**Anonymous Referee #1**

**General Comments:**

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

**Response:** One of the primary goals of the research is to provide remotely sensed soil moisture profiles at operational or near operational (1-5 km), spatial resolutions. At present the authors have a suite of hydrologic and agricultural models running over the Southeastern United States at a 4.7-km resolution to be consistent with the NWS Stage IV multi-sensor precipitation product. The NASA SPoRT project at MSFC also maintains a number of near-real-time land surface models within the NASA Land Information System (LIS) at a 3-km resolution (SPoRT LIS). So the purpose of our research was to provide soil moisture profiles at a resolution that would be consistent with our current modeling system and that can be compared to the land surface models within the SPoRT-LIS framework. We also note that the downscaling method we utilized appears to have improved the coarse resolution microwave data in a number of ways. We will revise the objectives section of the manuscript to provide further explanation of our goals in this regard (see response to detailed comment #2 below).

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).

**Response:** We thank the referee for this comment and agree that perhaps the manuscript can be improved by further discussion on this point. While the entropy method does assume a uniform probability distribution of soil moisture initially, the final profiles are not linear in real space after the integral is performed subject to the boundary conditions and mean moisture content. We refer the referee to our previous paper on this point (Mishra et al., 2013) where a full range of profiles are shown corresponding to all possible cases that may arise in nature. Unlike other methods, the entropy approach suffers from no a priori assumptions about the nature or shape of the moisture profiles in real space. The method is a statistical approach and guarantees the minimum variance unbiased profile subject to the boundary and initial conditions specified. We consider this an improvement over other analytical methods that do presuppose a functional form of the soil moisture distribution. We refer the referee to the article by Singh, (2010) for a full explanation of the theory of entropy of moisture movement in porous media. Further, the method can easily be modified even to include non-monotonic profiles through the identification of the inflexion point based on a few well-known principles of vadose zone hydrology (Mishra et al., 2015). In addition, the proposed method is ideal for the integration of remotely sensed data from multiple sensors which is one of the primary goals of the research. In fact, the primary objective of the project was to merge microwave (MW) and thermal infrared (TIR) soil moisture estimates into a unified profile. We will revise the manuscript to add this material.

3. Please add units to all the figures

**Response:** Thank you for pointing out this oversight.

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

**Response:** We will endeavor to condense the conclusion section by converting bullets into 3 concise paragraphs. We will then add another paragraph with condensed version of the error issues.

**Detailed comments:**

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

**Response:** Thank you for pointing out this oversight.

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

**Response:** If we understand the comment correctly, the referee would like for us to mention the purpose, or potential uses if the developed profiles here. As the referee mentions in the first item above, perhaps this is a good place to position some of the material discussed in that item.

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.

**Response:** This is an excellent point. As mentioned previously, the authors currently participate in an extensive research program centered in the Southeastern US. The region is data rich, thus providing us with ample opportunities to test and validate the results. However, as the referee mentions, the region represents a humid subtropical climate with nearly 85% of the area represented by either forest or shrubs, making it one of the toughest regions for MW based SM estimations. As pointed out in the manuscript, the X-band MW signal is greatly attenuated by moderate to high vegetation cover such as represented by the selected study region. Therefore, we feel that, in addition to providing added benefit to the research that we are already doing in the Southeast, the study represents an opportunity to evaluate the performance of the merged MW/TIR profiles in a challenging environment. If successful, then the study will provide greater confidence towards the robustness of the method. We believe that the results of the study do provide evidence of that robustness. This will be clarified in the revised manuscript.

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?

**Response:** It is known that the lower frequency bands are better for surface SM detection since the higher bands suffer disproportionately from the effects of atmospheric interference, vegetation cover and radio interference (Albergel et al., 2011; Brocca et al., 2011). This study employs one of the more extensively used and validated MW based SM data sets from the AMSR-E (2002-2011) mission operating in the X-band frequency from the National Snow and Ice Data Center (NSIDC) and employs the standard NASA retrieval algorithm (Njoku et al., 2003). The NSIDC is one of two AMSR-E data sets supported by NASA with the other being the Vrije Universiteit Amsterdam - Land Parameter Retrieval Model (VUA-LPRM) data. The LPRM uses a single dual polarized channel (X- or C- band) to deduce relationships between geophysical variables such as soil moisture and vegetation characteristics and brightness temperatures (Cho et al., 2015). Several studies such as Wagner et al. (2007); Draper et al. (2011); Jackson et al. (2010); Gruhier et al. (2008) etc. have compared the two data sets and the general conclusion has been that the VUA-LPRM algorithm may be slightly superior in terms of correlation to *in-situ* data, especially at lower latitudes and sparse vegetation (Brocca et al., 2011). However, Jackson et al. (2010) found that for southeast U.S, the NASA retrieval algorithm outperformed the LPRM in terms of bias and RMSE. Furthermore, as pointed out by authors such as Njoku et al. (2005); Jackson et al. (2010) the effects of radio frequency interference (RFI) on C-band are more pronounced over countries such as United States and Japan, therefore X-band retrievals are preferred over such regions. Hence X-band based on the standard NASA (or NSIDC) data set was selected for this study. This issue will be further discussed in the revised manuscript.

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

**Response:** Thank you. We will remove equations 6 and 7.

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

**Response:** We thank the referee for pointing out this oversight.

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.

**Response:** We agree with the reviewer, since neither NOAH nor *in-situ* observations can be considered as a perfect reference due to their inherit errors and scale mismatch issues, we will rectify this mistake and replace RMSE with RMSD in the manuscript.

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

**Response:** We agree that RMSE/RMSD = 0.06 $m^3/m^3$ is little on the higher side, however the statement in question (Line 349) in quotes, "*Moreover < 97% pixels across the study area showed ubRMSE of less than 0.06 across all layers, indicating good agreement between the POME model and Noah SM estimates*". The greater than sign was wrongly put in as less than. The statement simply intent to states that the for more than 97% of pixels the ubRMSE was <u>less than</u> 0.06 $m^3/m^3$

Further, as pointed out by Jackson et al. (2010), the agencies such as NASA and JAXA specified an accuracy goal of errors less than 0.06 $m^3/m^3$ for AMSR-E retrievals. Since this study utilizes AMSR-E SM estimates, we assumed the similar accuracy standard. Although, we acknowledge some inconsistencies, such as spatial mismatch (originally AMSR-E spatial resolution is 25 km while we downscaled it to 5 km) and the specified accuracy goal was limited to surface measurements so we assumed it would be appropriate to adopt it for the rootzone as well.

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.

**Response:** We agree that correlations below 0.4 would not be generally considered "good". In this paragraph our main point was that the better correlations (0.6 and above) were generally in the agricultural areas. We think that most people would consider such correlations "good" when dealing with remotely sensed data. Further we characterize correlations around 0.5 (0.46-0.54) as "fairly" good, a designation we think many people would agree with. While the figure does show that there are areas where the correlations are not as good, the discussion is focused on the overall correlations.

10. please add row indices.

**Response:** We assume that the referee is referring to the identifications on the rows (i.e., 2009. 2114, etc.). These are the SCAN station ID's and we will add that to the figure.

11. Line 476 to 477: please rephrase.

**Response:** We assume that the referee is referring to the sentence that begins "Figure 6 shows that …". Perhaps the objection is to the general characterization that the figure is showing the poorly defined "accuracy" of the method. If this is the case, then we apologize for the poor wording here. We will expand this section to be more specific in what we mean by accuracy according to the indices reflected in the figure.

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

**Response:** In this study, we used covariance notation to compute TC error, which allows us to solve for the unscaled error variances directly. As pointed out by Gruber et al., (2016), with covariance notation, the posterior scaling of dataset is possible and is an optional process. Also, both scaled based difference notation as well as unscaled covariance notation mathematically leads to same results, therefore either method can be used. Further, the advantage of using covariance notation is that it provides an estimate of sensitivity of the dataset to soil moisture change. The sensitivity estimates allows for additional validation and inter-comparison of the datasets. We will include this information in the revised manuscript.

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

**Response:** Thanks for pointing this out. As mentioned in response 12 above, the study uses covariance notation for TC analysis which also estimates sensitivity of the datasets represented as: $\beta_i^2\sigma_\theta^2$, which can be used to further validate and inter-compare datasets. Recently, McColl et al., (2014) proposed to use TC analysis to estimate the correlation coefficient between datasets involved and the underlying 'true' signal as:

$$R_i^2 = \frac{\beta_i^2\sigma_\theta^2}{\beta_i^2\sigma_\theta^2 + \sigma_{\varepsilon_i}^2}$$

$$\beta_X^2\sigma_\theta^2 = \frac{\sigma_{XY}\sigma_{XZ}}{\sigma_{YZ}}$$

Where X,Y,Z refers to three datasets involved in TC analysis.

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.

**Response:** We agree that TC includes total error and did not mean to infer otherwise. We apologize for the confusion. The point we were making is that the root zone soil moisture values in the NOAH LSM are the result of a deterministic equation and thus are not observations of a random variable or statistical estimates as in the POME method. For this reason, we were a little uncomfortable in making decisive statements with regard to the behavior of the NOAH data. We will rephrase this paragraph to make clear our intentions, or if the editors prefer we can remove it altogether.

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.

**Response:** Please refer to response 1

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.

**Response:** We thank the referee for this observation and agree that there is bias in the SCAN data. Here we are merely comparing the bias of the different approaches. We agree that because of scale disparities we cannot make definitive statements relative to true bias. We will revise this section accordingly to acknowledge that bias exists in all three data sets.

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.

**Response:** We agree with this statement to a large extent. We are certainly aware of the uncertainties in using point data to represent larger spatial domains. In fact, this is the reason that we added the gridded NOAH soil moisture estimates as a comparison metric in addition to the point SCAN data. However, in our review of previous studies such as ours we found that it was not uncommon for other authors to use point data as we did (we cited several of these in the manuscript). To counter this, we added the gridded data sets and the TC analysis where the point data are treated as merely another estimate of the true value of the grid soil moisture. We feel that the results of all the analyses are compatible and lead to similar conclusions.

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

**Response:** Please refer to response 1

## References:


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