

## ***Interactive comment on “Estimating root zone soil moisture using near-surface observations from SMOS” by T. W. Ford et al.***

### **Anonymous Referee #3**

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### **Summary**

Deriving root zone soil moisture from satellite is a very interesting topic for HESS readers and an even more general audience. The manuscript is well organized and the results are well discussed too.

The major issue I find about this study is the lack of analysis on the difference between SMOS and ground observations and how such differences can be accounted for. There is only one short section (Section 4) and one figure (Figure 14) devoted to this. All the  $T$  parameters are derived from ground data and almost all the uncertainty analysis is carried out on station data, while systematic differences between SMOS and ground data are expected given the large discrepancy in spatial scales. SMOS data should

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first be thoroughly compared to ground observations in terms of their differences in mean, variability, correlation, autocorrelation (especially this one), and other related characteristics. How these differences would affect the use of the exponential filter should also be investigated. Other authors, especially in data assimilation studies, tend to establish a transfer relationship between the satellite and ground before the satellite data is used as if it's from the ground. So I think the authors should consider adding similar process or otherwise show it's unnecessary.

Given that the title of the article is “estimating . . . from SMOS”, and the authors state that the study “is novel in that we are evaluating the utility of this method for generating root zone soil moisture based on SMOS-derived surface soil moisture”, I truly expect the authors to focus on how things get adapted to the particular nature of SMOS more than how the method works in general on ground stations.

The presentation could be improved too.

I suggest the authors re-submit after performing some significant analysis focusing on SMOS or changing the title/objectives. At least major revisions are needed before the manuscript can be published.

### **Details**

There are too many numbers in the tables, especially in Table 3, 4, 5, and 6. I find it hard to read tables with 100+ numbers. The worse thing is that I don't know the locations of the stations listed in there. Is there any spatial coherence among the correlation/lag time/RMSE/MAE/etc? Or are they more related to other factors like soil characteristics, vegetation properties, annual precipitation, etc? Please plot these numbers on the map (e.g. with color shading) and that will be much more friendly for readers.

Page 8331, line 17: -100 to +100 days range is a very confusing statement for lag correlation between the surface and root zone. It takes no more than a couple of weeks

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for the water to drain down to root zone. So lag cross-correlation beyond certain limit, for example 25 days (depending on the root zone depth), is due to the autocorrelation in soil moisture *itself* instead of the time lag introduced by vertical drainage.

Section 3.1: As just said above, the lag cross-correlation seen between the surface and deep layer is not only due to the lag caused by vertical drainage, but also the autocorrelation in soil moisture itself. I suggest plotting the autocorrelation of surface soil moisture along side the lag cross-correlation in Figure 4 and 5.

Section 3.2: The opposite response of peak lag correlation to annual precipitation is a very interesting finding but not really explained. The annual precipitation explains only 33

Figure 3: It's much better to plot the average water content on the spatial map than binning them by longitude.

Figure 4 and 5: I wouldn't show the negative lag time at all – it carries no significant physical meaning. Fonts are all too small and their size should be at least doubled. What are the two blue lines around zero (x-axis)?

Page 8335, Equation (1): It's better to add a “,” in the subscript of SWI, i.e.  $SWI_{m,n}$  instead of  $SWI_{mn}$ . Explain the “*m*” in the subscript.

Page 8335, Equation (2): should  $K_{t-1}$  be  $K_{n-1}$ ?

Figure 10 and 11: Too many colors are used and some of them are too close (like the light purple and dark purple). Try to use different line types like solid versus dashed. Also, no need for *T* to go too far and 25 days may be sufficient given the reason I mentioned earlier.

Page 8339, line 8-9: which exact version of SMOS soil moisture produce is being used? The sensor itself has a 2-3 days revisit time and this must be an interpolated product.

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Section 4: too little analysis is performed on the difference between SMOS and ground stations. How do the bias/RMSE/etc metrics vary in space, in time, and from 25cm layer to other layers? Is the 8 days the optimal *T* for SMOS? If so (or not), why? Can you show at least a couple of time series of root zone soil moisture from actual obs, predictions by surface obs, and predictions by SMOS? I'd expect this section be a focus of the article but this is really too little here.

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