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Hydrology and Earth System Sciences Discussions

Interactive comment on "A physically based approach for the estimation of root-zone soil moisture from surface measurements" *by* S. Manfreda et al.

Anonymous Referee #2

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Title: A physically based approach for the estimation of root-zone soil moisture from surface measurements

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The paper develops a simple model ("SMAR") that generates estimates of root zone soil moisture from a time series of surface soil moisture estimates. The model is based on the Green-Ampt approach for infiltration and on the assumption of a simple, linear loss function for the second (root zone) layer soil moisture.

The authors demonstrate the model using in situ measurements from three sites in the AMMA region. A continental-scale demonstration is also provided using data from NLDAS. The authors claim that the approach is an improvement over the Wagner et al (1999) exponential filter without and with location-specific calibration of the parameters.

I have serious concerns regarding the conclusions and usefulness of the SMAR model as outlined in my comments below. Moreover, there are errors and omissions in the figures and captions that require second-guessing by the reader. Some of the concerns may therefore result from a misunderstanding, and some may be alleviated through further analysis involving many more in situ measurements, but in the present form the manuscript can definitely not be accepted for publication.

I therefore recommend MAJOR revisions before the manuscript can PERHAPS be published in HESS.

Major comments:

1) I have doubts regarding the usefulness of both the SMAR and the exponential filtering approaches for the stated "applications in the use of satellite remote sensing retrievals of soil moisture" (P 14129, line 5). Specifically, both models perform acceptably ONLY after calibration with location-specific root zone soil moisture data. I do not consider the performance of the SMAR estimates in Fig 6 to be acceptable (particularly in the middle and bottom panels).

Unfortunately, root zone soil moisture data are NOT available for calibration except at a few hundred point locations (mainly in the US, Europe, and Australia) with all the usual caveats of single profile sensing systems. Root zone soil moisture model data (such as from NLDAS) depend on the complex subsurface parameterizations and parameters that the authors are trying to avoid. Using SMAR with NLDAS at best shows that the VIC model can be replicated, it does NOT show that actual root zone soil moisture can be estimated using SMAR.

A location-specific calibration can therefore not be accomplished globally without making additional assumptions.

This need for location-specific calibration has NOT been emphasized nearly enough by the authors. This caveat must be front and center in the manuscript.

Moreover, the authors have NOT demonstrated in this paper that the calibrated SMAR model parameters can be transferred from one location to another. Such a demonstration would involve calibration using root zone soil moisture data from one location and then assessing the skill at a different location for which root zone data are available but have NOT been used in the calibration. The temporally split sample approach used for calibration in the paper is NOT sufficient because it does not demonstrate the global applicability of the approach.

2) The scale discrepancy has not at all been addressed. The AMMA in situ measurements are point-scale data, but the target application is for distributed surface soil moisture retrievals from satellite. At the very least, this scale discrepancy must be discussed more prominently. (Note again that I do not consider the use of NLDAS data to be helpful, see my previous comment.)

3) The authors use only 3 point locations within a single climate regime (Sahel). There are many more in situ time series available (eg., SCAN and Oznet) that should be used to assess the success (or failure) of the SMAR approach across many locations. (Note again that I do not consider the use of NLDAS data to be helpful, see my previous comment.)

4) The authors suggest that the depth of the surface layer should be at least 5-10 cm and proceed to use the in situ measurements at 5 cm depth in their analysis. This is consistent. However, surface soil moisture retrievals from SMOS are representative of a 0-5 cm layer (ie, the equivalent in situ sensor depth would be at 2.5 cm). Arguably, the SMAR model is quite sensitive to the depth of the surface layer. The difference between the in situ data being at 5 cm versus the 0-5 cm sensing depth of SMOS (or

SMAP) has not been discussed sufficiently. Note that the SMAR approach is even more suspect for surface soil moisture retrievals from C- and X-band sensors such as AMSR-E or ASCAT. Some of these concerns could perhaps be alleviated if hourly surface soil moisture data instead of daily data were used, but then the application to satellite soil moisture retrievals (with at best daily repeat cycles) would no longer make sense.

5) Page 14138, lines 20-21: I do NOT agree that Fig 1a suggests that the “assumption of a linear loss function is a reasonable one”. Figure 2 must show the coefficient of determination and it must be mentioned in the text so that the reader can judge for him/herself.

6) Page 14141, lines 19-21 and Page 14142, lines 6-28: The language about the calibration procedure is VERY unclear. It is not obvious from the text that ROOT ZONE soil moisture data are needed for the calibration. For example, Page 14142, lines 26-28: “Calibration is carried out comparing {the time series of the filter values computed using the NLDAS data at 10 cm} and {the time series of the relative saturation in the first 100cm}.” I believe that the second time series refers to NLDAS reference root zone data, but the term “reference” (or observations, or measurements, or NLDAS root zone data) is missing. This must be made much clearer to the reader. Likewise for the calibration using in situ measurements.

7) Figure 8 top left (A) and bottom left (C) panels are almost mirror images in terms of R value. That is, where the performance is good during the calibration phase it is bad during the validation phase, and vice versa. This is does not make much sense. Why should the performance be good during the validation period and much worse during the calibration period?

Minor comments:

Page 14130, line 21: The references provided here are neither comprehensive nor particularly appropriate. Please cite a review paper instead (eg., Seneviratne et al,

Earth-Science Reviews 99, Issues 3–4, May 2010, Pages 125–161).

Page 14139, line 26: NLDAS data are NOT for “the entire North America”, they are only for CONUS and limited border regions in Mexico and Canada.

There are several errors and omissions in Figure labels and captions:

Figure 2: The x-axis label is the same for (A) and (B): “relative saturation”. Does this refer to surface soil moisture in both (A) and (B), to root zone soil moisture in both (A) and (B), or to surface soil moisture in (A) and root zone soil moisture in (B)?

Figure 6 caption refers to a “green line” but probably should refer to the black line.

Figure 6 and Figure 7 captions refer to S100_SMAR but the middle and bottom panels show S130_SMAR and S135_SMAR, resp.

Figure 7 caption does NOT specify which line belongs to the estimates derived from the exponential filter (presumably the red line "SMARxxx*"). Even when viewing the pdf on my large office monitor I cannot make out a difference between the two red lines for the in situ root zone soil moisture and for the exponentially filtered estimates.

Figures 8 and 9 need to be specific about the units of the RMSE. Is the RMSE computed for the relative saturation s? Or for the effective saturation x?

Figures 8 and 9 should use the same colormap and colorbar for all RMSE figures.

Figure 12 caption should refer to the “rainfall arrival rate (lambda) ” (rather than the “rainfall rate”)

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