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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 reviewer 3 very much for reviewing our manuscript and for the helpful comments. In the following, we would like to address the reviewer's major comments in detail.

General comment: 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.

We agree with the reviewer that the discussion on modelling aspects could be made more succinct. In a revised manuscript, particularly the discussion of the implementation on solute transport will be rewritten as we plan to include updated solute transport simulations (please see our response to the third general comment below). As one objective of the paper is to investigate the modelling approach with explicit representation of structures, we think, however, it is necessary to discuss some modelling aspects that either relate to the interpretation of the results, or might be useful hints for future attempts using the approach.

General comment: 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 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.

Perhaps we have not formulated our conclusions very clearly, also in the abstract, and we would like to clarify our reasoning. The field observations suggested that preferential flow in a network of connected structures could be the reason for the observed hillslope response at the investigated hillslope. Therefore, it was not completely surprising that the incorporation of preferential flow features in the model provided satisfactory results, as this corresponds to the perceptional model we gained from the field observations. Nevertheless, it was not clear beforehand if the approach and the simplified setups could capture the observed hillslope response. We think our study has demonstrated that on the one hand, this approach works to certain degree, and on the other hand, preferential flow in connected structures is a plausible explanation for the observed hillslope response at the study site.

Certainly the incorporation of preferential flow in a model of a site where preferential flow in structures is important does not only make sense, but is definitely necessary. Of course, this can be achieved in several ways, of which the explicit consideration of distinct structures is only on example. We believe that the explicit consideration of flow paths can be especially helpful for using models as learning tools or for 'virtual experiments' to understand possible controls on hillslope hydrology, while in the past virtual experiments often have neglected preferential flow in structures.

The reviewer is right that there are typically many degrees of freedom in a hillslope hydrology model, especially when it is spatially explicit. We also fully agree that all available evidence should be used to set up a model and reduce the degrees of freedom. In our study we have tried to pursue this approach by keeping things fixed for which we had some data (e.g., soil matrix parameters, topographic gradient, soil depth along the slope line), while we used other observations, e.g. from dye-staining experiments at the plot scale, to guide our conceptual model of preferential flow paths at the hillslope scale. As we did not have information on the exact arrangement of flow paths, we chose to generate different realisations, also including quite contrasting scenarios. Testing different realisations will be required as long as our information on the subsurface flow network (or other structures at the hillslope scale) is incomplete, and, as the reviewer correctly points out, this will probably always be the case. Equifinality is a possible consequence of testing different (equally likely) scenarios, because there might be several scenarios with the same explanatory value for the tested hillslope response.

The number of equifinal setups will increase with the number of "similar" setups that are tested. Assessing the similarity of equifinal setups in contrast to unsuccessful setups can help in learning about possible configurations of the system under investigation. If complementary information is available that has not been used during model setup, the equifinality could possibly be further reduced, and hence the picture of the investigated system will become finer. For example, the different configurations could be tested for different long-term behaviour, if long-term data are available for comparison. We agree with the reviewer that efforts to reduce equifinality should be made, but even then we possibly end up with some equifinal configurations. We do not, however, consider this a disadvantage of the approach; because it also means that the configuration of subsurface flow paths at the hillslope scale does not need to be known that exactly. If we would need to implement into our model an exact representation of the hillslope at the centimetre-scale, we would certainly hardly ever be able to use spatially explicit models at all. On the other hand, simulations with equifinal model setups could also be used in an ensemble approach to assess the range of possible system behaviour in the light of uncertain model setups.

General comment: 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.

The reviewer is right that it is much better to include updated simulations than to merely discuss possible workarounds. An error made in haste appeared to have thwarted an earlier attempt to consider the factor representing the macroporous cross section, as discussed in the first version of the manuscript. In the meantime, we have revisited our code and fixed this in the calculation of the transport velocities from flux densities. This modification considerably accelerates tracer transport times in the models. Admittedly, the modification of the model does not solve all issues. A reduced cross-sectional area of structures could also lead to a reduced infiltration of tracer solution into these structures and hence a lower fraction of tracer transported into these structures. But this transition is much more complicated to handle in the model. It is not done with a nodewise factor as for the velocity calculation, and probably a dual-domain approach would be necessary to split the amount of solute between the structure domain and the matrix domain. We would like to include these new results and this discussion in a revised version of the manuscript.

Specific comment: 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).

We agree that variable soil depth and equifinality should be mentioned in the abstract, and will consider these suggestions when revising the abstract. We would also like to refer to our response to the second general comment above.

Specific comment: 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.

We agree we have to improve this discussion on the effect of contrasting configurations on the simulated hillslope behaviour. The tested configurations included different setups with a (conductive or less conductive) soil layer on top of bedrock, or with a (thin or thick) litter layer on top of soil matrix, which resemble some kind of layered soil profile. Of course, we cannot exclude that some configuration we have not tested would give similar or even better results, although we tried to cover quite a range of configurations

constrained by the available field observations. From the range of configurations we tested, however, only setups with vertical and lateral flow paths were successful.

Specific comment: 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?

Of course we do agree that site-specific conditions should be accounted for as much as possible. For this specific study site, a connected network of several vertical and at least one lateral flow path was necessary to provide successful simulations within our modelling approach. At the end, we could not assess an optimum number of vertical pathways, or an optimum location of the lateral pathway (within the soil, or directly above bedrock), but by contrasting the successful setups with other tested setups, the results corroborate the importance of flow in a connected network of structures at the study hillslope. We think it is straightforward to apply this approach similarly at other sites where flow in connected structures might be important. We would highly recommend using all available information for guiding the development of the conceptual model of that site, on which the exact specification of individual configurations (or the range of possible arrangements) will have to be based.

Specific comment: 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! / General comment: 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.

This conclusion on structural equifinality was originally drawn by Weiler and McDonnell

(2007). It is cited here because we think our results seem to corroborate their findings, in the sense that the incorporation of vertical and lateral flow paths is of primary importance, whereas the detailed configuration of these flow paths does not have to be exactly met. In our simulations, for example, there is not such a big difference in the hillslope response whether a lateral flow pathway that is quasi-parallel to the surface topography is situated at 0.45 m depth within the soil matrix or at 0.85 m depths above the bedrock ("run119" and "run120" in Table 2 of the discussion manuscript). As already discussed in the response to the second general comment above, this is in our view rather encouraging, as the geometry of subsurface flow paths will hardly be known in full detail at the hillslope scale. One should, however, have evidence for the assumption that flow paths are present at the specific site, or use a model to test if flow paths could be a possible controlling factor.

In our opinion, the successful scenarios are not differing so much in the dominating processes they represent. In all these cases, fast vertical and lateral flow in preferential flow paths is the major component of hillslope outflow, and the presence of these flow paths thus is the primary control of hillslope response. That contrasts with possible other controls, like variable saturation patterns controlled by bedrock topography (the fill and spill idea, also see below). Admittedly, the aim of the paper is not to comprehensively test and weigh all possible controls on hillslope hydrology, but we believe this case study encourages consideration of preferential flow paths in models of sites where these might important in order to discriminate their role against other factors. In our opinion, this will also help to learn more from modelling studies using hillslope hydrological models.

Specific comment: 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 obser-

vations that led to the conceptual model.

We agree with the reviewer that detailed field observations are essential to build and test hypotheses of how hydrological system work. The efforts made at the Panola research site have without doubt provided an impressive and very useful database, which has been used for a number of excellent studies that have added significantly to our understanding of hillslope hydrology, and especially the possible role of bedrock topography. These works have also motivated us to do measurements of soil depth at our study site, and implement the variable bedrock topography in our model. The 'fill and spill' mechanism referred to by the reviewer, however, might be of greater importance at sites like Panola than at our study site. The Panola hillslope is less steep (13°; Tromp-van Meerveld and McDonnell, 2006) than our hillslope (18° to 54°), and the soil matrix is much more coarse-textured and permeable (saturated hydraulic conductivity $K_s \approx 1.8 \times 10^{-4}$ m s^{-1}; Hopp and McDonnell, 2009) than at the Heumöser study site ($K_s \approx 1.8 \times 10^{-7}$ m s^{-1}). This means that at Panola it is much more likely that infiltrating water percolates down to the bedrock and builds a water table at the interface to the less permeable bedrock. The gradient of this water table, which in turn is determined by the interplay of percolating water and bedrock topography, then drives subsurface flow processes. At the studied hillslope at Heumöser, infiltration and percolation were observed to occur in macroporous structures at the plot scale. We did not know a priori if these structures funnelled the flow onto the bedrock surface, from where it then would flow downslope in some sort of permeable interface or other, laterally extending structures, or if the flow was funnelled directly into a network of connected flow structures. Our model results support the latter hypothesis. If flow is directed into a network of pipes before it percolates to the bedrock, the geometry of the preferential flow network will determine the driving gradient much more than bedrock topography. That is why we propose to consider possible preferential flow paths in modelling studies that explore controls on hillslope hydrology. Pipe flow is also an important factor at the Panola research site, which delivers over 40 % of total hillslope outflow (Freer et al., 2002; Tromp-van Meerveld and McDonnell, 2006). Modelling studies that made use of the dataset, however, have not included preferential flow paths in their spatially explicit models (Hopp and McDonnell, 2009; James et al., 2010). In our opinion, this might be a direction for further research.

Specific comment: Site description: information on bedrock material is missing (geology, minerals, permeability, fractures etc.)

We will add information on the bedrock material.

Specific comment: p. 6480, L 21: how was lateral flow observed in these different pathways?? This is important!

Vertical and lateral flow paths were identified after staining with infiltrated dye and excavating, and during excavation of soil blocks for lab tests. Flow from pipes was also observed (visually) at the cut-bank.

Specific comment: p. 6481, L 1: where and how was discharge (subsurface flow?) measured?

The seepage from the cutbank was funnelled into a temporarily installed V-notch weir, which was equipped with a pressure gauge to record water levels. The second measurement mentioned here was the measurement of the spring discharge further downslope. As these data are not used in the present study, it might be better to focus the description on the measurements made at the cut-bank, and we will revise the paragraph accordingly.

Specific comment: p. 6481, L 24-28: not quite clear what this means, please rephrase

The recovery of uranine in a soil column experiment with an undisturbed soil block (surface area (0.25 m)^2, depth 0.35 m) was only 22 % after eight days leaching, indicating that at maximum 22 % of the input mass should be expected to be mobile and able to be recovered at the hillslope scale, provided that the outflow from the hillslope was sampled completely. We will attempt to present this more clearly in the description of the experimental data.

Specific comment: 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 beginning 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. 6488, L 16: 65 simulations? on p. 6485, L 25 it says 64, and total number was 122?

We will improve the description of the model setups to clarify the number and details of the configurations that we have tested.

Finally, we also gratefully acknowledge the other, more technical comments and suggestions, which we will consider for revision of the manuscript. We would like to thank you again for your time and effort reviewing our manuscript and providing helpful feedback.

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