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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 #2

Received and published: 5 August 2008

Summary:

This paper investigates the utility of an exponential filter for retrieving the fraction of available water (faw) in the root zone from successive observations of surface soil moisture along an observation transect in the south-west of France. A map of modelled decay parameter (T) is also shown for continental France. This parameter is derived from modelled surface and root-zone soil moisture through the modelling package SIM. No clear relationship between this parameter and the soil textural properties is found for the later synthetic dataset.

1) Does the paper address relevant scientific questions within the scope of HESS?

Yes

2) Does the paper present novel concepts, ideas, tools, or data?

Also the relative efficiency of the exponential filter to provide a moisture index has been already shown in works by Wagner, spatial pattern of T from synthetic data at continental scale and the use of the SMOSMANIA network is new and could be useful for higher level products of missions like SMOS.

3) Are substantial conclusions reached?

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

4) Are the scientific methods and assumptions valid and clearly outlined?

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.

5) Are the results sufficient to support the interpretations and conclusions?

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.

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

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Interactive Comment

Not for soil moisture (lack of information on normalization process)

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

I think so

8) 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.

9) Does the abstract provide a concise and complete summary?

Yes

10) 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)

11) Is the language fluent and precise?

No (please delete some lengthy sentences and improve the overall English)

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

No (frequent confusion between soil moisture content and SWI index)

13) 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

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should be either extended or combined with the conclusion section.

14) Are the number and quality of references appropriate?

Yes (also one would expect some references on "surface/deep moisture decoupling")

15) Is the amount and quality of supplementary material appropriate?

Most of the figures can not be read (see below)

General comments:

This paper is interesting but leaves the reader a little frustrated;

1- 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's behaviour and the retrieved Topt than the soil properties.

2- 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).

3- 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.

4- 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.

5- Why not performing a classical split-sample analysis ? The interannual stabil-

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ity/robustness of the filter and the retrieved Topt should be analyzed.

6- The synthetic data set is produced by the force-restore scheme that de-facto 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).

7- 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, lenght of the interval between two rainfall events, max. potential evaporation, average ratio between annual rainfall and annual potential evapotranspiration etc).

Recommendation:

Major revisions (please pay particular attention to points 3, 4 and 7 above)

Specific comments:

P.1605: provide SMOS spatial resolution.

P.1606; L.19: "may also be" $>$ is also

P.1607; L.6: the objectives of the paper are not clearly indicated.

P.1607; L.7: "materials and methods": this part is a hotch potch and is not really organized; it should be better structured; some information about SMOSMANIA and SMOSREX are not necessary.

P.1609; L. 14: what is the instrument used to measure soil moisture ?

P.1612; L. 13: SPECIFY HOW SOIL MOISTURE IS NORMALIZED (cf. P. 1618; L. 14).

P.1612; L. 20: it does not make sense to compare SWI with soil moisture.

P.1612; L. 23: a transition is lacking; why do we need a recursive formulation ?

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P.1614: part 3.1 is too long;

P.1614; L.17: "when the observed mean is a better predicator than the model" is not clear; it should be replaced by "if the observed mean is a better estimate than the modelled output" or an equivalent formulation.

P. 1615; L. 10: "of the latter": the later what ? P. 1616; L 10: it could be interesting to show the bias as well (Tables 2 and 3), cf. L. 24 "biased retrieval".

P. 1617; L. 9: in theory, the average of the product of faw and the dynamic range is different from the product of the respective averages.

P. 1619: "performs well": why this N=0.86 cut-off value ?

P 1619; L. 19: "correlated with" $>$ "as expected, increases with" (moreover, one expects such a result, it is a bit strange to read that as if one would be surprised !).

P. 1621; L. 13: no fact supports this finding.

Fig. 2 and 3: Y axis is "SOIL MOISTURE, in m3/m3".

Fig. 4 and 6: Y axis is "Fraction of available water [-]".

Fig. 4 and 6: large and small dots cannot be clearly separated.

Fig. 5: station names cannot be clearly attributed to symbols.

Fig. 7: legend ? Maps are too small.

Fig. 8: how this pixel has been chosen ?

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