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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.

T. Blume et al.

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Thank you very much for your constructive review of our manuscript. The specific and detailed comments are very helpful for the improvement of the article. The revision of the manuscript will be carried out based on your recommendations. Following below you will find a detailed response to each of your comments.

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 spatialtemporal 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.

RESPONSE: We agree with the reviewer that the 3 questions listed by him cover important aspects of the paper and should be included in the introduction. While the reviewer is correct in assuming, that the 4 questions on p. 2591 are also the objectives of a larger study, we still feel that these more general objectives should also not be omitted.

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.

RESPONSE: A summary of the hydrological characteristics of this catchment as ob-

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tained by other data sets and experiments will be included in the revised document. Furthermore, site selection and connectivity between the sites/along the hillslope transect will be discussed.

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.

RESPONSE: We agree with the author that a better understanding of the spatio-temporal connection of preferential flow would be desirable. While we were able to observe both rapid lateral and vertical flow at the point or plot scale it was not possible to observe hillslope scale connectivity of these flow patterns. This connection can only be deduced from our analysis of the changes in response times from winter to summer (when preferential flow observation were more pronounced). Additionally, hillslope scale patterns were also investigated with the space-time maps of soil moisture for the two hillslope transects (the "threshold plots").

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 look-

ing 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.

RESPONSE: We agree with the reviewer that antecedent moisture content is indeed of major importance and that this information needs to be given in order to interpret the color maps. In the reviewed version of the manuscript this information was included in Table. 1. Changing the color maps to depict absolute moisture content instead of relative change would produce a very confusing picture, making it impossible to observe event dynamics. This is due to the fact that moisture content is highly variable across the different soil depths/horizons and thus the spatial/vertical variability is much higher than the temporal variability (Blume et al. Hydrological Processes (accepted)). However, we agree with the reviewer that it would facilitate the analysis if antecedent moisture content was included in the figure. In the revised manuscript the colour maps now include 3 plots (one for each probe) of antecedent moisture in comparison to annual median soil moisture for each depth.

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.

RESPONSE: We agree with the reviewer that at steady state and in the absence of layers with highly different matrix potential/hydraulic conductivity/porosity considerable flux can occur that would not result in a change in soil moisture content. For these steady state fluxes to occur the soils need to be at least at or above field capacity. This fact will be included in the revised manuscript.

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 75Was it for a specific depth over all the 11 (or 14) sites on each of the 41 specific sampling occasions?

RESPONSE: In these plots not absolute values but indicator variables (non-linear transformation) are shown. The data is thus treated as a binary indicator (where values above a certain threshold become equal to 1 and values below or equal to this threshold become equal to 0). This allows for a comprehensive structural analysis and the values are robust with respect to outliers and missing data values (Lyon et al., 2006, HESS). These indicator variables can also be used in geostatistics for the determination of "indicator semivariograms" as a description of spatial structures. The use of the term "threshold" in this context is not unusual, as it is in geostatistics in general. In our case the calculation of semivariograms was not possible due to the limited number of data points. The median was calculated separately for each depth and sampling occasion but over all sampling locations (both transects) - it is thus a time-variable threshold for each depth. This means that the plot shows for each sampling time which locations are wetter than 50% of the sensors of that depth, thus giving valuable information on "relative" moisture patterns along the hillslope.

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.

RESPONSE: These figures were prepared with a simple EXCEL macro. However, they can also easily be produced with R or Matlab or similar programs.

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?
RESPONSE: This data refers to the (arbitrary) well datum (the higher the groundwater level, the closer it is to the surface). This will be clarified in the revised manuscript.

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.
RESPONSE: We assume the reviewer is referring to p. 2601 lines 15-30. The soil physical characteristics of the several commonly found horizons are now shown in more detail and are summarized in a table. This includes porosities, hydraulic conductivities, grain size distributions, bulk densities, field capacity and permanent wilting point. However, these characteristics were not determined specifically for the location of each of the sensors, but on the basis of commonly found soil horizons. It is thus difficult to assign the sensors to a specific horizon.

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.
RESPONSE: A precise attribution of sensors and soil horizons proves difficult, a) due to the extreme heterogeneity of the soil (physical characterisation of soils was only carried out for the most common horizons) and b) due to the fact that soils were not sampled and characterized at the exact locations of each of the sensors (sampling and soil characterisation was carried out during an early stage of the investigation, were destructive sampling at the measurement locations was not an option).

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

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RESPONSE: These analyses were carried out with soil cores (8cm diameter). This fact and the specifics of the different most common soil horizons will be included in the revised manuscript.

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?
RESPONSE: The calibration was done for the two most commonly found upper horizons, H1 and H2. These horizons can extend to a depth of 130 cm.

10. p. 2594 lines 25: the points are not quite evenly spaced in Fig. 1.
RESPONSE: Due to the very steep terrain these points are only roughly evenly spaced. This will be corrected in the revised manuscript.

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?
RESPONSE: The plots have a size of 1.2m x 1.2m. The adsorption coefficient of the brilliant blue in volcanic ash soils is unknown. In these experiments the focus was on the crossection containing the soil moisture sensor. Going into detail on the 3D nature of the flow would increase the length of the manuscript considerably. In a more extensive dye tracer study carried out in this catchment, similar flow patterns were observed in all sites under forest (Blume et al. Hydrological Processes, accepted). This information will be included in the revised manuscript. The initial soil moisture content (and the comparison to median soil moisture) of all sites/soil depth is included in Figure 3 of the revised manuscript.

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

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RESPONSE: These values were chosen as threshold as they were identified as high enough to mark the onset of an event (in contrast to "the noise" of the dataset).

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.
RESPONSE: We agree with the reviewer that a more detailed description of this perception of the active flow processes is necessary and it will be included in the revised manuscript.

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)?
RESPONSE: This data was used only for the rough estimation of flow path persistency and therefore not much detail to the methodology was included. The estimation of the unsaturated hydraulic conductivities was done for the two most commonly found horizons, H1 and H2. These horizons can extend to a depth of 130 cm.

15. p. 2608 line 21: what is "back-of-the-envelope" calculation?
RESPONSE: "The phrase back-of-the-envelope calculations (abbreviated BotEC) refers to rough calculations that, while not rigorous, test or support a point. They are far more than a guess but far less than a proof. The phrase is generally used in mathematics, physics and engineering. It refers to the practice of quickly jotting down calculations on the nearest available piece of paper, such as the back of an envelope." (from http://en.wikipedia.org/wiki/Back-of-the-envelope calculation) However, to avoid misunderstandings we will call it "a simple estimate" in the revised manuscript.

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 relation-

ship 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. 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.

RESPONSE: Hydrophobicity generally increases with decreasing moisture content, which means that on the drier occasions we are more likely to observe patterns caused by repellency (time variable hydrophobicity). However, this does not mean that the entire soil surface or upper soil layers turn repellent - hydrophobicity is also heterogeneous in space (spatially variable hydrophobicity - small scale differences in water repellency (Ritsema and Dekker 1995)). These small scale differences in repellency can result from water redistribution by canopy, litter, microtopography or by variability in organic matter content (Ritsema and Dekker 1995, Dekker and Ritsema 1994). The O horizon in our study is very thin, but due to the higher organic matter content it is likely to cause redistribution of water due to spatially variable hydrophobicity.

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.

RESPONSE: Lateral flow was directly observed in a dye tracer experiment with an application intensity of 20mm/h. Here lateral flow occurred in the duff layer (Blume et al., 2007, Hydrological Processes, accepted). Furthermore we conclude from the short response times of streamflow that lateral subsurface storm flow must be of importance. Within the soil moisture study we observed that in certain locations we see soil moisture response while the sensors just above do not show a change in soil moisture. As water thus does not seem to reach the lower sensor vertically (where it would have been measured with the upper sensor), it must have been transported to the lower sensor by lateral flow paths. However, we do not now on what scale these thus "observed" lateral flow paths are active (several decimetres, meters or hillslope).

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).
RESPONSE: In this manuscript the term "runoff generation" includes all components of streamflow (groundwater, subsurface stormflow and surface runoff (which was not observed in this study). To avoid misunderstandings we will include this definition in the introduction of the article.

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).
RESPONSE: For event 3 probe 3 shows a strong response at a depth of 20 cm, however, transport to greater depth is still slower than at sensor 1 (sensor 2 only shows a reaction to a depth of 30 cm - below there is likely to be steady state flow. Whether this is no flow or steady state flow > 0 cannot be said from this data.)

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?
RESPONSE: Precise soil textural differences cannot be provided for each measurement depth (as already explained above). However, if there was a pocket of loose soil around the 20 cm sensor of probe 3, this sensor would not measure consistently higher moisture content than the sensors just above or below (the higher the amount of large pores, the lower the field capacity).

21. Table 2: Were forest 1, 2, and 3 correspond to P1 to P3 or not?
RESPONSE: No, these sampling locations do not correspond to P1 to P3. However the sampling was carried out on the same hillslope as the soil moisture measurements.

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.
RESPONSE: The part of the catchment boundary which can be seen on the right map is covered by the map symbol of the main stream gauging station on the left map. The location of the right side map is described in the figure caption as the slope at the catchment outlet. The claim of evenly spaced monitoring sites P1 to P3 will be corrected to "roughly evenly spaced". The other wells (apart from well 1) are used to explain hillslope characteristics (see section 2..1 p. 2592, ll. 23-25).

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,

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groundwater level, and soil moisture (as that done for the rainfall) so easier comparison among the three events can be made.
RESPONSE: We agree with the reviewer that these links indeed are valuable. In the revised manuscript a figure is included giving the lag times for each probe separately while relating it to the soil moisture annual variability at these depths. As for figure 2, the relation of stream and ground water dynamics to the observed soil moisture dynamics will be included in the interpretation of this figure. However, using the same color ramps for discharge, groundwater level and soil moisture for all events would obscure the event response for the smaller events (the ranges of discharge and groundwater level of the third event are too large). The same is true for the less pronounced soil moisture reactions.

24. Fig. 3: I assume the soil moisture data here also represent change relative to antecedent soil moisture content? Please clarify.
RESPONSE: The same type of plot was used here as in Fig.2. This will be clarified in the revised manuscript.

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.
RESPONSE: Adding precise depth labels to each of the sensors on the photographs will not be possible as the actual sensors are difficult to make out on the photographs. As mentioned above a summary of the soil physical parameters of the most common horizons will be included in the revised document. No trend in soil properties along the hillslope transect could be observed in the field. This might be due to the fact that these soils are very young and not well developed.

26. Fig. 5: Could shorter lag time of response in the summer season sug-

gest a more pronounced preferential flow in the catchment, probably caused by stronger hydrophobicity in the drier conditions?
RESPONSE: This possibility was described on p. 2602 ll. 7-11.

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.
RESPONSE: The general moisture profile including its variability is now included as a boxplot for each sensor into Figure 2. We therefore think that adding additional two line plots would not add a lot of new information to the manuscript. The marking of the drying periods will be changed in order to improve the legibility of the graph.

28. Fig. 7: The explanation of Fig. 7 is insufficient. What do we learn about hillslope 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?
RESPONSE: The most striking fact about this plot is that we are only able to see hillslope scale patterns at the 10 cm depth. For all other depths there does not seem to be a trend along the hillslope. More emphasis will be put on this fact in the revised manuscript. Concerning the shading effect at the lower end of the slope: The shading we are referring to, is shading from solar radiation due to topography. This shading will not have an effect on event response. In the revised Figure 2, where antecedent moisture conditions are included, it can be seen that median soil moisture at the 10 cm sensor is highest for probe 1, which is located at the lower end of the slope and is lowest for probe 3 at the upper end of the slope. The fact that we see the effects of hydrophobicity at probe 1 but not at probe 2, despite the fact that soil moisture content is higher at probe 1 cannot conclusively be explained. It might, however, be due to different organic matter content (not determined for these locations).

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.
RESPONSE: We agree with the reviewer that the locations of probes H4 and H5 in Figure 1 should be indicated. However, we are unclear on how showing the plots of directional variability for all sensors will help to interpret the data with respect to artificial air gaps? Plotting and describing the plots of all 11 monitoring sights would increase the length of the manuscript, while not adding much more information. As was mentioned in the manuscript, special care was taken to prevent air gaps during installation. Due to the fact that only very few stones were encountered in the first meter of soil, installation was relatively easy and the risk of air gaps due to dislocated stones is relatively small. While it is not unlikely that in certain cases air gaps occurred despite the careful installation, we do not expect this to be the case for all of the installed probes. Furthermore the presence of air gaps is likely to cause low soil moisture readings, but small scale variability was also found at higher moisture contents (Figure 8, H5 60).

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 pro- files 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"?
RESPONSE: Corrections 30-34 will be made in the revised manuscript.

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