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Hydrology and Earth System Sciences Discussions

Interactive comment on "From near-surface to root-zone soil moisture using an exponential filter: an assessment of the method based on in-situ observations and model simulations" *by* C. Albergel et al.

Anonymous Referee #1

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Title: From near-surface to root-zone soil moisture using an exponential filter: an assessment of the method based on in-situ observations and model simulations

Authors: C. Albergel et al.

Summary of the paper:

Point soil moisture measurements at 12 sites within the SMOSMANIA network and at the SMOSREX site in Southern France are studied to convert surface soil moisture to root-zone soil moisture by a smoother. A characteristic time length parameter (T) in this smoother, which is an exponential filter, is optimized based on the Nash-Sutcliffe measure between the resulting soil water indices (SWI, calculated based on the surface soil moisture) and the observed root-zone SWI. Next, synthetic (simulated by the ISBA land surface model as part of SIM) soil moisture profiles at 8 km resolution over France are used to study the link between T and region specific soil and climate characteristics.

Recommendation:

This paper is relatively well written, but is quite lengthy and contains no real new insights. It should be revised, mainly to increase its relevance.

Major Revisions

MAIN ISSUES:

What is the (dis)advantage of this exponential filter compared to other techniques for low pass filtering? Why even consider studying this filter and its single parameter T, if it is known in advance that the filter is not very sensitive to T (Wagner 99)? The authors find a little sensitivity as well, so how useful is this study? How would this filter compare to other simple techniques like CDF-matching or a simple linear (or higher order) relationship between surface soil moisture and root-zone soil moisture? The latter techniques allow to 'fully' convert the surface soil moisture climatology to the root-zone climatology, while the exponential filter only smooths the surface observations (i.e. changes the temporal variability) without changing the absolute level of estimated soil moisture.

What is the practical use of a SWI-value [0,1] when obtained from satellite data and given that no other information on the deeper soil is available?

It is not very clear which variables are compared or analysed: are the SWI results scaled to [0,1] and are all soil moisture observations also scaled to [0,1], i.e. SWI_{obs}? This is not explained in the paper and units are missing. Note that an indication of 'no units' [-] versus something like [m3/m3] for soil moisture can be very helpful already to

distinguish between the different soil moisture related variables: scaled info [-] would fall in [0,1], while soil moisture vol% or m3/m3 would be bounded by physical minimum and maximum soil water content.
Just a thought: I have some doubts about using a RMSE and Nash-Sutcliffe measure for comparing scaled time series. Both measures include some notion on the bias between time series, while that bias seems to be artificially removed in this study. A simple comparison (difference) of the correlation length and the temporal variance in the time series might be more justified. Also, through scaling, the variability is altered. Since I am not entirely sure about the actual applied operation of scaling or normalization of the observations, it is hard to know if the validation measures are justified.

It is not justified to make a general statement that T would be linked with climate effects, based on entirely synthetic profile simulations. The finding tells something about the model physics, NOT about nature (how well is the LSM calibrated/validated?). Also for the relation between T and the soil depth, it should be recognized that T is smaller for the simulated profiles at the coarse scale than for observed ones at the point scale, because the correlation between modelled profile layers (definitely at the coarse 8 km resolution) is generally larger than reality (here at a point scale). Both the issue of the scale effect and the fact that the link of T with soil depth and climate is more a model-related conclusion should be more stressed on. Is there any chance to average some point profile observations to a coarser scale and compare those profiles to simulated ones (at that same coarse resolution)?

I do not agree with (the reference to) Stroud (1999) on the idea that the exponential filter resembles the Kalman filter and I would rather like to see any reference to Kalman in the paper under review being removed, because it gives a false and confusing impression on the actual paper contents. The exponential filter in this study has no specific feature of the Kalman filter at all, the only common feature is the shape of its update (filter, recursive) equation, but that is something that all recursive linear filters have in common! The proposed exponential filter is simply a low pass smoother, nothing else.

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It is a smoother in a recursive formulation.

The technique is proposed for use with RS data. However, it requires time series of **available** data over a relatively limited time window (to allow smoothing over that window): how realistic is that for France? I assume that small scale radar data will be cancelled out often, because of precipitation events? What RS data were you thinking of?

TEXT-SPECIFIC COMMENTS:

ABSTRACT

Even though intuitively it might seem right, it is not justified to make a general statement that T would be linked with climate effects, based on entirely synthetic profile simulations. The finding tells something about the model physics, NOT about nature (the model is not truly calibrated/validated).

INTRODUCTION

Line 9-14: Why would the lack of info on model parameters at global scale and uncertainties related to the physical description of the water and energy balance be a disadvantage to 1DVar or other assimilation techniques other than the Kalman filter? For the issues you might have in mind, 1DVar, Kalman and other filters are essentially identical.

Line 12: ’Moreover, the analysed profile soil moisture is model-dependent’. Therefore, the authors choose to use a ’method which solely relies on remotely sensed soil moisture’. It should be recognised that each conversion from surface soil moisture to some other soil moisture is done through some operator, i.e. a model: consequently, the results from the exponential filter are also ’model’-dependent and are thus not solely relying on remotely sensed data, but also -as is extensively discussed later- on the structure and parameters (e.g. T) of the transformation model, i.e. the exponential filter.

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MATERIALS AND METHODS

Suggestion: I would change 'materials' in the title to something like 'data'.

2.3 SIM

- 'ISBA parameters were aggregated to 8 km' probably, it is meant that the ECO-CLIMAP parameters are aggregated to 8 km and the ISBA model simulations were performed at this resolution?
- Line 25: SWI is compared to w2: do both variables end up with the same units? Probably, the comparison is performed after scaling? Please, do mention that in the text.

2.4 EXPONENTIAL FILTER

- line 15: 'convoluting the surface soil moisture time series with a filter': this formulation is strange. The filter performs a convolution with some exponential function.
- Eq.2-3: in Eq.2 the integration is up to time t, while in Eq. 3 tn is introduced. I think that tn in Eq. 3 can be simply t or vice versa for clarity. In any case: tn is not explained in the text.
- Eq. 3: ms(ti) is normalised: do explicitly state how that is done. Do you mean it is scaled to [0,1]? That is different from subtracting the mean and dividing by the variance (=classical normalization).
- Line 17: Pellarin uses the interval [tn-3T, tn], but Wagner uses [tn-5T, tn]: what is the criterion behind the 3 or 5?
- Line 21: RMSE of 0.0022 m3/m3 by comparing SWI with root-zone soil moisture: here the RMSE is expressed in m3/m3, so probably no scaled SWI is used (I did not check the referred paper). I am confused about the units used for SWI, please clarify somewhere.

2.5 RECURSIVE FORMULATION

I do not agree with Stroud (1999) on the idea that the exponential filter resembles

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Discussion Paper

the Kalman filter and I would rather like to see any reference to Kalman in the paper under review being removed for clarity. The exponential filter in this study has no specific feature of the Kalman filter at all, the only common feature is the shape of its update (filter, recursive) equation, but that is something that all linear filters have in common! The applied filter shows more resemblance to something like nudging, it has no reference to dynamical updating of states and uncertainties. Even referring to nudging is wrong: this exponential filter is simply a low pass smoother, nothing more, nothing less.

p.1613
- line 12: the 'original' exponential filter: that is Eq. 3? Please do refer to that different expression and line 17: 'it was checked' is redundant, because both expressions should be mathematically equal.

APPLICATION OF THE EXPONENTIAL FILTER

3.1 STATISTICAL SCORES
- Eq. 7: variable p is not explained
- SWI_{obs} is never really explained. Again: what are the units, are they scaled [0,1] observed soil moisture?

3.2 SMOSMANIA, 3.3 SMOSREX
- Why would you choose 30 cm soil moisture, while you can as well calculate the actual root zone soil moisture? The reasoning of the high correlation with the other layers is not really convincing: if root zone soil moisture is available as well, then there is no justification for taking a proxy. Furthermore, for the SMOSREX experiment, all different soil layers and the integrated profile were studied.

ANALYSIS OF THE RESULTS

4.1 PERFORMANCE OF THE EXPONENTIAL FILTER
Indicate the period of validation for each case.

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4.1.2. SMOSREX

- inter-annual variability of T: does that show some non-stationarity in the error which is filtered?
- Fig.6, line 14-15: text description is confusing: in the figure, scaled observations are shown. How are the 'better results' obtained? Through which 'normalization'? Again: what is meant by normalization of the observations? Also, through scaling, the variability is altered. Fig.6 suggests that the filter still passes too much high frequency information. Would another low pass filter (with a more narrow band) give better results? See major comments: would any other filter or operator work equally well or better?

4.2. IMPACT OF SOIL DEPTH

- Figure 9 a and b: a and b are not marked
- Line 19: 'with the depth of the observation' -> 'with the depth of the validating root-zone observation'
- Figure 9:

+ what is the considered time period for these graphs? Mention this in the text/caption.
+ could you add a line for a single SIM pixel where the SMOSREX site is situated in the left figure, just to see how the SIM and SMOSREX compare (but do stress the scale difference!) or how well SIM performs.
+ do add error bars to the right hand plot (like in fig 11)
+ for SIM the averaged T values are consistently lower than for the single SMOSREX site. This could be because the SMOSREX site is only a single point, but it should also be recognized that these are simulated profiles and that modelled profiles typically show much more correlation between soil layers (even more when they are averaged over 8x8 km2) than actual natural observed point profiles. I suspect that the T values obtained for the SIM data set give more information about the dynamics of the used LSM than about the actual nature. How well are the SIM simulations calibrated/validated?

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DISCUSSION

The technique is proposed for use with RS data. However, it requires time series of available data over a relatively limited time window (to allow smoothing over that window): how realistic is that for France? I assume that small scale radar data will be cancelled out often, because of precipitation events? What RS were you thinking of?

- Line 14: reference to fig 10 should be fig 11
- Line 18-20: sentence construction

CONCLUSIONS

Last sentence: 'data poor areas': you mean poor in ground measurements, but not poor in RS data. For this filtering technique, several time steps of observations are required (see above).

TABLES

Table 3: Indicate the time period and add units (either [-] or [m3/m3], so that at least we can have an idea of the range of the actual compared values, i.e. [0,1] or [minimum water content, maximum possible water content]). Formulation is not very clear: a first reading gives the impression that the filter was applied at 30 cm, while it is not.

FIGURES

Figures 2-3-4: vertical axis labelling: sometimes only units are given, sometimes the name of the variable, please be consistent (give both for all plots).

Figure 5: axis labelling: 'Nash'; is a little strange, please give the measure a proper name. The legend is hard to link to the symbols when they are overlapping. Please, do provide a regular legend with symbols and names listed outside the plot.

Figure 8: label the axes. Please indicate somewhere how the observed SWI is scaled.

Figure 10 can be removed and the discussion limited.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 1603, 2008.

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