

Interactive comment on “In situ investigation of rapid subsurface flow: Temporal dynamics and catchment-scale implication” by L. Angermann et al.

Anonymous Referee #1

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General comments

This paper presents very interesting data from a plot/hillslope scale sprinkling experiment combining TDR observations with time-lapse GPR to investigate soil moisture dynamics. Sadly, the promised (in the title) investigation of subsurface flow and the link to catchment scale is not convincingly presented, interpretations are often over-drawn and not supported by the results, and the paper currently reads more like a patchwork of data and ideas and not like a well aligned story. During the read, it felt as if the authors were not sure of what to present and what the key message of the work can/should be. The data is visually very well presented, but the actual analysis of the experimental data remains poor, eventually supplying temporal evolution of soil

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moisture (although the method combination gives an interesting depth distribution to 4 m below surface) and flow velocities for the first respond. Moreover, these velocities are not really used. From a water balance perspective it is interesting, if the occurring amounts of PF can be, by any means, linked to the generated runoff volume in events. I also think that the inclusion of one event based hydrograph separation is a little bit thin.

Stated by authors, the runoff ratio of 4% during the rapid peak (that consists of event water) cannot be explainable by direct rainfall on saturated areas. Nevertheless, the experimental observations and the fact that rapid flow downslope of the sprinkling site can be detected do not, in my opinion, convincingly explain the rapid stream response that is observed during rainfall in the study catchment. And this is why: I think that the method of the sprinkling experiment, with the use of a sprinkling intensity of 30mm/h, is simply not appropriate for investigating if preferential flow reaches the stream and generates the hydrograph under natural conditions.

I do understand that the purpose was to initiate the preferential flow with this rate, but I disagree with the authors that any explanation for the stream response can be done in this way. Such high intensities will of course lead lateral flow out of the sprinkling site as soon non-vertically oriented preferential flow paths are filled, or some low permeable lense or layer is reached. It would be interesting to report the natural rainfall intensities in the area and for the particular shown runoff event. As only a total of around 20 mm occurred. I am wondering if natural intensities would be indeed able to establish, supply, and connect a preferential flow system from the hillslope to the stream under unsaturated conditions on that time scale. This would be very important, since initiation of preferential flow strongly depends on intensities. I do not doubt that PF flow might be active at the hillslope and connecting to the stream in events with 30 mm/h rainfall intensity over hours, but when do such events occur? I think that there are no data and results provided in this manuscript that showed that preferential flow at natural rainfall intensities, amounts, and duration can explain the observed behaviour. Furthermore,

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early work of McDonnell and various follow up studies showed that much of the water in preferential flow actually is not event water. This adds further doubt to the authors' interpretation, since the observed peaks in the stream in the area are dominated by event water.

Leaving the interpretation of the catchment scale interpretation and the lateral preferential flow aside, the paper does not provide much novelty from a process perspective. The depth distribution data of soil moisture is interesting from methodological perspective, but work on this was published previously (see the Allroggen references). The discussion about the processes is not supported by the results of the work, and the methodological discussion is overly long, repetitive, lacks nearly any citations, and I was not struck by a major scientific message. That just leaves a case study of a sprinkling experiment, which is - in the current form - not sufficient to be published in HESS. That said, the dataset is very interesting and could supply interesting insights, but a clear idea of what should be presented, and what conclusions can be supported by the data is needed. Following, an in-depth analysis is needed that goes beyond the very nice visual presentation of soil moisture data and calculated flow velocities.

Detailed comments: The syntax is often awkward (except in the introduction) and could highly benefit from a very careful revision (or editing by a native speaker). P1L10: "hillslope-scale connectivity". There is no evidence provided for connectivity. There is evidence that some water moves downward from the sprinkling site. This neither indicates connectivity in this case nor connectivity under natural conditions. P1L11-12: "These processes" is somewhat vague. P2L13: Change: "is on" to "was on" P2L26: Change: "At minimal..." to: "GPR provides..." P3L7ff. I am not sure, if introducing multi-modal transit time distributions here and in the following parts is beneficial for the manuscript. If the authors introduce it and eventually mention it at several points, they should start calculating them for the mentioned events, rather than doing a simple two component end member mixing (although this seems not to be possible with the isotope sampling). References are needed after "flow process". P3L16ff. References

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for the claims are needed. Why vague? The research need that is outlined here, is not convincingly derived and link to the majority of the introduction. The introduction exclusively deals with preferential flow processes, before some double-peaked hydrograph behaviour is mentioned. After that, the research needs is only related to these hydrographs. As mentioned earlier, I miss the well thought out story line. I also missed references to important connectivity work, since the connectivity (hillslope-stream) part seemed to be an important part of the paper. P6L10: What is the uncertainty that is associated with this very simple hydrograph separation, especially when accounting for the high variability of stable isotopes in rainfall? P7L3: Please provide the relative location of the irrigation site, when hillfoot is 0 and hilltop is 1. What is the length of the hillslope? Language and syntax are improvable. P7L10: What is the return period of the sprinkling event (intensity, amount)? P7L11: Here it is stated that it is not intended to mimic natural conditions, but then the authors used the results to infer flow processes under natural conditions. This does not fit together. P7L17ff: Information about size and height above ground of the samplers are needed. What were the intensities of the natural rainfall events? This could be a good comparison to the experiment. P9L26-28: "The..." Better delete? Not needed. Another suggesting: better not start sentences with "Figure 2 shows/presents/" or "This/that is presented in Figure 2"; rather report the finding or what the reader show see and then reference the figure. P10L1: "Similar". So what is the difference to the cited work? P10L14: "suggested" Is certain or uncertain? I am also wondering where the separation between mobile and stagnant water was made? P10L31: Why was the 2% value chosen? P10L4ff: "were calculated for the entire depth": Do you mean, that if you give the value for 10 cm it is the velocity for 0-10 cm, if you report the velocity for 20 cm depth, it is the velocity from 0-20cm? P10L9: "main stem". Delete? P12L13ff: How are these hydrographs separated? What method was chosen? This needs to be mentioned in the methods. What are the uncertainties associated with the isotope hydrograph separation? Why was only one event chosen for analysis? P13: Strange font size/type of "20 mm" in the figure captions of fig.3. P13L2: 0.5 L. This was over the whole experiment? So more or less unimportant in

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the water balance. P13L2 “relatively”. Please provide a objective description, rather than using subjective terms like small, relative, best with numbers. Do that throughout the manuscript. P13L8: Can these 20% be the uncertainty of the water balance? Or what is the actual uncertainty and how does that influence the results and interpretation? I am not sure what “average storage deficit of 20%” means. P14: Fig4. Uncertainty needed. P15L4-7: Move to methods. P15L13: “generally low dynamics”. Please change P15L31: “all fast dynamics” P15L32-33: Is there a possibility to differentiate if it is in the matrix or left the monitored area? How about working more with the water balance? P17L2: This is more discussion of processes then results. How was this inferred? It seems like a statement, but not supported by data. There is no evidence for this. P17L4: Again, there is no proof for this. It is just speculative. How deep below the soil surface is the bedrock? P18: Can velocity distributions be estimated? They might be more interesting/informative than just the first response time. One could see how the majority of the water moves, as the response time of the preferential flow should be somehow consistent with the time of the hydrograph response, especially if the preferential flow should explain the delivery of event water to the stream. P22L6: This is commonly observed. Soil moisture variability increases with decreasing moisture contents P22L12: “high portion of mobile water”. Please quantify. This is important P22L13: “Velocities over...” But this only relates to the fastest component. The water itself should travel with a velocity distribution, which was not estimated. I would argue that the median or average speed to the “mobile” water (the matrix water is mobile too) is $<10^{-3}$ m/s. Give the numbers for MaiMai. P22L16ff. This is true, but it still does not show water transport of volumes to the stream that might explain the hydrograph response. Is there impermeable/less permeable material below the field site? How deep is the bedrock? Was an attempt made to observe the subsurface stormflow there? In addition: The activation of the preferential flow is highly dependent on the rainfall intensities. They might never be active under natural conditions when rainfall intensities are an order of magnitude lower (over event scale). P22L24-25: Connectivity at hillslope scale was not shown in this experiment. The data simply showed that

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some preferential flow out of the sprinkling site occurred. P22L30: Every hydrograph is likely a result of multi-modal distribution of travel times. P23L20: How were the riparian zones and wetland patches delineated? What is the uncertainty? Is there a contraction/extension during events that could change this? P23L22ff. Again, there is no evidence shown in this work that there was any connectivity between hillslope and stream. This is even more true when natural conditions are considered. P24L26ff. This methodological discussion is overly long, repetitive, and lacking references to work of others. P24L27-P25L3: Here I disagree. As said, the experiment was not designed to mimic natural conditions. This is completely fine, since it seems to me that the original idea was to employ the GPR system. Nevertheless, the setup does not allow inferring natural flow processes that explain hydrograph response. P25L10: Explain: “sufficiently steady state” P25L24: Sat. conditions are most likely occurring at the bedrock interface. Technically the periglacial layers are not “soils” anymore. Try to have a look at the work of Uhlenbrook et al. on periglacial slopes. P27L31ff. This seems to be unnecessary - no important point was made in this section. P29L4: The resulting connectivity was not shown and remains highly speculative. P29L5ff: This is speculative. Furthermore, the link to transit time distributions is somewhat strange. P29L20: Again, I think this was not shown here.

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