuscript being published in Hydrology and Earth System Sciences by taking care of the following minor comments.

Specific comments

1. P1, Line 9-10: soil moisture evaluations are based on anomaly rather than the absolute values. Thus I would suggest rephrasing this sentence as "... to model-only simulations in terms of unbiased root mean square difference and anomaly correlations during the period ..."

2. P3, Line 5: what does the "treatment" exactly refer to? Do the authors mean RFI and uncertainty screening and regridding as depicted in section 2.2? if yes, I would not consider this as a major difference compared with previous studies.

3. P5, Line 1-6: what is a "footprint scale"? As I read from Table 1 of De Lannoy et al. 2014b, the RTM calibrated parameters are assigned to the same IGBP vegetation class, but how do you manage to make "all 36-km grid cells within one footprint area are assigned the same set of RTM parameters". When you practically do assimilation for a specific "footprint", does all the 36-km grids use the same RTM parameters or they are vegetation-class-dependent? Please elaborate.

4. P5, Line 30: what criterion do you use when excluding frozen soil and snow cover during assimilation?

5. P8, Line 23-25: it looks the representativeness error during the upscaling process as described in P3, Line 26-29, is not considered.

6. P8, Line 6-7: the work of Reichle and Koster (2004, GRL) looks a better reference on CDF-matching. Besides, on what temporal scale did the authors conduct this CDF-matching? Is it for each month, year, or the entire study period (2010-2015)? As also stated by the authors in P10, Line 5-6, could the SM innovation be seasonally corrected as well if do CDF-matching on a monthly basis? Please clarify.

7. P10, Line 13-19: another reason for the degradation of TB assimilation might be the modeled vegetation. Meanwhile, Leaf area index, other than vegetation water content, has been found to be reliable in estimating vegetation optical depth at global scale (Kerr et al. 2012, IEEE-TGRS). To ingest real-time dynamic vegetation observations (e.g., LAI from MODIS) might help mitigate the TB assimilation. In any case, RTM over highly vegetated land cover is always tough, and the authors may consider excluding areas with vegetation water content over 5 kg m-2.

8. The in-situ soil moisture is usually measured at the 5 cm depth whereas the model output represents the first layer's average (0-5 cm), and this vertical depth-mismatch can potentially introduce biases in soil moisture evaluation given that the topsoil moisture usually have larger variations. Horizontally, the direct comparison between model estimate of a gridcell average and point-scale in-situ observations can also be questioned due to high sub-grid heterogeneity. For the former, it could be alleviated by configure the land model to have denser soil layers at the top. For the latter, a way of mitigating this spatial representativeness issue is to compare their spatial averages (e.g., Xia et al. 2014, JH). However, I realize it might be difficult for the authors to reconfigure the land model or redesign the evaluation scheme within this paper but it can be considered in future studies.

9. Similar DA framework has already been used in the SMAP_L4 algorithm to produce a value-added root zone soil moisture. Thus in the conclusion section I would like to see a short paragraph of the authors' speculation on possible improvements in the future SMAP TB and SM assimilation as well as the feasibility of their joint assimilation given that these two products complementarily have different spatial coverage and content different land surface information.

Technical corrections

1. P7, Line 14: "as well as surface soil temperature. . ."

2. P22, Figure 3: should the captions of g, h, and i be for subplots j, k, and l?

Additional references

Reichle, R. H., and R. D. Koster (2004), Bias reduction in short records of satellite soil moisture, Geophysical Research Letters, 31(19), L19501, doi:10.1029/2004GL020938.

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