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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Development of Soil Moisture Profiles Through Coupled Microwave-Thermal Infrared Observations in the Southeastern United States" *by* Vikalp Mishra et al.

Anonymous Referee #1

Received and published: 23 October 2017

Here I reviewed "Development of Soil Moisture Profiles Through Coupled Microwave-Thermal Infrared Observations in the Southeastern United States" by Vikalp Mishra et al. The manuscript aims to develop a soil moisture profile estimation methodology based on remote sensing data. It may have some potential contributions to global scale root zone soil moisture estimation. However, some key information was missing in the figures, which made the manuscript hard to follow and evaluate. Further, the structure of the manuscript should be better organized.

General comments: 1. AMSR-E downscaling: I would first suggest the authors to

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clarify their research goals. If providing high resolution data is not part of the goal, the authors can perform analysis at 25-km, i.e. upscale ALEXI. Downscaling MS is usually challenging. The consequences on POME are also difficult to evaluate, as pointed by the authors (page 16, line 535).

2. The proposed method can only handle cases with soil moisture is linearly increasing/decreasing with depth, if I am correct. If that is the case, the authors should discuss why the proposed method is preferable than other remote sensing based method, e.g. exponential filter (Albergel et al., 2008).

3. Please add units to all the figures

4. The conclusions should be presented in a more concisely.

Detailed comments: 1. Line 16 to 18: please add units to all the numbers being reported. I assume it is in m3/m3?

2. Line 67 to 69: please revise/modify the goal here. The authors should at least mention the methodology should satisfy what applications.

3. Section 2.1: Please specify why this area is selected. It is known that AMSR-E has the poorest performances over dense vegetation areas. This means AMSR-E is usually more accurate over southwest part of the CONUS and less accurate over the eastern part of the CONUS.

4. Section 2.2.1: Please justify why LPRM based C-band AMSR-E data were not used, since it is usually considered to have a better quality?

5. Equation 6 and 7: the author can remove one of them

6. The captions of figure 2 should be modified. Please specify which products are compared in the caption.

7. Line 343 and elsewhere: RMSE is the root mean square error. It can be calculated when you have a known “truth” or a very good reference. If NOAH is not assumed to

be perfect, I would suggest the author to change it into RMSD, i.e. root mean square difference.

8. Line 349: provide the unit here is m3/m3, I would not say ubRMSE = 0.06 m3/m3 is small. . .

9. Line 353: the author may need to define a threshold of "well" or "good". As shown in the third row of Figure 2, large fractions of correlations are below 0.4. It is hardly to be considered as "well" in my background. However, I agree that this threshold varies according to different applications.

10. Figure 5: please add row indices.

11. Line 476 to 477: please rephrase.

12. Line 478: the implementation of TC should include more information. Was the climatology removed from each dataset?

13. Line 484 and figure 7: it is not common to express TC results in R2. Please specify how this metric was derived.

14. Line 488 to 490 is incorrect. TC estimates the total error, instead of just the random error. Please see Yilmaz and Crow 2014. This means if NLDAS is less accurate, either due to random or temporally correlated errors, it will be shown in the TC results.

15. Section 4.4.1: please refer to my general comment. If the reviewer can perform analysis at coarse scales, this section is unnecessary. This may make the manuscript cleaner.

16. Line 578 to 579: I would not consider the minimum bias is the key advantage of POME. This is because it is nearly impossible to define an absolute bias at large scales, since the reference dataset (e.g. SCAN) can also be biased.

17. Line 585: Root zone soil moisture at large scales can have significant spatial variability, according to my experience. It can result in large errors/bias using limited

point sensors to represent large scale root zone soil moisture. Hence, I'm suspecting how confidently the authors can draw this conclusion.

18. Line 598 to 600: please refer to my General Comment 1

References: Albergel, Clément, et al. "From near-surface to root-zone soil moisture using an exponential filter: an assessment of the method based on in-situ observations and model simulations." Hydrology and Earth System Sciences Discussions 12 (2008): 1323-1337. Yilmaz, M. Tugrul, and Wade T. Crow. "Evaluation of assumptions in soil moisture triple collocation analysis." Journal of Hydrometeorology 15.3 (2014): 1293-1302.

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