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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Estimating spatially distributed soil water content at small watershed scales based on decomposition of temporal anomaly and time stability analysis" *by* W. Hu and B. C. Si

Anonymous Referee #1

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Manuscript hessd-12-6467-2015 introduces an empirical orthogonal function (EOF) approach for analysing spatio-temporal patterns in soil water content observations. The presented approach is similar to other principal component analyses recently applied to spatio-temporally resolved geo-data. The approach may be seen as an extension to the one presented by Parry and Niemann (2007), a reference that is frequently cited in the manuscript. Parry and Niemann (2007) first extract the spatial arithmetic average soil water content from the 2-D spatio-temporally resolved measurement data. They then apply an EOF on the residuals which are consequently split into expansion coef-

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ficients (ECs, i.e. the eigenvectors of the space-time matrix of residuals) and empirical orthogonal functions (EOFs, i.e. the residuals mapped on the eigenvectors). EOFs may then be used to identify regions with similar hydrologic behaviour or to down-scale average water contents of the entire region. The novelty of the approach presented in hessd-12-6467-2015 is that first the temporal arithmetic average is subtracted from the data as in Mittelbach and Seneviratne (2012, also frequently cited). In a next step, the spatially constant fraction is isolated from the residuals. The EOF is then only applied on the residuals of the residuals. The authors discuss cases in which their approach has advantages over the one of Perry and Niemann (2007) and demonstrate that their approach yields water content better cross-correlation results for a dataset collected along a transect in the Canadian prairies.

The manuscript hessd-12-6467-2015 is in an already well developed state which made it relatively easy to read. As far as I can judge the English is good with only a few exception missing articles and occasional strange wording. The manuscript is largely well-structured albeit that I think that the manuscript would gain if the discussion on when the here presented EOF approach is advantageous (P6484,L12 – P6485,L23) was moved to the material and method section. As the authors write on P6484,L16 and L23, most of the text in these three paragraphs is founded on theory and is known a priori. I think it would make it easier to understand the new approach if the circumstances under which it is advantageous would already be quantitatively explained in the material and methods section. Moreover, the discussion section could be improved by better separating discussions i) on correlations between site factors and time events with model parameters (e.g. M_tn or EOF1) and ii) on prediction performance of the model. Also, the conclusions are more of a summary in its present state.

I was furthermore wondering why S_tn from the Parry and Niemann (2007) based model are not also correlated against the site factors (i.e. soil properties, slope, etc., see table 1). This would help to understand the differences between the two investigated approaches.

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Then a comment on section 2.3: does the performance of TA and SA not mainly depend on how well the respective ECs can be reproduced by the fitted function? I have the impression that the scatter in the S_tn – EC1 relationship is reduced for TA. . . may this be interpreted as such that the TA pre-filters more of the variance from the original data? But then, in a distant future, it may be desired to estimate the EOFs for ungauged catchments from a (future) database with data from water content observation networks, in a similar as done with pedotransfer functions. However, in the case of the TA, one more spatial distribution would have to be estimated. This is certainly not an advantage. Could you comment on this?

Finally, for the sake of clarity, I suggest to expand the sentence on P6472,L14-16 and convert it in a little section on how the site properties where compared to which model parameters. This section would nicely fit in before section 2.3. Also the multiple stepwise regressions used in table 1 should be mentioned here.

Otherwise I only have some specific comments. I recommend a publication of hessd-12-6467-2015 after revisions.

Specific comments P6468L4-6: this sentence disconnects the sentences before and after which belong together. It is difficult to understand what is meant. I would rephrase it.

P6470L2 and L3: “may be further”?

Section 2.1.: I suggest presenting the study area in more detail and include soil textures, elevation differences and vegetation. It would be also nice to be informed about the CaCO2 layer before it is discussed in the material and methods.

Section 2.2.: I found this section contains many long sentences, some of which are formulated in a misleading way.

Equation (2): In this point the SA method deviates from the one described in Perry and Niemann (2009). Please point this out and explain and justify why you preferred to

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estimate S_tn in this way.

P6473L15-P6474L4: see remark on section 2.2. I only understood what was meant in this section after reading Perry and Niemann (2007). It is for example not clear from the text why the abbreviation of EC is used and that EC corresponds to the matrix of eigenvectors. The manuscript would gain considerably if this passage was better explained.

Equation (4): Please also explain shortly why it is necessary to approximate ECt by a continuous function

P6479L8 and following: Percent of what? How can something contribute to another thing by more than 100%? What are %ˆ2 (Figure 5)? It needs to be explained in the material and methods what “percents” is referring to.

P6479L18: arithmetic average?

P6481L19: These values do not fit to the y-axes of figure 7. Please adapt. Please also call out figure 8 already at this point.

P6483L2: Please be more specific with what you mean by “needed”.

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