Hydrology and
Earth System
Sciences
Discussions

Interactive comment on "Soil moisture: variable in space but redundant in time" *by* Mirko Mälicke et al.

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We would like to thank Anonymous Referee 1 for the constructive and insightful review of our work.

Major & Moderate comments

In the following, we will respond to the Referee's comments in the order of appearance (Referee comments in italics). For those comments, which are in line with Ryan Teuling's (RT) comment, we refer to our corresponding response.

1) We agree that our findings need more comparison to existing literature and will revise the paper as outlined in our response to RT's comment. We thank the referee for providing various examples for presenting and interpreting our results in a more quantitative way:

  a. *How much rainfall is needed to have a change in a different cluster?*
  Response: As we pointed out in the manuscript, on p.17 l.32, there are instances, where a cluster transition is not forced by a rainfall event and not every significant rainfall event is causing a cluster transition. Additionally, we remind that the dispersion functions are based on a 30-days rolling mean, which makes it difficult to relate a dispersion function and therefore a cluster transition to a rainfall event on a daily basis. However, we agree that rainfall sums preceding a cluster transition might be an interesting detail to add.

  b. *What is a "strong" rainfall event causing the changes?*
  Response: We will define a threshold and give basic statistics on rainfall events and available soil hydraulic properties.

  c. *Is it rainfall amount or frequency that is important?*
  Response: This is an interesting point, we will calculate rainfall event frequency and add them as a result if it turns out to be insightful.

  d. *What is the quantitative difference in spatial dissimilarities between clusters during raining periods? How it varies with sampling distance?*
  Response: We think that the difference in dispersion functions are shown in various figures, and they characterize how average differences develop with increasing separating differences (similar to a semivariogram). So the spatial dissimilarities are readable from the difference on the y-axis, while the sampling distance is given on the x-axis. (e.g. Fig 4. a-c, Fig. 5 d, e, f Fig. 6 a-d).
  However, we agree that a quantitative measure for the range of dispersion functions within one cluster might be helpful. We will try to find a good

measure for quantifying this range

e. *How long are cluster periods in different conditions?*
Response: Thanks for this good idea. We will extract the period lengths from the figures and give them as numbers. The fact that under dry conditions generally less cluster emerge (which last longer) is from our point of view already discussed in section 4.3 of the manuscript.

f. *How do they vary with depth?*
Response: We will extract the difference in period length over depth from Fig. 5.

g We agree that calculating the quantitative measures described above can be helpful to back up our findings where applicable. The Referee requests a comparison to existing quantitative results from literature. We will review the literature and include comparisons in the discussion.
We would like to emphasize that we consider qualitative descriptions of our results necessary to better understand the findings of this study. We agree that quantitative results will add clarity and comparability. Hence we will include quantitative measures as indicated in a-f in an additional result table.

2) *An important information that is currently missing is the "typical size of the hillslopes". Is the size of the different hillslopes similar? Can the authors add a figure with a typical configuration in 2/3 hillslopes? Moreover, what is the size of the basin?*
Response: We completely agree with the Referee and thank for pointing out this missing information. We will add the basin size and characterize the hillslopes in the study site description.

3) *The paper is too long in many parts (at least for me), e.g., in the description of the methods and the discussion of the results. I strongly believe the paper may benefit from a reduction of the text by focusing on the main (quantitative) results that*

have been obtained from the analysis of the soil moisture data. Some descriptions of the methodology can be moved in the appendix. The current version of the paper is not easy to follow.
Response: We will consider revising the methods to shorten them. However, as the development of the methodology is a major focus of this study, a detailed description of the methods is vital to understand the results.
We will revise the discussion of our results including the results that will be obtained from our comment 1).
The Referee argues that we should shorten the discussion of our results in favor of a focus on quantitative results. In comment 1) the Referee requests a more detailed comparison of quantitative results, which will be added, and comparison to the existing literature which will also be added. This is important. A detailed discussion of the already presented results is however of equal importance. We will revise the discussion to achieve a good balance between these issues and will make an effort to improve readability and ease of comprehension.

4) *It's not clear to me how the authors have aggregated the data for the different hillslopes. How are measurements from different hillslopes aggregated? How to address the differences in land use and topography? The problem is underlined at page 6 but not addressed in the paper.*
Response: To clarify: The data has not been pooled into datasets per hillslope but is analyzed across the whole study area. This aggregation refers to an average value for each time series within the moving window, not a spatial aggregation. See equation (1) (p.5) We will clarify the paragraph.
We hope that we understood the Referee correctly, but differences in land use and topography are addressed in the paper. The paragraph on p.6 (l.16-19) is further referencing the paragraph from l.18 to l.30 on page 8 and both are dedicated to this issue. To clarify: We only formed point pairs, which are not further apart than 1200 m, because beyond this distance the observations are most likely

located on different hillslopes. In these cases, observations might be more similar due to land use or topography, and not separating distance. We will revise the paragraphs to make this clearer.

5) *In the discussion two "periods" are highlighted, drying and wetting. However, in the paper 3 to 4 clusters have been identified. Why? The authors should add a clear explanation for that, I believe that we do not have only drying and wetting periods, but it depends on when they occur with respect to vegetation cycle. Is it a possible explanation?*

Response: We will add a clear explanation about the emergence of clusters. We will highlight that the number of identified clusters is already an important result. We will clarify in the discussion that the clusters found in winter/spring and summer/autumn are similar to each other and this broad grouping is comparable to the often cited two 'soil moisture states', that were found in the Tarrawarra catchment in Australia (Grayson et al., 1997, Western et al., 1999). We will rephrase these parts.

We agree with the Referee that there are more than only 'radiation driven drying' and 'rainfall driven wetting' periods, there is also seepage, which we loosely subsumed under drying. We also strongly agree the annual climate cycle and vegetation cycle are important seasonal controls, which might manifest in clusters. The proposed method is feasible to characterize particularly the latter influence as we use 30-days mean values. We will add a clear explanation that our method did identify more than two clusters. However, in many cases the found clusters were similar during 'wet' and 'dry' conditions (similar to Grayson). We agree with the Referee that we should directly address this in the manuscript. We will clarify the respective sections.

6) *Throughout the text, some small formatting and typo corrections are needed. Please carefully check the text for such errors.*

Response: We apologize and will carefully proofread our manuscript before re-

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submission.

Specific Comments:

P1, L10: The extent of the hillslopes and of the basins should be specified in the abstract.
Response: We will add these numbers.

P1, L14-17: The concept of "redundancy" and "compression" are clear only by reading the full paper. It is not clear by reading the abstract what is communicated here in these sentences. Please revise.
Response: We will clarify these sentences.

P2, L10-11: "Soil moisture at the headwater..." This sentence should be revised, and also the paragraph L13-17
Response: As indicated in our response to RT's comment, we will revise this part and correct wrong contextualized references.

P3, L19: The concept of "redundancy" should be clarified here in the introduction. Otherwise, it is hard to understand what the authors mean here.
Response: We will clarify and add an explanation on 'redundancy'

P3, L28-...: The hypotheses to be tested are too specific. The authors have written such hypotheses after knowing the results (I guess). I suggest reformulating them to be less specific
Response: We respectfully note that we think our hypotheses should be specific to be testable. Each of them is focusing on one part of the proposed method to guide the reader through our manuscript. However, we suggest a revision of the paragraphs in the introduction, which leads to our hypothesis. This will help to better follow our argumentation. The affected paragraphs include: H1: p.2 L.18-21; H2: p.3 L.10-15;

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H3: p.2 L. 23-28; H4: p.3 L.18-24.

Finally, we would like to thank the Referee for his helpful, detailed and insightful review of our work.

References

Grayson, R. B., Western, A. W., Chiew, F. H. S., Blöschl, G. (1997). Preferred states in spatial soil moisture patterns: Local and nonlocal controls. *Water Resources Research, 33*(12), 2897–2908. https://doi.org/10.1029/97WR02174

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