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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Temporal stability of soil moisture under different land uses/cover in the Loess Plateau based on a finer spatiotemporal scale" *by* J. Zhou et al.

Anonymous Referee #2

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Ref to "Temporal stability of soil moisture under different land uses/cover in the loess plateau based on a finer spatiotemporal scale" with MS number hess-2013-307.

Any research of soil moisture dynamics is an attempt to understand the mechanisms of ecohydrology, it is much critical in dryland area due to the environment being sensitive to change. The paper used four types of landuse/cover on the loess plateau, based on the concept of temporal stability of soil mositure, tried to develop an empirical model called ET-TSSM, to understand the soil moisture dynamics in two different hydrological processes, i.e. DTW and WTD in a finer temporal scale, i.e. precipitation events. The paper described ET-TSSM defined by themselves, also employed the data of soil water

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content in four types of landuse/cover to test the model, and tried to explain the result using the differences in the ecological and physiological characteristics of the single plants.

The paper involved a “very fine” experiment design, seemly rational results from the data and complex discussion with only highly “possibility” to interpret results. The paper provided a so called new method which still need further validate, also threw a heavy uncertainty and doubt (“promising research”) for readers to guess. The main comments and problems are:

1. The sentence, language and even words all through the paper need to refine to reflect the exact meaning the author wants to express.

2. In general, “Introduction” is not very clear and logic. It is not easy to understand and follow, or the logic is not very persuasive for readers. Some sub-conclusion sound to be far-fetched, references cited weren’t strong to support conclusion. Give some example like: What is meaning of “uniform sampling strategy”? How to approve the “uniform sampling strategy” used by “former TSSM studies”? What means of “coarser spatial scale”? How can you speak your study is based on finer spatial scale, not a coarser scale, what is your reference system? What is the “important temporal information of the soil moisture existing at finer spatial scale”? What are included in these “some important information”?

3. The sampling scheme in methodology is not clear. Why the plots are designed by 60x60cm not other size? What is the depth? What’s the materials to separate the plots? Do you have the lateral effects on soil moisture by those separating materials? How to deal with the lateral effects? How do you measure the soil water in CP position, especially in plot4? How do you deal with the effects of root? Why these four land use types or plants used? How about these plants growing? What is the above-ground biomass and underground biomass? Leaf area index? What is the density or distribution of stem and root system? What is litter above ground? What is the differ-

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ence about the precipitation interception, stemflow, evapotranspiration between these single-plants? Is there soil crust or biological soil crust? What is the influence of these crusts to soil water and soil water dynamics? What is the statistic characteristic of the data? The groups of data obtained in the paper are significantly different? Or the data are normal distribution? Please evaluate the data by yourselves in view of statistics. And please explain why no-significant-difference data among sampling points and plots could be used in the paper, or in the research? What level of errors would be induced in the result? Exact information from experimented plants could give strong support and make it much clear of the complex results, not only possibility mentioned in discussion.

4. Results: Is it contradictive of the "In the rainy season of 2012, the bare land cover appeare to be more sensitive to the influence of rainfall and radiation. . .. " and " the soil water content in the bare land cover displayed a stronger temporal stability during the WTD process. . .. "? What is the implication of "But, according to the significant difference analysis, in both the WTD and DTW processes, the soil moisture of the different land uses/cover at the same sampling position showed no significant difference, and the soil moisture in the same land uses/cover at different sampling positions also showed no significant difference."? It would affect the correction of result? Or accuracy of result in the paper? Is it contradictive between the statement of "With respect to the MRD, the soil moisture of the vegetated land uses tended to underestimate the mean soil water content due to their MRD values being larger than zero" and "With respect to the AP-TSSM, in the DTW process, the soil moisture of Artemisia coparia was overestimated, with its MRD being larger than zero"? To the statement of "Consequently, plot4(3) and plot3(2) which had an average soil moisture in the WTD process of approximately 16.6% and 16.4 %, respectively, were determined to represent the ïĄśs in the CP and AP sampling schemes, respectively." Please give the reason why plot4(3) and plot 3(2) could be selected as the representatives of ïĄśs? Do they have deterministic topography? plant growth duration? soil texture?

5. In the section of "discussion", the paper used much more theoretical description in

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words to explain how the function and effect of “canopy”, “litter layer” and “root system”, or “stemflow”, “the point-based litter” and “infiltration zone of main root zone”, or “diversity of morphological structure” to act in the hydrological processes of WTD or DTW in different vegetation types, but what on earth the differences in the four specific landuse types used in the paper?

So “4.4 Uncertainties and limitation” exactly exits in the paper. But readers need much more detailed explanation not only theoretical “uncertainties and limitation” description.

At last, the “Conclusion” is much more like “Abstract”, and “Abstract” like “Conclusion”, isn’t it?

Due to too much "uncertainty" and "limitation" in the paper, I would suggest a major revision of the paper before publish it.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 10083, 2013.