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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 #2

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This study presents the application of the principle of maximum entropy model (POME) to estimate soil moisture profiles from remote sensing data for the area of the southeastern United States. The ultimate goal of the approach is to estimate soil moisture profiles from remote sensing data exclusively without the need of onsite measurements or additional information obtained from a land surface model. The only input data required by POME are ground surface soil moisture content (in this case derived from downscaled AMSR-E) data and profile mean soil moisture content estimated from thermal-infrared data using the ALEXI surface energy balance model. POME-

estimated soil moisture profiles are compared to soil moisture profiles simulated with the Noah land surface model and in situ data from 10 sites (SCAN sites) distributed throughout the study area and representing different land uses. Comparison is done by simple statistics (bias, RMSE, unbiased RMSE and correlation coefficient), whereas mean values and, in some cases, pixelwise values are evaluated and advantages and limitations of the different models and datasets are critically discussed.

Overall the paper is very well written and the text is well supported by appropriate, high-quality figures. I am a soil hydrologist by education and I am not or only little familiar with most of the models and datasets applied in this study, so it is rather challenging to judge whether the application of the presented models and data products is a good choice or reasonable. In this respect, I have to rely on the experience of the authors. Nevertheless, I have some comments I would like to have elaborated or explained in a revised version of the manuscript:

POME model: I would still like to have some more information about the model. It only requires surface soil moisture and soil profile mean soil moisture as input information and assumes that moisture is either monotonically increasing or decreasing. If this is not the case, an inflection point that is located in the soil layer with the highest field capacity is assumed

- How does the POME model consider several layers of different texture? They might exhibit rather different soil moisture contents with distinct "jumps" between layers. In addition, infiltration fronts might induce further "inflection points". I cannot really imagine how this can be estimated from surface soil moisture and mean profile water content only. Will there be just one optimal soil moisture profile or might there be several (concept of equifinality).

- How is the lower boundary condition parameterized?

As the authors state correctly, both the Noah and the POME model will be affected by model errors and consequently may provide erroneous soil moisture profiles while the in situ data were measured at a much smaller measurement scale and the measurement site may not be representative for the entire pixel. Nevertheless, for a soil hydrologist, the time series shown in Figure 6 provide the most valuable information for identifying model errors and weaknesses of models and data. Unfortunately, this figure is almost not discussed within the manuscript. Observations to be addressed are e.g.

- Except some sites where the SCAN data show some bias (mostly towards the dry range), SCAN and Noah data appear to agree rather well, both in absolute soil moisture content and in the amplitude of the annual dynamics whereas POME often shows a very strong dynamics (especially towards the wet range). Might this cause problems with the upper boundary condition?

- The same strong dynamics occur in the Noah model at the lower boundary condition at many sites whereas, in this layer, POME and SCAN data better correspond to each other

- In some cases, the SCAN data sometimes show strong (or strange) soil moisture dynamics in the lower part of the profile (2013, 2115) while dynamics in the upper part of the profile is much less pronounced. Please explain.

Specific comments and technical corrections:

L5: please correct: ALEXI

L39: replace “and” by “that”

L43: define CONUS

L67-84: In my view, these two paragraphs would better fit into the methods section

L153: typo: consistent

L159: yes – please define which meteorological forcing was applied in Noah. Was the model calibrated and if yes, to which data?

L162-165: Yes but then it must be proven that the POME-derived soil moisture profiles are correct. How will they be validated if not in a similar approach as applied in this manuscript?

L170: Please provide reference for the MW soil moisture depth? I think at X-band it should be even less than 2-2.5 cm

L183-185: No sentence – please revise

L337: How were the POME based profiles aggregated?

L424: Don’t the RS measurements “see” the wet surfaces caused by irrigation?

L 433: please correct: Fig. 4

L 459: How was this aggregation done?

L482: Aren’t the SCAN observations the “true” SM? The RS soil moisture contents are also estimates.

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