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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Depth scaling of soil moisture content from surface to profile: multistation testing of observation operators" *by* Xiaodong Gao et al.

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The manuscript presents a statistic approach to derive Root-zone soil moisture (0-100 cm) from near-surface soil moisture measurements (5cm). This is an important and current topic since remote sensing techniques devoted to measure soil moisture are only sensitive to the top 5 cm soil moisture and most of applications (agriculture, meteorology, hydrology) are interested in deeper soil moisture estimates. The proposed methodology consists in using a CDF-matching procedure applied to the 5 cm soil moisture data to predict 0-100 soil moisture dynamic. To my knowledge, this has not been tested in previous studies. Authors address the problem of three issues: the calibration period-length, the temporal sample, and the climatic effect on the surface/deep soil moisture link. Dataset comes from the US SCAN network. Although the subject is interesting and deserves publication in HESS, the paper presents some deficiencies. For this reason, I would recommend major revisions prior to potential publication in HESS.

General comments:

The main problem of the proposed methodology rely on the fact that it can't be used in ungauged regions (as state by authors in conclusions). In my opinion, this is not totally true and the use of the SCAN database is probably underutilized in this study. There is no interest to use a method which can be used only in regions where we have the truth. However, the paper can be much more improved if a last paragraph is added to discuss about the way to derive the 5 required fifth-order polynomial coefficients from climatic/soil/vegetation characteristics related to other 190 SCAN sites. Depending on the results, this publication can be useful to explain that this solution is probably (or not) the right direction to search.

The effect of soil freezing is also not addressed in the paper while there is a clear signal in humid continental pixels (Fig.6). Sudden decreases of the surface soil moisture (5cm) in winter periods (around 1/1 and 3/1 in Centralia Lake and Molly Caren for instance) are probably occurring only at the surface and not in deeper layers. This water doesn't disappear contrary to sudden decrease during summer periods. Consequently, the CDF model generate too strong decrease in winter. This point has to be discussed in the paper.

It is necessary to use a coherent formulation for the different soil moisture profile: in-situ soil moisture profile (sometime called "Profile", or "ThetaP"), the CDF profile ("ThetaPˆ" or "Profile (CDF)"), as well as the exponential filter ("wpˆ" or "profile(SWI)", I would suggest Profile(EF)).

Specific comments

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1) US-SCAN network consists of over 200 soil moisture station across US but only 12 of them were used without any explanation of that choice (p.3 line 29). Authors should clarify this point.

2) "The outliers were then excluded from the analyses" (p.4 line 5). Does this mean that the station is excluded from the analysis or only the period concerned with outliers?

3) P.6 line 26. The term "reference" is somewhat confusing. Actually, the exponential filter is not used as a reference method to judge the performance of observation operators but as an alternative method. The reference dataset is in-situ soil moisture. Same remark p.9 line 2 and p. 12, line 2.

4) P.7 lines 15-20. I do agree that exponential filter (EF) provides index values (between 0 and 1) and need to be rescaled using w2max and w2min values. But this solution only required 2 parameters whereas the CDF matching procedure required a large amount of 5 cm soil moisture measurements to derive the 5 coefficient of the fifth-order polynomial procedure. The sentence seems to indicate that these 2 coefficients are a strong limitation of the EF method. Authors should reformulate this paragraph.

5) I don't think Eq. 12 is required as Eq. 11 is supposed to provide a 0-100cm soil moisture content. I expect results of each equation to be quite similar (L2(=100)»L1(=5)).

6) Section 3.2.1. It is not clear how the three correlation coefficients (0.92, 0.75 and 0.59) can be observed in Fig. 4. Please clarify

7) P.10 lines 18-19 and Figure 9. I can understand why the bias related to the exponential filter method is not equal to 0 since the 0-1 time-serie is scaled using the min and max value. However, looking at Fig. 8 (top-middle graph for instance), the green curve does not seem to be scaled to the black one. Therefore, I expect the bias (Fig. 9) to be slightly overestimated.

8) P.11 line 10. I do not agree with the sentence "that only surface (5 cm) soil moisture is needed as the input". Actually, the method also requires deeper layer soil moisture

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measurements (up to 100 cm) to derive fifth-order polynomial coefficient of the CDF procedure. Please reconsider this sentence.

Technical corrections

1) Table 2: Authors should indicate in the legend which are the time-serie compared in this Table. Idem in Fig.3

2) Fig. 4, indicate that the 3 top graphs correspond to "humid continental" pixels, and idem for middle and bottom graphs.

3) Idem Fig.4 remark

4) Fig.9: Authors should better explain the meaning of "a", "b" and "ab"

5) P.5 line 17: a word is missing after "then". Probably "used" ?

6) P10. Line 30. A work is missing after "clearly". Maybe "shows"

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