Hydrol. Earth Syst. Sci. Discuss., 9, C6240–C6244, 2013
www.hydrol-earth-syst-sci-discuss.net/9/C6240/2013/

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.

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Received and published: 11 January 2013

With the increasingly availability of satellite derived surface soil moisture data the question of how to accurately estimate the soil moisture content in deeper layers from these satellite measurements becomes increasingly important. Currently, one of the most widely used methods to achieve this goals is the exponential filter method – also referred to as Soil Water Index (SWI) method – which my co-authors and I introduced in 1999 as a practical means to compare ERS Scatterometer soil moisture retrievals to in situ soil moisture data over the Ukraine (Wagner et al. 1999). The theoretical

basis of the method was discussed in more detail in Ceballos et al. (2005); Albergel et al. (2008) introduced an iterative formulation of this algorithm. Many validation studies have demonstrated that the SWI method performs in general surprisingly well. This, together with the ease of its implementation, has most likely contributed to its popularity. Yet one disadvantage of this method has so far been that its link to more physically based infiltration models has not been clear. This problem has been successfully addressed by Manfreda et al. in their study, which I regard as a very important contribution to our field. Therefore I recommend the paper for publication after some major revisions.

Starting from the idea that infiltration from the top soil layer to lower ones only occur when the soil moisture content exceeds field capacity, Manfreda et al. develop a new model termed Soil Moisture Analytical Relationship (SMAR) that can be considered a generalisation of the SWI method. Rather than having three model parameters (T, minimum and maximum soil moisture content) that need to be calibrated to optimise the fit of the SWI derived profile soil moisture estimates to independent reference data, the SMAR method uses four (sw, sc1, a, b), where for three of them a certain degree of correspondence is given: The SMAR parameters sw and sc1 may be regarded as corresponding to the minimum and maximum soil moisture values used to convert the SWI values to absolute soil moisture data, and the new parameter a corresponds to the characteristic time length T of the SWI method.

The SMAR model is theoretically attractive because all four parameters are physically reasonably well defined. I only use the relatively weak phrase "reasonably well defined" because one of the four SMAR parameters, a, is mostly depended on the soil water loss coefficient V2 which shall account for both evapotranspiration and percolation losses. These losses can be estimated but still, one will have to accept using simplifying assumptions and approximations. Please note that evaporation is also one of the main drivers of the SWI model parameter T (Ceballos et al. 2005; Delworth and Manabe 1988). Despite the theoretical advantages of the SMAR over the SWI model it remains to be seen whether SMAR can perform better than the SWI in practical applications without extra calibration effort. The experiments carried out by Manfreda et al. in this work suggests that this is the case, but using only data from three AMMA in situ sites situated in West Africa and a model implemented over North America. I recommend that the authors use additional in situ data from temperate regions to substantiate their conclusion that “the method performed better than a traditional low pass filter”. From my experience using data from in situ networks situated in temperate climates (Europe, North America) it will be more difficult to improve the SWI method than was the case for the in situ data from West Africa.

SPECIFIC COMMENTS

Page 14131, Lines 15-19: It is presented as a disadvantage of the SWI method that Albergel et al. (2008) could not determine a significant relationship between the T value and soil and climate conditions over France. But considering the physical meaning of T, as discussed by Ceballos et al. (2005), another possibility may simply be that this reflects the temporal scaling properties of soil moisture. Please also see the paper by De Lange et al. (2008) who studied the link of T to soil texture.

Page 14133, Lines 18: Remote sensing sees a much shallower layer as the mentioned 5-10 centimeters. Please discuss if this constitutes a problem for our approach.

Page 14133, Line 4: Subscripts the formula for delta should not be "i" and "j" as "j" characterises the end of the integration interval. I recommend the use "i" and "i-1"

Page 14135, Line 15: “The last value . . .” should really be the parameter a (and not b as such referred to).

Page 14137; line 7: Also provide a reference to a journal paper, e.g. to Dorigo et al. (2011)

Page 14138; line 20: I do not “immediately realize” that Figure 2 justifies a linear loss function (in fact the scatter plots is highly non-linear).

Page 14140, Lines 22-25: From our experience estimating wilting point or other soil hydrologic properties from different in situ networks is not straight forward. The exact answers will depend on the set up of the network and the used measurement technology.

Page 14141, Line 16: According to me Fig. 6 does not illustrate a “good performance”. The correlations are good, but the behavior during the peak of the rainfall season, and particularly at the beginning of the dry season is not very satisfying.

Page 14143, Lines 15-17: I do not understand this sentence

Page 14146, line 5: “two” instead of “to”

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