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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Predicting subsurface storm flow response of a forested hillslope: the role of connected flow paths and bedrock topography" *by* J. Wienhöfer and E. Zehe

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We thank the Anonymous Referee 1 very much for the comments on our manuscript. Our responses to the specific comments are detailed below.

Comment 1: In the Introduction (Section 1), a few lines on what is still missing in our conceptualization and understanding of preferential subsurface flow processes should be added.

This is a valuable suggestion, which we will pick up for the revision of the paper.

Comment 2: 6481, 19-21. "This analysis indicated, however, that the tracer uranine was not retarded compared to conservative salt tracers". Data of conservative salt tracers are not shown (at least they are not described previously in the section). Please, specify.

What we want to state here is that we have indication that retardation was not an important factor in transport of uranine at the hillslope scale. The tracer experiments at the hillslope scale included applications of sodium chloride closer to the measurement location (8.2 and 16.9 m along the slope surface). These were done after the application of Uranine which had been selected for this study as it was the first stage of the experiment, and also represented the longer transport distance (28.2 m along the slope surface). As retardation of uranine, e.g. due to reversible adsorption, could not precluded a priori, we checked for possible retardation by optimizing a retardation factor in a one-dimensional convection-dispersion model, fixing other parameters by taking into account the breakthrough of sodium chloride at the hillslope scale and the findings on uranine and sodium chloride breakthrough in a soil column experiment with an undisturbed soil block (surface area 0.25×0.25 m, depth 0.35 m). This analysis, which is detailed in Wienhöfer et al. (2009), indicated that at the hillslope scale the tracer uranine was not retarded compared to the conservative salt tracers. In a revised manuscript we will simplify the statement and also refer to the experimental paper.

Comment 3: 6489, 4-7. Please, explain why the occurrence of surface runoff is invoked from the observation of Fig. 3f: couldn't simply be interpreted as overestimation of subsurface flow?

The Fig. 3 displays total hillslope runoff, which is the sum of surface and subsurface runoff. In the case of Fig 3f, this total runoff is completely composed of surface runoff, and thus the figure is intended to illustrate the surface runoff dynamics simulated by the model. In a revised version, the reference to Fig 3f will be placed at the end of line 9, and the caption of the figure will be expanded to explain this better.

Comments 4 - 6: C: 6491, 1-22. In this Section (4), some concepts are repeated (e.g., lines 1-6, 7-9) and should be avoided. Moreover, the whole section is more methodological than discussion. So, I suggest compacting it and moving it to ones of the previous (methodological) Sections. C: 6493, 11-14. These two sentences are a little too vague and should be expanded and discussed a bit more. C: Fig. 3. Peak discharges produced by the sprinkling experiments and by natural rainfall should be distinguished and indicated in the Figure panels.

These are, again, valuable suggestions, which we will pick up for the revision of the paper. We are also very grateful for all the minor and technical comments, and will consider the suggestions during the revision of the manuscript. We thank the reviewer very much for the time reviewing our manuscript and the useful feedback.

References

Wienhöfer, J., Germer, K., Lindenmaier, F., Färber, A., and Zehe, E.: Applied tracers for the observation of subsurface stormflow at the hillslope scale, Hydrol. Earth Syst. Sci., 13, 1145-1161, 2009.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 6473, 2013.