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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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First of all, we would like to thank Wolfgang Wagner for his constructive review and encouraging comments. Obviously, considering that the present work is inspired by his previous work it was a great opportunity for us to have his personal comments on the paper that we greatly appreciate.

We agree with him on the general comment provided and we would like to underline that the main task of this paper is not to define an operational approach used in place of other systems, but to shed light on a phenomenon of general interest. The approach

proposed here represents an attempt to provide a physically interpretation of the relationship between the surface soil moisture and the root zone soil moisture value.

It is necessary to underline that the SWI method has been cited in more than 270 papers and have been used in hundreds of experiments. This provides a huge number of applications and a significant experience developed on this methodology. Considering this as a starting point, it is clear that we cannot provide a full and comprehensive comparison between these two methodologies in just one paper. We agree that some additional experiments are needed and this will be a subject of future work aimed at the identification of new sites and the understanding of the potential of the present method or the definition of new strategies to improve it. Nevertheless, we believe that the present paper may benefit by the inclusion of some additional in-situ data. For this reason, we included in the revised version of the paper new point measurements taken from the AMMA network.

Coming to the more specific comments, we have commented these points one by one:

RC: 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.

AC: Thanks for this suggestion. The paragraph have been modified including the reference to Ceballos et al. (2005) and De Lange et al. (2008).

RC: 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.

AC: We do not think that this may represent a limitation for the methodology. A thickness of less of 5cm will imply numerical problem in several algorithms and for this rea-

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son most of the hydrological models that incorporated a surface layer in the simulation that is generally of about 10cm. It is absolutely true that remote sensing cannot penetrate deeper than few centimetres, but it is a reasonable assumption that this measure can be representative of the dynamics of a surface layer of about 5-10cm.

RC: 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"

AC: Thanks for the comment. The text have been modified.

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

AC: The text has been modified referring to the parameter a.

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

AC: This reference has been included.

RC: 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).

AC: It is true that there is a significant scatter in this figure. Looking carefully at the values estimated for a lower relative saturation there is a linear trend in figure 2.A that is more cloudy in Figure 2.B. This may be certainly due to the seasonality of the evapotranspiration losses. The text has been rewritten removing this emphasis on the linear trend.

RC: 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.

AC: We are not claiming that this represent a strategy to estimate the wilting point, but

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it was an option to derive the lower limit for the soil moisture.

RC: 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.

AC: Obviously, this result should be interpreted as a first guess of the model with parameter assigned on some of the available information. Considering all this, it may be considered sat-isfactory.

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

AC: We rephrased the sentence in the following: “Another difference observed is a higher RMSE for the exponential filter that in the present application is not rescaled”.

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

AC: This change has been made.

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