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Hydrology and Earth System Sciences Discussions

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Interactive Comment

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.

C. Albergel et al.

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The authors thank the anonymous referee #2 for his in-depth review of the manuscript and for his comments. For an easier comprehension, general comments of the referee are also reported (2.XX).

2.1 [Given the historical success of the Force-Restore models (of which the exponential filter is only a gross simplification), one can expect that a recursive filter will work reasonably well to retrieve a faw index. But to be "substantial";, this paper should investigate how efficient this method is 1- compared to simple SVAT models (such as

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Discussion Paper

the ISBA Force-Restore scheme) and 2- to retrieve a soil moisture value instead of an index.]

Response 2.1

The reviewer is correct to say that the exponential filter is a gross simplification, if we consider that the lack of any physical formulation and its purely empirical parameterisation can be seen as a simplification. Also, Eq. (1) differs in one important aspect from the force-restore equations. In Eq. (1) w2 is driven solely by changes in wg, whereas the force-restore method used (e.g.) in ISBA is bi-directional water balance model where wg is driven by w2, but also vice versa. The performance of the exponential filter and of the ISBA model is similar for the SMOSREX site. Sabater et al. (2007) give the Nash-Sutcliffe score on w2 of a force-restore version of ISBA, driven by the atmospheric variables observed in-situ. For the 2001 to 2004 annual cycles, they obtained N values of 0.82, 0.92, 0.83, 0.51, respectively. In this study (Table 3) the N values for the SWI are 0.51, 0.85, 0.83, 0.88, respectively, which shows that very similar results may well be retrieved. Root-zone soil moisture values (in units of m3m-3) depend to a large extent on the soil characteristics (soil texture, porosity, organic matter content, etc.). The rationale for retrieving a SWI, instead of root-zone soil moisture values is that the spatial resolution of soil moisture products from microwave radiometer and scatterometer systems is coarse (25-50 km). Over a large footprint, the variability of soil characteristics may be very high and may not be represented accurately. In this context, only the relative dynamic range of the soil water content can be represented. Another reason for retrieving SWI values is that root-zone soil moisture is model-dependent. Data assimilation methods are based on unbiased observations. In the case of soil moisture, this is tantamount to using SWI values.

2.2 [The "material and methods"; part is rather lengthy and there is a constant confusion between soil moisture (from 0 to saturation, in m3/m3) and a fraction of available water (no dimension, from 0 to 1); this misunderstanding should be solved in the paper, esp. in the Figures 2 to 6.]

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Response 2.2

In this study, the SMOSMANIA data are published for the first time, which warrants its length as we cannot refer to previously published material. Yes, we agree that the different soil moisture definitions may be confusing. wg is the water content of a surface soil layer [m3/m3], 5cm for SMOSMANIA stations, 0-6cm for SMOSREX, a few mm for SIM. w2 is the root-zone soil moisture content [m3/m3], measured at 30cm (SMOSMANIA), at various depths (SMOSREX), or integrated over the root-zone profile (SIM and SMOSREX). Prior to filtering, soil moisture wg observations or simulations are scaled between [0,1] using maximum and minimum values of each time series (ms, dimensionless). The dimensionless SWIobs used in Eq.(7) to assess the quality of the results is the reference w2 (either observed in situ or simulated by SIM) scaled to [0,1] using maximum and minimum values of each time series. SWIm is the result of the exponential filter. It is dimensionless and comprised between 0 and 1.

2.3 [The authors claim that the climatic conditions are the main source of variation for Topt but this is not justified in the paper; it's a pity because Topt is derived from a synthetic dataset for which climate conditions (intensity/duration of interstorm/storm periods) are fully known.]

Response 2.3

Indeed, it is difficult to find a climate effect on T in our simulations. The Rhône valley example is a rather extreme case. The Rhône gradient found on T from model simulations only suggests that a climate factor may exist. Further investigation is needed to consolidate this result.

2.4 [Does the title clearly reflect the contents of the paper? No, because the paper is always looking at faw (normalized) values, never at soil moisture.]

Response 2.4

Soil moisture and scaled soil moisture values are considered in this study.

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2.5 [Is the overall presentation well structured and clear? No (materials and methods, results, discussion sounds very old fashion and no transition exists between paragraphs; the paper should be better structured to provide a guideline from intro to conclusion)]

Response 2.5

In this study, several data sets are used, resulting in a rather complex article. In order to facilitate the understanding, we preferred using a simple, classical organisation of the paper. The other reviewers are, overall, happy with the organisation of the paper.

2.6 [Is the language fluent and precise? No (please delete some lengthy sentences and improve the overall English)]

Response 2.6

Additional efforts will be made for the final version of the paper.

2.7 [Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? No (frequent confusion between soil moisture content and SWI index)]

Response 2.7

Yes, we agree that the different soil moisture definitions may be confusing (see response 2.2).

2.8 [Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? The materials and methods section is too long and the context of the SMOSMANIA and SMOSREX experiment should be reduced to what is necessary for the paper. The discussion section is too short and sounds more like a conclusion than a discussion. It should be either extended or combined with the conclusion section.]

Response 2.8

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In this study, the SMOSMANIA data are published for the first time. They have to be described, as there is no previously published material available to reference to. This explains why section 2 is rather lengthy. SMOSREX is presented in 12 lines. It cannot be shorter. Elements from the current interactive discussion may help improve the discussion section.

2.9 [Many findings are not surprising (yes, one expects that Topt increases with the depth for which the observation is made, and, yes, one expects that climatic factors are at least of the same importance to explain the filter behaviour and the retrieved Topt than the soil properties.]

Response 2.9

The conclusion section has to be improved in order to highlight the findings of this study.

2.10 [Some findings are insufficiently commented; for instance, the few (why not analysing wilting point and field capacity ?) textural data available at the SMOSMANIA site show that the sand fraction has a large impact on the retrieved Topt and the accuracy of the filter (the more sandy stations coincide, as expected, with the lowest Tops values).]

Response 2.10

Wilting point and field capacity were not measured. The more sandy stations coincide with the lowest performance of the filter, not with the lowest Topt values.

2.11 [It is very convenient to normalize soil moisture and analyse normalized results only; but many fields of application of this work requires an estimate of the soil moisture, not an estimate of how wetter or how drier the soil is (the "fraction of available water"). This reduces considerably the interest of the work and should be at least briefly commented.]

Response 2.11

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A scaled SWI does allow to combine the different dynamic ranges of surface soil moisture and profile soil moisture. This is essential, as the surface may well show soil moisture values below the wilting point and above field capacity, while the profile soil moisture is generally bound by those two parameters. It is also possible that different soil horizons exist throughout the soil profile. Using a scaled value does alleviate this problem, assuming that the hydrologic response is the same in all soil types. We agree that certain application do require correct absolute estimates of soil moisture, though this paper does not want to provide this detail, as it is also a function of the knowledge of the soil types within a satellite footprint and its subpixel variability. Those are issues that have to be solved at a later stage.

2.12 [Why not performing a complete comparison between the filter and a classical Data Assimilation procedure in a SVAT model ? For SMOS, it seems to me that ECMWF prediction/ analyses will be used to derive higher level products, why not run ISBA instead of a filter ? This should be discussed.]

Response 2.12

A comparison of the exponential filter and a data assimilation procedure is beyond the scope of this study.

2.13 [Why not performing a classical split-sample analysis ? The interannual stability/robustness of the filter and the retrieved Topt should be analyzed.]

Response 2.13

The results presented in Table 3 permit to analyse the interannual stability/robustness of the filter. The Topt derived for the whole data set (6 days) is equal to the Topt of 2006 and 2007. Therefore, the Table 3 comparison is equivalent to optimising T for a subset of the time series (2006-2007) and then apply this value to the whole data set (i.e. a split-sample analysis).

2.14 [The synthetic data set is produced by the force-restore scheme that de-facto

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assumes a complete coupling between the surface and the subsurface moisture evolution. Is there any evidence of decoupling in the observed (SMOSREX/MANIA) dataset? (decoupling could explain the bad performances of the sandy sites I suppose).]

Response 2.14

The force-restore scheme, as implemented in the ISBA model, is able to simulate the decoupling between the surface and the subsurface soil moisture evolution (Calvet and Noilhan 2000). For the SMOSMANIA stations, the lowest correlation ($r2 < 0.5$) between surface soil moisture (at 5cm) and the root-zone soil moisture (at 30cm) is obtained for the LHS, MTM, and LZC stations (not the more sandy sites).

2.15 [More importantly, and this justifies my recommendation, the impact of the climate on the retrieved Topt should be fully analysed; the authors can use easily accessible indicators relevant to the study such as intensity/duration parameters of the storm/interstorm periods to do so (max. rainfall intensity, length of the interval between two rainfall events, max. potential evaporation, average ratio between annual rainfall and annual potential evapotranspiration etc).]

Response 2.15

It is difficult to find a climate effect on T in our simulations. The Rhône valley example is a rather extreme case. The Rhône gradient found on T from model simulations only suggests that a climate factor may exist. Further investigation is needed to consolidate this result. A very detailed analysis of the climate effect is beyond the scope of this study.

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

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