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Discussions

Interactive comment on "Characterization of Hillslope Hydrologic Events Using Machine Learning Algorithms" *by* Eunhyung Lee and Sanghyun Kim

Anonymous Referee #1

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General comments: In this study numerous rainfall events are classified in a ten years data set of continuous soil moisture measurements using machine learning approaches. The resulting clusters of events are interpreted in terms of prevailing runoff processes. Thus the presented approach could be a way to extract valuable information from apparent complex data sets which would be highly appreciated by the scientific community. Unfortunately, though, the paper falls short to reach that aim. The approach is based on a number of non-transparent choices in regard to selected indices, to statistical methods and to the methods' parameters. Furthermore, the inferences with respect to prevailing processes are neither comprehensible nor convincing. These criticisms are detailed below.

Specific comments:

1. Stability indices: The study performs an analysis of temporal soil moisture variability. Thus the "Index of Temporal Stability" (ITS) is introduced. Unfortunately, it is defined in a really counterintuitive and misleading way: It measures exactly the opposite of what the term suggests (l. 155-158). Please rename to "Instability index" or the like. Secondly, the mathematical definition is unnecessarily complex and hardly comprehensible (equation 2). Why do you add squared mean and variance of normalized differences (δ)? What information does that index provide that would not already given, e.g., by the mean of normalized differences δ? In addition, both ITS and the normalized difference δ (equation 1) can easily be mixed up with the "soil moisture difference index" $\Delta\theta$ (equation 4). The latter seems to have been used only for the SOM classification. Why do you need that many different indices for soil moisture variation? This is very confusing and not comprehensible for the reader.

2. Classification of hydrological events: Assignment of hydrological events is a crucial point in this study, as clusters were assigned to processes. Unfortunately, the clustering is a very weak point of the study. First of all, I do not understand why an SOM has been performed prior the cluster analysis. I acknowledge that SOM is a nice tool to visualize multivariate data sets (cf., Fig. 5). However, assigning data to the nodes of an SOM is not without loss of information, as even slightly different instances of the data set are assigned to the same node. Thus I would highly recommend performing the cluster analysis directly on the data rather than on their aggregated representation in an SOM. Secondly, the number of clusters was selected in "a heuristic approach aiming to achieve a hydrologically meaningful classification of events and parsimonious clustering" (l. 289-290) without any clearly defined and understandable criterion. Likewise, no criterion is given for deciding on the superiority of one cluster approach compared to another (l. 211-213). Thus arbitrary decisions seem to have a major effect on the assignment to clusters, and subsequently to hydrological processes. Last but not least, according to Fig. 5, and except for antecedent soil moisture and rainfall (?) duration

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there is a rather smooth and continuous increase of the soil moisture difference indices. The only exception concerns cluster 5 and 6. In cluster 5, the soil moisture difference index at the upslope sites below 10 cm depth is clearly less compared to the downslope sites, whereas the soil moisture difference index is high both at upslope and downslope sites in cluster 6. Except for these two clusters the cluster analysis obviously subdivides the data set more or less arbitrarily along a single continuous gradient rather than identifying clearly distinct groups (cf., Fig. 8).

3. Identification of representative soil monitoring points: A major topic of the study is identification of "representative soil moisture monitoring points" (e.g., l. 30-31). It remains unclear in what regard these sites should be representative. According to l. 50-51 "high stability is an important criterion for determining the best location for the monitoring spatially averaged soil moisture of a given area". The study focuses on the temporal dynamics. But here the term "representativeness" cannot refer to the temporal dynamics because "high stability points" would systematically underestimate that dynamics. On the other hand, "high stability" points could not be representative for the spatial mean either. Highest temporal stability likely occurs at sites where the soil is close to saturation all the time, that is, at sites with the highest soil moisture values. Or do you mean "representative" in regard to ascription to hydrological event clusters (L. 114-116, l. 485 et seq., l. 511 et seq.)? But then temporal stability of soil moisture would not be a relevant criterion.

4. Assigning clusters to processes: Results from the upslope and downslope sites, respectively, are aggregated (per depth), and differences between these groups are interpreted in terms of systematic differences between these groups. However, it has not even been tested whether these groups differ significantly at all. Data of all single sites are presented in Fig. 2 only, revealing substantial heterogeneity even within the upslope or downslope sites, respectively, and suggesting more small scale variability (consistent with my own experience) rather than systematic differences. At least there seems to be substantial overlap between upslope and downslope sites. The study

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aims at assigning single hydrological events, characterized by meteorological and soil moisture data based indices, to clusters, which in turn are interpreted in terms of hydrological processes (l. 90, l. 111-113). The latter step is of fundamental importance for the study. Unfortunately, that step remains completely obscure to me even after having studied the manuscript again and again. There is no clear and comprehensible reasoning at all. How do the indices relate to the respective processes? Relationships are postulated but without sound justification (e.g., l. 317-319, l. 322-324, l. 334-l.338, l. 402-462). Assigning differences in soil moisture at a scale of a few hours to lateral subsurface flow over a distance of roughly 50 m (Fig. 1) would require fairly high lateral flow velocities. Is there any additional evidence (e.g., tracer experiments) for that? Did you account for the spatial heterogeneity of throughfall and stemflow? What about surface runoff that might have re-infiltrated on its way downslope?

5. The manuscript requires substantial language editing.

Technical corrections:

6. L. 23: Please be more precise. According to Fig. 2 soil moisture was measured at 10 sites but at three different depths each.

7. L. 142: Were the trenches re-filled after installation of the soil moisture probes? If not, how to deal with resulting artefacts?

8. L. 186-188: Please be more precise. Logarithm transformation is one out of a set of Box-Cox transformations. Did you apply other Box-Cox transformations as well? If so, for which observables, and how? Besides, neither the logarithm nor other Box-Cox-transformations centralize the means of the variables to zero. Instead, these transformations are usually applied when Gaussian distribution is required. However, that is not the case for any of the applied approaches.

9. L. 279-281: I do not understand why you select soil moisture measured at one single point "as the representative soil moisture before the event for the SOM analysis". That

introduces an unnecessary bias. Why not taking the mean of the values measured at the different sites?

10. L. 574: “https://www.re3data.org/” is not a repository but gives an overview over numerous repositories. Please be more precise: Where will the data be published?

11. References: Some references are out of alphabetical order (Minet et al. 2013, Montero and Vilar 2014, Zhu et al. 2014).

12. Fig. 5: Figure caption: Missing explanation of the lower panels (cf., caption of Tab. 1).

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