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Hydrology and Earth System Sciences Discussions

Interactive comment on "Use of soil moisture dynamics and patterns for the investigation of runoff generation processes with emphasis on preferential flow" *by* T. Blume et al.

Anonymous Referee #2

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Review comments on MS-NR: hessd-2007-0109

Overall comments:

This paper presents interesting and valuable dataset regarding how soil moisture spatial-temporal patterns may be used to infer soil hydrological processes in a forested catchment in Chile. The graphical methods employed (space-time color maps and indictor maps) are interesting ways to facilitate the visualization (and thus the understanding) of flow processes involved in the change in soil moisture spatial-temporal patterns. The dye-tracing study provides added insights regarding the flow pathways in the three continuous monitoring sites along a hillslope transect.

With further modifications and clarifications, this paper has the potential to make new contributions to our understanding of 1) how the map-based graphical methods can do a better job than the classical line-based plots in revealing soil moisture spatial-temporal patterns and the underlying hydrological processes, 2) what new insights can be gained through the proposed multitude of experimental methods (i.e., the combination of spatially scarce but temporally high resolution soil moisture profiles with episodic moisture profiles at additional locations, plus one-time dye-tracing experiments) that none of the individual approach alone can obtain (i.e., the connection and synergy among the three datasets), and 3) how the volcanic ash soil hydrology may be different (or indifferent) from our common understanding of soil moisture dynamics. The listed four questions on p. 2591 seem more like the general objectives of a larger study that this paper was part of. To answer these four questions thoroughly would require a more comprehensive treatment of all the datasets collected. Therefore, this paper may serve the scientific community better by focusing more on the unique/new aspects of the study (such as one or more of the three aspects highlighted above) in an in-depth manner.

While data-rich and considerable work went into this research, the connections among the pieces of the datasets have not adequately emerged from the results and discussions reported in the current version of the manuscript. The reviewer is left wondering about 1) the overall picture of this catchment and its hillslope hydrology beyond the point observations, 2) how the 14 monitoring sites were selected and how their data could be linked (from a landscape perspective) to shed light on the underlying runoff and preferential flow processes, and 3) how soil type and catena (from hilltop to hill bottom) may play a (significant) role (in addition to topography and vegetation) in assisting the experimental design and data interpretation. Besides the claim that preferential is significant in this catchment and that preferential flow is related to hydrophobicity, local heterogeneity, roots, and others (BTW, such knowledge is commonly reported in the literature), more in-depth understanding of the spatial-temporal connection of preferential flow would be desirable. In other words, the connection of the three datasets (3 continuous monitoring datasets, 11 sporadic manual measurements, and one-time dye-tracing experiments) would add more values to our understanding of the preferential flow dynamics beyond one-time or one-site observations.

Clarifications on some key terms (such as adding specifics to the definitions of pattern, threshold, runoff, long-term, etc.) and additional details of the methodologies used would be needed to facilitate a better judgment and interpretation of the scientific merit of this paper. See specific comments in the following.

It is hoped that the following specific comments would be helpful to the authors for further improvement of this manuscript.

Specific comments:

Methods related:

1. The space-time color map of soil moisture is certainly a newer way of looking at soil water spatial-temporal patterns. However, I wonder whether absolute moisture content (instead of relative change) would be more valuable to use for understanding the complex dynamics of soil moisture, since initial/antecedent soil moisture content has long been recognized as a critical factor in controlling soil hydrology. The zero on soil moisture color ramp in Figs. 2 and 3 (corresponding to antecedent soil moisture content) seems to have removed such an important factor in understanding soil moisture dynamics and related hydrological processes.

2. Another concern about using soil moisture content changes to refer soil hydrological processes is that soil water storage could remain unchanged or little changed while the flux (such as preferential flow) might be occurring significantly. Is there anyway that additional constraints could be imposed so that the inference about the underlying hydrological processes could be better achieved? Such a concern is not unique to this

study, but to all of this type of research. I raise this issue merely to see whether the authors might have further insights along this line.

3. I also have some unclearness regarding the indicator maps: I had a bit hard time understanding the justification and meaning of the so-called threshold values (median and 75% quartile). Threshold value as used in hillslope hydrology has a different meaning. It was also not completely clear to me how the median and 75% quartile were determined. Was it for a specific depth over all the 11 (or 14) sites on each of the 41 specific sampling occasions?

4. Since the graphical methods employed in this study is relatively new to many readers, it would be helpful to provide more specifics regarding how Fig. 2 type of maps were produced, including the steps taken and the software used.

5. Ground water table data in Table 1: please clarify whether it refers to the depth below the soil surface? In Figs. 2 and 5: ground water level increase means exactly what?

6. p. 2591 lines 15-30: It would be easier to follow this information if the whole soil profile description is provided, and the physical/hydrological data can be described according to soil layers or in a table format.

7. It would be helpful to data interpretations to indicate what soil horizons each of the 6 monitoring depths (10, 20, 30, 40, 60, and 100 cm) correspond to in each of the three continuously monitored sites.

8. p. 2592 lines 15: what kind of soil samples were used for lab Ksat determination? A bit more specifics are needed.

9. p. 2594 line 16: why the calibration was done only for the upper soil horizons while the Delta-T profile probes reached 100 cm depth in the monitoring?

10. p. 2594 lines 25: the points are not quite evenly spaced in Fig. 1.

HESSD

4, S1303–S1310, 2007

Interactive Comment

11. More details are needed for the dye-tracing experiments: please indicate the width and length of the 1.2 m2 plots; what was the adsorption coefficient of the Brilliant Blue dye by the young volcanic soils; how the profile sectioning was conducted during the excavation (Fig. 4 only shows roughly the central section, while additional sections of dye pattern may show better the 3-D nature of the flow); what was the initial soil moisture content for each of the three sites/soils in Fig. 4?

12. p. 2596 lines 2-5: please justify the threshold values used.

13. In several places of the manuscript, “our perception of flow in the unsaturated zone” was mentioned (e.g., p. 2595 line 3-4). It would be helpful to spell out this "perception"; and how the results confirmed or deviated from such a perception.

14. p. 2597 lines 1-6: specifics of soil water retention determination are needed. Why only the 1st two horizons below the humus layer were determined (while the monitoring depth reaches 100 cm depth)?

15. p. 2608 line 21: what is "back-of-the-envelope" calculation?

Results related:

16. As the authors indicated, "Water repellency under field conditions is likely to be less pronounced" (p. 2598, lines 23-24). I wonder about the possible relationship between antecedent soil moisture content vs. hydrophobicity? Besides, under forested cover, generally there is an O horizon, which the rainwater must infiltrate through first before getting into the mineral soil layers. Therefore, it is not clear what degree of relevancy the WDPT test results reported in Table 2 may have in explaining the observed soil moisture spatial-temporal patterns at different antecedent soil moisture contents. Table 2 basically shows that the top 20 cm soils have water repellency, while the subsoils are wettable. However, in Fig. 2, for example, probe 2 in the 1st rainfall event under the driest condition (presumably with highest water repellency), probe 1 in the event 2, and probe 3 in the event 3 all have pronounced moisture increase in the top 20 cm soils.

HESSD

4, S1303–S1310, 2007

What is the explanation for this? Perhaps the three probes in each rainfall event had different antecedent soil moisture contents, but the space-time color maps (and Table 1) did not indicate such possible difference.

17. In several places of the results and discussion section, the authors attributed observed soil moisture spatial-temporal patterns to (assumed) "lateral flow." Was there direct evidence of such lateral flow in this catchment that can be provided? This would be very helpful.

18. The title and other places in the manuscript where the term "runoff" was mentioned, a more explicit "subsurface stormflow" would seem to be more appropriate since "surface runoff" is unlikely in the study catchment as indicated by the authors (e.g., p. 2602 line 16).

19. p. 2601 line 28 to p. 2602 line 2: how Fig. 2c probe 3 response could be explained then? Probes 3 had stronger moisture response than probes 1 and 2 that were (presumably) located in preferential flow paths (Fig. 4).

20. p. 2602 lines 24-26: If this statement is to be defensible, soil textural difference between 10-, 20-, and 30- cm soil at the Probe 3 location should be provided, and the actual root depth in Fig. 4c should be indicated (which does not seem to match with the 20 cm mark?). A clear soil description and soil profile characterization, as suggested in an earlier comment, would be helpful in explaining the 20-cm soil being consistent wetter. Could it be possible that a pocket of loose soil around 20-cm probe location would have accumulated more moisture, as guessed from a quick examination of Fig. 4c?

21. Table 2: Were forest 1, 2, and 3 correspond to P1 to P3 or not?

22. Fig. 1: The catchment boundary between the left and right figures does not seem to match exactly (especially its shape around the outlet). Please indicate the right side figure location on the left side figure. Again, the manual monitoring sites between P1

and P2 are not equally spaced as claimed in the text. If other 3 wells (other than W1) were not used in this paper, it may be better to remove them; otherwise, please indicate what the other 3 wells are there for.

23. Fig. 2: It would be very valuable if links can be established between the soil moisture change pattern with stream discharge and groundwater level change within Fig. 2, and to connect Fig. 2 data to the lag time in Fig. 5 and the longer-term soil moisture dynamics in Fig. 6. Another improvement that may be made to Fig. 2 is to make all 3 sub-figures to use the same color ramps for discharge, groundwater level, and soil moisture (as that done for the rainfall) so easier comparison among the three events can be made.

24. Fig. 3: I assume the soil moisture data here also represent change relative to antecedent soil moisture content? Please clarify.

25. Fig. 4: Please add depth labels of actual sensor locations along each probe profile. Again, I would strongly suggest a corresponding description or characterization of these three soil profiles in the text, probably best summarized in a table. A general discussion on the trend of soil properties along the hillslope transect (i.e., soil catena) would also be helpful to connect the point observations.

26. Fig. 5: Could shorter lag time of response in the summer season suggest a more pronounced preferential flow in the catchment, probably caused by stronger hydrophobicity in the drier conditions?

27. Fig. 6: I would suggest Fig. 6 to include all three continuously monitoring line plots in a similar manner, and to explain more fully the differences among the sites as well as between the depths with each profile. Again, a link to soil profile characterization would be helpful in this regard. I would also suggest the use of two arrows instead of the big circles to indicate the drying periods in the summers.

28. Fig. 7: The explanation of Fig. 7 is insufficient. What do we learn about hillslope

HESSD

4, S1303–S1310, 2007

Interactive Comment

EGU

hydrology and soil catena here? Even for the 10-cm probes, was the explanation of shading effect consistent with the data shown in Fig. 2?

29. Fig. 8: Could this consistent local variability pattern be attributed to the way the access tubes were installed and the surrounding soil heterogeneity around each tube? Such obvious directional variability, plus the preferential dye movement shown in Fig. 4 for 2 out of 3 probes, makes me wonder how significant the artificial air gap might have had an impact on the collected data. It would therefore be desirable to look at all of the 11 manually monitored sites rather than only H4 and H5 sites. It would be helpful to indicate where H4 and H5 sites are located in Fig. 1.

Misc. others:

30. Some sentences started with number or symbol, which should be avoided.

31. Some repeated statements should be avoided. For example, the statement related to "The combination of spatially scarce but temporally high resolution soil moisture profiles with episodic and thus temporally scarce moisture profiles at additional locations provides information on spatial as well as temporal patterns of soil moisture at the hillslope transect scale" has appeared in at least many places throughout this manuscript.

32. p. 2695 line 5: May 2007 should be May 2006 as indicated in p. 2599?

33. Table 1: the 2nd and 3rd events should be for 2005 instead of 2004?

34. p. 2601 line 9: "arrow 1" should be "arrow 3"?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 2587, 2007.