

## ***Interactive comment on “Soil water content evaluation considering time-invariant spatial pattern and space-variant temporal change” by W. Hu and B. C. Si***

**Anonymous Referee #2**

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This paper adopts a new method of decomposition of the measured soil moisture in space and time with some modifications. Such decomposition procedure was introduced by Mittelbach and Seneviratne (2012) and the authors suggests the use of EOFs for a further decomposition of the redistribution term  $S_r(i, j)$ . The aim of the paper is the construction of a model for soil moisture downscaling starting from preliminary information of the space-time dynamics of soil moisture. Even if the topic is certainly of great interest, the paper is not clear in the presentation of the methodology, of the aim of the research as well as in the presentation of the results. I have found very confusing the organization of the paper that contains a number of useless contents that do not

C6993

help the reader in understanding the real aim of the research. I think that the objective should be clarified reducing the length of the paper and removing all unnecessary contents for the economy of the paper.

The paper in my personal opinion is not well presented; there is an excess of useless information and the procedure suggested is not well described. There are two different analyses that in my opinion do not talk to each other the analysis of the EOF correlation with physical factor and the model application. What is the role of the first respect to the second is totally unclear to me. The paper is not fluent and provides a limited test of the procedure suggested on a small experiment with only 23 dates of measurements. In my opinion, the paper is not ready for publication on HESS.

In the following, I have summarized some of my major concerns regarding the present paper.

1. First of all, the reconstruction of soil moisture pattern is relatively easy when space-time data is available for the period under study. The authors in the present paper test the procedure in the ability to reconstruct the SWC pattern starting from a data driven procedure. In my opinion, the real challenge is the prediction of the soil moisture pattern during a not monitored period. This is the challenge that authors should address. Regarding this specific aspect I believe that analysis should be extended over a period of more than one year. In fact, equation 12 is calibrated on the estimated values of EC over the considered period; how can we use these equations over different periods?
2. Analyses have been carried out on 128 sample points in 23 dates. The total number of data used is about 2944. The first consideration regards the period investigated that is limited compared to the spatial sampling density. Some comments of this would be useful. Furthermore, this dataset seems limited to provide a validation of a new methodology.

C6994

3. The comparison of the two procedures is made neglecting the level of complexity introduced by the new method. In fact, the second method uses more information respect to the ordinary EOF approach and obtaining limited advantages (see figure 5).
4. I agree with the other reviewer regarding the need to provide further analysis of the procedure suggested mainly for two reason: 1) authors cannot compare their results with those obtained by Mittelbach and Seneviratne (2012) (see e.g., page 12843 – lines 26-28), because the two applications refers to different spatial scales; 2) The method proposed is provides limited advantages that are more evident in dry periods. This last result is obtained for a dry or wetter periods where usually it is easier to predict soil moisture pattern due to the reduction of the spatial variance of the process (this s certainly connected to the result obtained and to the shape of the curve reported in figure 7).
5. The Section “2.5 Other statistical analysis” describe additional analysis that are not functional for the paper. This section for me does not help the reader to understand the scope of the research. It must be removed.
6. Personally, the paper should focus strictly on the procedure and its validation. All the analyses of correlation of the EOFs with some physical pattern are useless in the economy of the paper. Findings are not surprising and also Therefore, I strongly suggest removing section 3.1 and 3.3.

#### Minor Aspect

Page 12883-line14: Why this term, obtained from the decomposition of  $\Delta S(i, j)$ , should represent the effect of soil hydrological processes? It means everything and nothing.

Figure 1: in addition to the spatial description of the temporal mean soil moisture, it would be useful for the reader to provide a graph with the evolution of the spatial mean SWC.

C6995

Figure 4: Only three dates are plotted over 23 and these raises the reasonable suspect that these dates where chosen among the best performing ones.

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C6996