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Hydrology and Earth System Sciences Discussions

Interactive comment on "Temporal stability of soil moisture spatial variability at two scales and its implication for optimal field monitoring" *by* X. Zhou et al.

Anonymous Referee #1

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Overall:
This paper is based on a very interesting dataset of spatio-temporal soil moisture distributions. Unfortunately, the paper is not very well structured: The objective of the paper (p.1188:19ff) was to link soil-landscape features to soil moisture distribution in order to better locate representative soil moisture monitoring sites: This goal has not been reached. The paper shows numerous graphs showing correlation between single locations and catchment mean, but none of the graphs gives advice how to locate a monitoring site! In addition, the text contains numerous comments and explanations (expressing common hydrological knowledge) without clear reference.
In Detail:

The Title is misleading: Optimal field monitoring is not the concern of your paper.

The accuracy of the TDR methodology used is not addressed. The TRIME-T3 TDR applied in conjunction with access tube shows a high measurement uncertainty due to the fact that even a small air cushion between tube and soil results in large measurement errors (underestimation for dry conditions, overestimation for saturated conditions) and the non-validity of calibration parameters for soils with a high clay or stone content. In addition, the tube may enhance/enable vertical preferential flow processes influencing the vertical soil moisture distribution. These facts may also explain in part the spatial and temporal pattern of soil moisture. Measurements always have uncertainty bands; this fact has to be addressed in a paper totally based on measurements!

The nomenclature ("swale scale" and the region soil names) is not the international standard.

On p1192:2ff the selection of the IDW method for spatial interpolation is explained. It is mentioned that other interpolation methods (e.g. ordinary kriging, universial kriging, and co-kringing) were tested, but IDW method provided the smoothest map. Why the "smoothest" map? Kriging techniques are the state-of-the-art methodologies to
(a) analyse the spatial correlation and
(b) interpolate on the base of a well adjusted semivariogram.
This methodology would also provide substantial information of correlation lengths between your measurements which would enable you to better address the question weather or not your sampling grid adequately represents your spatial variability (e.g. by giving you probability maps). In addition, co-kriging or similar techniques ("external drift kriging") would allow you to link soil moisture to soil-landscape features as you formulated in your objectives.

On p1192:24 you mention site 13: Only figure 1 allows finding single sites (after a long search). In addition site 13 has - in contradiction to your description a high temporal stability in depths 40cm / 60cm (Fig. 2).

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On p1193:7 you mention the concept of self-similarity without giving a reference.

On p1194:18 you argue that you only take soil spatial variation into consideration: But in the following sections you mainly discuss single sites instead of clustering your measurements in order to get reliable results which might also be transferred to other regions. Taking into consideration the high uncertainty of the measurement principle, it makes no sense to discuss (and explain by more or less general hypothetical hydrological processes/properties) the behaviour of single points.

Your Summary and conclusion (p1198:10ff) gives a good outline what you should have addressed in chapter 3: To really explain the differences in temporal stability not for single locations (with high measurement uncertainty) but for groups of similar sites (function of topography, soil type etc.). You mention again the self-similarity of the swale and the catchment scale: The also could be a major concern of your paper or even to main topic! Without references and detailed discussion of the back-ground of this theory, it is only one of the numerous (non-prooven) hypotheses spread over the entire paper.

The graphs show too many details (behaviour of single sites) without supporting the major conclusions made at the end.

Conclusions

I recommend to reject the paper in it's actual form, because:
-The paper does not reach substantial conclusions.
-The scientific methods and assumptions are not all valid (e.g. measurement uncertainty not addressed; the use of IDW as spatial interpolation method, the spatial structure of the data set has not been analysed by appropriate methods like kriging techniques).
-The results do not support sufficiently the interpretations and conclusions made.
-The references are not appropriate

The data set may allow the author's to reach substantial (and in part novel) conclusions

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by focussing e.g.
(a) on the implication of your findings for a optimal field monitoring
(b) on the self-similarity concept
(c) on a more general pedo-hydrological interpretation of spatio-temporal pattern of soil moisture

Therefore, my point of view is that the paper needs to be completely rewritten.

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