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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.

**S. Manfreda et al.**

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we would like to thank also the second referee that stimulated a deeper review of the manuscript with his critical review of the paper. We have carefully taken into considerations these critics and we believe the paper has been significantly improved.

Following the same scheme of the review, we provided a point-by-point comment.

RC1) 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.

AC: It is our feeling that the paper has been appreciated, but probably the motivation of the work was not well described. The main task of this paper is not to define an operational approach, but to shed light on the relationship between the surface soil moisture and the root zone soil moisture value. In our opinion, this is of great interest for several scientists that make use of methods such as the exponential filter approach.

The SWI method (exponential method) has been used in several study cases for hydrological forecast, soil moisture retrievals, modelling calibrations, etc. The model is probably not the best we can get, but it has a great advantage that lays in its simplicity. This is the reason why hundreds of researches around the world use it.

The exponential filter proposed by Wagner et al. (1999) was derived by the following differential equation

$\frac{s_2}{dt} = \frac{C}{nZr}(s_1 - s_2) = \frac{1}{T}(s_1 - s_2)$

where $T = nZr/C$ is a time characteristic constant and $C$ is a pseudo-diffusion parameter (see Wagner et al., 1999).

From our point of view, there is a limitation in the use of this method that is related to the calibration of $T$. This parameter can only be estimated with soil moisture data, but what if this data is not available. What would be the best performing parameter? What are the controlling factors for this parameter? This are the driving questions that we try to tackle somehow.

This paper addresses the need of comprehension toward these simplified approaches. Defining a new simplified scheme that probably is more complex of the previous, but that provides an analytical description of the relationship between the two variables of interest using parameters that have a clearer physical interpretation.

We agree that some additional experiments are needed and for this reason the present paper may benefit by the inclusion of some additional in-situ data. Following referee suggestion, we included in the revised version of the paper new point measurements taken from the AMMA network.

Regarding the use of NLDAS dataset, we believe that this data was useful to understand the limitation the SMAR method.

RC 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.)

AC: This is an interesting point that has been addressed in the paper discussion. More-



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over, we have also included some additional analyses carried out with soil moisture measurements averaged over 25x25 $km^2$ in the section of the model application.

RC 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.)

AC: Following referee suggestions, the number of in-situ stations considered has been increased using additional station from the AMMA database. Regarding, the use of NLDAS dataset, we still believe that this application was useful to understand the limitation for both the exponential filter as well as SMAR.

RC 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.

AC: This point is similar to one of the point raised by the first referee and consequently our answer is the same. We do not think that this may represent a limitation for the methodology. A thickness of less of 5cm will imply numerical problems in several algorithms. For this reason, most of the hydrological models, that incorporates a surface layer in the soil description, assume a thickness of about 10cm. It is absolutely true that most of the sensors cannot penetrate deeper than few centimetres, but it is a reasonable assumption that this measures can be representative of the dynamics of a surface layer of approximately 5-10cm.

RC 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.

AC: It is true there is a significant scatter and the linear trend is clear only in the surface layer (Fig.1A) and for the lower values of the relative saturation. The cloudiness observed may be due to seasonality, non-measured rainfall and other sources of error. The text has been modified.

RC 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.

AC: The description has been rewritten and simplified in order to improve readability of these paragraphs.

RC 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



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during the calibration period?

AC: It is true that there are portion of the domain where the calibration period has lower correlation respect to the validation, but this may always happen. In general, we observe a significant reduction of the mean values of R during the validation period and this is consistent.

Minor comments: RC 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).

AC: The reference to Seneviratne et al. (2010) have been included in the intro.

RC 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.

AC: This correction has been incorporated specifying that the administrative borders of Mexico and Canada limit the region.

RC 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)?

AC: The caption has been modified specifying that the two panels refer to the surface and the root zone soil moisture.

RC Figure 6 caption refers to a "green line" but probably should refer to the black line.

AC: Thanks; it is a typing error that has been fixed.

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

AC: Thanks; it was a typing error that has been fixed.

RC 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.

AC: On each graph, we reported a legend with the description of the graphical convention adopted. Nevertheless, it is true that the function describing the result of the exponential filter RMSE is not clearly distinguishable and for this reason has been modified in all graphs.

RC 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?

AC: RMSE is computed for the relative saturation. Units have been included in the new version of the paper.

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

AC: In the new version of the paper we incorporated the same colorbar in both Fig. 8 and 9.

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

AC: Caption have been modified.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 14129, 2012.