

## ***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***

**Anonymous Referee #3**

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Comments to Wienhoefer and Zehe General comment: This paper presents a modeling study using a 2D physics-based model to reproduce observed runoff and tracer breakthrough on a mountain hillslope. Specifically, the authors investigate the effect of explicitly incorporating preferential flow paths in the modeling domain on simulated water flow and tracer transport. They examine numerous scenarios of stochastically generated preferential flow path configurations combining vertical and lateral structures with different sizes and spacing. In addition, they also examine the effect of bedrock topography and soil depth. Their main finding is that of “structural equifinality” – several different setups of preferential flow path configurations successfully simulated water

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flow whereas tracer breakthrough was reproduced by neither of the scenarios. The authors reason that also the a priori perception one may have about the relevant structure of a flow domain (hillslope, catchment) which then dictates which structures are considered in a model influences the outcome and interpretation of simulations. This is definitely an interesting study. The model they use, CATFLOW, has been used before by a number of authors (all from the same group) for simulating flow and transport at the hillslope to catchment scale. The modeling approach is quite comprehensive with the explicit description of preferential flow paths and the testing of many different configurations and combinations. The reference list documents a very good knowledge of the existing literature. This study builds on previously published work, and at some instances more information on the site and methods is needed for the reader to better understand what was done (see specific comments). One drawback may be that the paper in the discussion section elaborates in great detail on modeling strategies and numerical aspects, particularly the implementation of solute transport, which may not be of interest to a great number of people and does not really help with the main message of the paper. While I think that this is an interesting approach and well done I don't fully agree with how the authors interpret the results and draw conclusions. At the end the reader is somewhat left to wonder if the incorporation of macropore-like features makes sense and how to deal with the structural equifinality. I am not convinced of the last sentence of the abstract after reading the manuscript – that distinctive flow paths should be considered explicitly. I am not surprised that the explicit incorporation of preferential flow features improves simulation results. But how do I describe those structures at individual sites? The simulations indicate that there are many degrees of freedom. Our information on the subsurface flow network will always be incomplete and thus the representation of preferential flow pathways will be arbitrarily and random to some extent. How can I incorporate macropores if I don't know their size and spatial extent and connectivity and if different setups yield very similar results? Which setup should I choose? Although I agree that our perception of dominant processes shape the way we set up our hydrological models I would argue that we then

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need more field evidence (soft and hard data, qualitative observations, different data types) in order to choose a suitable configuration and to reject other equifinal setups. The five setups that provided acceptable water flow simulations differ markedly in how they describe the flow domain (lateral pathway yes/no, bedrock present yes/no) and thus, different runoff generation mechanisms are happening. If those simulations were used to learn about the functioning of the hillslope, different outcomes would be the result. The breakthrough of the tracer was not simulated well in any of the scenarios. The authors state in the conclusions that “this can readily be attributed to the incorrect representation of the spatial dimensions of the..structures which led to an underestimation of.. velocities”. This sounds as if the authors could easily fix this problem by running some additional simulations? If this is the case I would recommend to include those additional simulations to corroborate that statement. I recommend publication of this manuscript after minor (to major?) revision, mainly to better explain which consequences the structural equifinality may have when we set up a model of a site. Specific comments - Abstract: mention in the first paragraph that you were also testing the effect of soil depth variability; otherwise the statement in line 25 is unexpected - The abstract does not mention the identified “structural equifinality” of the five suitable setups and its consequences – an aspect that the authors elaborate in great detail on in the discussion and which in my opinion is the main finding. In contrast, the conclusion in the last sentence is not in line with the discussion and is not a conclusion I would draw after reading the manuscript (see comments above). - p.6475, L 25: reference Zehe and Sivapalan is not in reference list - p. 6479, L 12: better “the vegetation is dominated by loose stands of..” or “consists of” - p. 6480, L 2: reached - Site description: information on bedrock material is missing (geology, minerals, permeability, fractures etc.) - p. 6480, L 21: how was lateral flow observed in these different pathways?? This is important! - p. 6481, L 1: where and how was discharge (subsurface flow?) measured? - p. 6481, L 24-28: not quite clear what this means, please rephrase - p. 6485: which are the five structural features? I find the description of the the implemented structures and the resulting combinations somewhat confusing. Maybe mention clearly at the begin-

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ning of this section which were the five basic preferential flow features that were varied and combined before you start describing how they were generated. - p. 6487, L 7: “..whereas the value for..” - p. 6487, L 12-13: “..during which there was only input at the four experimental plots” - p. 6487, L 21: instead of right boundary “at the toe of the slope” - p. 6488, L 16: 65 simulations? on p. 6485, L 25 it says 64, and total number was 122. . . ? - p. 6491, L 14-15: I don’t think that soil “types” is the correct term; maybe layers or material - p. 6491, L 19: delete “flesh out”... - p. 6492, L 14: was used - p. 6492, L 10-19: but that is no proof for the correct implementation of structures; maybe layers of different soil material would have generated a similar flow behavior (although soil layering can be considered “structures” already - p. 6492, L 23-26: this is a somewhat vague result – some kind of lateral and vertical structure is needed – how does that help with setting up a model of a site? Is it sufficient to just incorporate one vertical and one lateral flow path, irrespective of site-specific conditions? - p. 6493, L 12: could be due to (2x) - p. 6494, L 9: soil hydraulic.. - p. 6494, L 15: ruled out - p. 6495, L 27: this is an unrealistic - p. 6498, L 8-11: I do not agree with this conclusion. If my only interest is to get the hydrograph right, ok. But usually one also wants to learn from modeling. These acceptable scenarios represent quite different perceptions of the hillslope! - p. 6498, L 16-17 and L 24-26: at Panola, however, there is additional evidence for the role of bedrock topography in controlling connectivity – the relation between bedrock topography and spatial distribution of trench flow, measured saturation patterns, measured transient water tables that indicate a cascading response (the fill and spill idea); so it’s not only the perception but rather vice versa, the observations that led to the conceptual model

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

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