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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 reviewer 2 very much for the time reviewing our manuscript and for the helpful comments, which point out a number of important issues that require further explanation. In the following, we would like to address the reviewer's comments in detail.

General comment 1: However, some of the data used in this modeling study are not described very clearly. / Specific comment 3: Section 2.1.2: Give more information on the tracer and rainfall simulation experiments and refer to figure 1 when needed (e.g.

P6480L27 and P6481L10). [. . .]

We will include the requested details in a revised manuscript to give the reader a better understanding of the data that were used in the paper. We will also point out that we have chosen a subset of the available tracer data for the modelling study as a start, and refer to the experimental paper for the full details of the tracer experiments (Wienhöfer et al., 2009). We have chosen the data (experiment 'Uranine 1' at measurement location 'cut-bank') because i) it was the first tracer application during the experiments with steady-state flow, ii) resulted in a smooth tracer breakthrough curve in contrast to the measurements taken at the measurement location 'spring', and iii) represented the longer transport distance (28.2 m along the slope surface), compared to subsequent applications of sodium chloride closer to the measurement location 'cut-bank' (8.2 and 16.9 m along the slope surface). The additional data could potentially be used to further narrow down equifinal model setups (see response to minor comment 6), but this is beyond the scope of the present study.

Two points from the experimental data shown in Wienhöfer et al. (2009) are important here: i) The comparison with sodium chloride indicated that uranine transport at the hillslope scale was not subject to retardation, hence we did not consider a retardation factor in our transport simulation, and ii) the recovery of uranine in a soil column experiment with an undisturbed soil block (surface area $(0.25\ \mathrm{m})^2$, 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 attempt to present this more clearly in the description of the experimental data, and expand the discussion on tracer recovery in a revised manuscript.

General comment 2: The different models were probably tested in a systematic way but this is not clear from the text as the model results are not presented in a systematic way. / Specific comment 8: Section 3.1: It would be helpful to present the model results of the 64 model structures in a more systematic way. One way would be to create a *matrix [...] to indicate which models fulfilled a criteria and which did not.*

We agree that the presentation of the results can be improved, and thankfully accept the helpful suggestion of preparing a matrix to present all the results.

General comment 3: It is furthermore unclear why in addition to the 64 models that systematically test the variables given in table 1, there are also other model simulations that are not part of the systematic test and it is especially not clear why these model variants were not part of the test from the start. Instead they seem to be an ad-hoc addition, which makes the presentation of the result a bit messy and the testing of the model structures appear a bit unstructured / Specific comment 6: P6485L25-28: It is not clear why these additional setups weren't part of the systematic analysis of the different setups. Explain this better in the text. On P6486L1, homogeneous setups (several?) are mentioned but on L25, there is just one. On P6489, it appears that there were 3 homogenous model setups (one with the soil parameters, one with the whole model similar to the litter layer, and one with the whole model similar to the structures). Explain the number of models better to avoid confusion. On P6488L16, 65 models are compared (64 + 1 homogeneous?) and only 5 fulfilled the model criteria - it appears from the remainder of the section that the other configurations (nr 66-122?) don't change the simulations a lot. But on P6474L10 only 5 of the 122 models fulfilled the model criteria. In Table 3 and 4, it may appear that the simulations that fulfilled the model criteria were not part of the initial 64 systematically studied model setups as their number is >65. This makes it seem that the simulations were not carried out in a systematic way. Improve the description of the simulations.

We admit that our description of the simulations is misleading in some aspects, and we agree with the reviewer that this needs to be improved. The focus of the modelling study was to test if a model with explicitly represented preferential flow paths could simulate the observed hillslope responses. Because the configuration of preferential flow paths was not known, we tested different conceptual representations of flow path structures, which were combined with the known topography of the surface and two

different representations of measured soil depths (constant and variable). In doings so, we seek to vary the configuration of 'structures', while keeping fixed what we assumed to be known, e.g., surface topography, soil parameters, or rainfall input. We chose an initial set of configurations and tested all possible combinations of these. These combinations were termed "systematic model variants" in the discussion paper, which admittedly is an unfortunate formulation and would be better termed "basic" or "base case" model variants.

These 64 setups (32 with variable and constant soil depth, respectively) were complemented with several modifications of these setups. It is correct that these modified setups were not generated systematically in the strict sense that all possible combinations were tested. The modifications, however, were made in a directed way in order to investigate the effect of a modification in comparison with the results from the "basic model variants". For example, we wanted to investigate the effect of very densely arranged vertical flow paths having an average spacing of 0.5 m, and tested systematically all possible combinations with the other variables and a variable soil depth. By comparing these simulations with the "basic model variants" that also used variable soil depth, we could already judge how a more dense arrangement of vertical flow paths influences the results, without also having to test all possible combinations using a constant soil depth. Similarly, we tested the effect of widening the structures in lateral direction (lateral pathway, soil-bedrock interface, litter layer) from a thickness of one node in the basic setup to two or three nodes, and we tested all possible combinations of these with a variable soil depth. As none of these modifications improved the results of the basic configurations, it did not appear necessary to systematically test these with a constant soil depth.

Finally, "homogeneous" setups completed the set of tested configurations. One setup is sort of a zero combination in the sense that it contains none of the basic structural elements, i.e., the soil matrix parameters were used for the entire model domain. Other setups attributed as "homogeneous" in the discussion paper were setups with bedrock as only structure, i.e. a homogeneous soil mantle, for which the parameters of the soil mantle were changed. These were tested with both constant and variable soil depths.

We will improve the description of the model setups to clarify the number and details of the configurations we tested, and we will present the results of the additional model variants in a similar way as suggested for the basic model variants (specific comment 8, please see above). Another possible source of confusion probably is an unfortunate choice of identifiers for the different simulation runs, which are not numbered consecutively, although this might be suggested from the pattern of the identifiers. The 64 basic runs were originally numbered 001-032 and 101-132. To avoid such confusion, we will change the identifiers to a letter-and-number combination.

General comment 4: One of the conclusions of this modeling work is that bedrock topography has a secondary influence on modeled hillslope outflow. However, the effect of bedrock topography was not tested in a systematic way and therefore one cannot conclude this based on this study. Only two model runs (uniform soil depth and variable soil depth) are compared. Soil depth may have exerted a larger control on modeled hillslope outflow if the variability in soil depth was larger (and the bedrock topography thus rougher) than in this model (see comment 1 and comment 10). / Specific comment 1: P6474L25: The effect of spatial variability on hillslope runoff is not shown in this study. Only two models are compared. There was no systematic study of the effects of variable soil depth on modeled runoff and this statement thus overstretches the results (see also comment 10).

The reviewer is right that our study is not a comprehensive analysis of all possible effects of variable bedrock topography on hillslope response, which certainly is beyond the scope of our paper. We would therefore agree to remove "bedrock topography" from the title of the paper.

We would also refrain from concluding that bedrock topography generally has no influence on hillslope processes, and this was not meant in our conclusions. The importance of bedrock topography has been highlighted by a number of studies, for example those made at the Panola research site (Freer et al., 2002; Hopp and McDonnell, 2009; Tromp-van Meerveld and McDonnell, 2006). We therefore considered it necessary to include variable soil depth in our model, and that's why we made the soil depth measurements at our site.

We would, however, also tend to partly disagree with the reviewer in this point. In our opinion, the results do suggest that variable bedrock topography is secondary at the study hillslope, and we would like to elaborate on this point and document it with additional information. In a revised manuscript, we would like to include the discussion of these points, and clarify our conclusions.

The comment that bedrock topography was not tested systematically is perhaps also partly evoked by the lack of clarity in our description of the model setups (see response to general comment 3 and specific comment 6 above). We did compare more than two model runs, as all but one of the setups were made with either the constant or the variable soil depth. It is correct that we did not take other representations of soil depth into account, and possibly a larger variability in soil depth could have exerted a larger control on modelled hillslope outflow. We used, however, actual measurements of the soil depth to model the variable bedrock topography, and hence there is little scope left for assigning a larger variability of soil depths in our models. Admittedly, the measurements were interpolated and projected onto the 2-D slope line. We would agree if the reviewer argues that a comprehensive analysis focussing on the effect of variable soil depth would have to investigate the influence of different interpolations and projections, or consider using a 3-D model, which again would possibly give rise to a number of additional numerical issues. As this is not quite the focus of the study, we chose a 2-D model, implicitly making use of a symmetry assumption to simplify the problem. We implemented one representation of the interpolated measurements that captures the observed bedrock depressions (assuming that flows lateral to the slope line would take the route to the maximum depth), and which we consider adequate and representative for our purposes.

One question behind our modelling study was: Do we need the variable bedrock topography in our model? Or can we use a constant soil depth, i.e. a bedrock topography that resembles the surface topography? The results show that there is a difference in modelled hillslope response between these two configurations, but in our case, the setups with constant soil depths performed better. Likewise, setups which would be supposed to come nearest to the 'fill and spill' idea (Tromp-van Meerveld and McDonnell, 2006), did not match the observed response, for example the "homogeneous" setups with a variable soil depth and a conductive soil mantle, or setups with vertical flow path and a conductive structure (soil-bedrock interface) along the variable bedrock topography. We agree that this only partly justifies the mentioned conclusion, as we cannot exclude that setups we have not tested would provide similar or better results with the variable bedrock topography.

More important, our results show that when connected vertical and lateral flow paths are present within the soil mantle, these clearly dominate the modelled hillslope response. Water infiltrates into the vertical structures and flows down the hillslope in the lateral structure, while the saturation at the bedrock interface is not changing with the same magnitude. To exemplify this, we would like to add a figure showing the development of relative saturation in a simulation with vertical + lateral structures, litter layer, a soil-bedrock interface and variable bedrock topography (please see supplement for an example). This indicates that with the presence of structures above the bedrock, the role of the bedrock topography becomes secondary at our study hillslope. Generally, it is the geometry (topography) of the dominating structure that determines the water table gradient and in turn the flow response of a hillslope. This could be bedrock, but also a preferential flow network.

The field observations and our modelling results thus suggest that preferential flow in a network of connected structures is the most plausible explanation for the observed hillslope response at the investigated hillslope with its rather shallow, fine-textured soils,

while in the specific setting of the hillslope, the observed variability of soil depths is not necessary to explain the hillslope response.

We therefore propose that the effect of preferential flow paths should be included in future modelling studies that systematically analyse the controls on subsurface stormflow. For example, Hopp and McDonnell (2009) excluded pipe-flow and pipe-flow observations from their modelling study, although during the event that was chosen for calibration, pipe flow was contributing nearly half (45 %) of the observed hillslope outflow (Freer et al., 2002). We think the incorporation of preferential flow structures in this kind of numerical studies would be a valuable extension.

General comment 5: However, more discussion on how this work and how these results compare with other hillslope model studies (e.g. the Weiler 2004, 2007, 2008, Hopp 2009, James 2010, Ebel 2007 studies) or a discussion on what was learned from a hydrological process point of view would be useful as well.

This is a useful suggestion, which we will gladly consider for revising the manuscript.

Specific comment 2: P6480L11: Give some information on the size of these soil columns. How big is big and how many different columns were used for the measurements?

Three soil columns were taken in the field. Two rectangular columns with 0.3 x 0.3 x 0.8 m^3 were used for constant head permeability tests. The soil parameters used for the soil matrix in this study were determined with multistep outflow experiments only on one column with 0.3 m diameter and 0.72 m height, which we have to correct in the methods section.

Specific comment 4: P6485L9: Why was 0.9 m chosen as the final depth? Is this based on observations or literature values? Give a justification, also for why this standard deviation was chosen.

We chose a mean depth of 0.9 m and a standard deviation of 0.05 m in order to generate vertical structures that majorly extend down to the mean bedrock depth of 0.85 m, while allowing for some small variation that also produced some structures ending in the soil matrix, especially with the variable bedrock topography. It has to be noted that the vertical structures were cut off when crossing other structures (lateral pathway, soil-bedrock interface, bedrock).

Specific comment 5: P6485L20: How variable is this variable soil depth? What is the standard deviation of the variable soil depth? And what is the mean? Is it comparable to 0.85 m or more?

The variable soil depths range between 0.75 and 1.17 m, with a mean value of 0.85 m and a standard deviation of 0.11 m. We will add this information to a revised manuscript.

Specific comment 7: P647L23-27: It is unclear what these three different runs are. Did you run the model with the width of the experimental sites twice as the set up run to determine the initial conditions and then with the variable width as the real run? Are only the results of the simulations with the variable width shown? Explain this better.

The model was first run with the width of the experimental plots for two spin-up runs. Then, a 'real' run with the width of the experimental plots was made for the rainfall simulation phase, when input only occurred on the experimental plots. Finally, a 'real' run with the entire, variable width of the hillslope was made. The results shown are from the two 'real' runs. We will correct and clarify the description of the simulation runs accordingly.

Specific comment 9: P6489L26: Explain why solute transport was only simulated for 51 of the 65 structures. This is not clear.

Solute transport was calculated with all of the setups. In 51 simulations, a transport of solute to the hillslope toe occurred in the simulations, while the tracer remained in the hillslope domain in the remainder of the simulations. We agree that have to formulate

this more clearly.

Specific comment 10: P6492L26-29 and P6499L20-22: I don't see where you tested the effects of evapotranspiration. Also, the effect of the litter layer and bedrock topography were not systematically tested. For the litter layer, it was only a comparison of models with a thin litter layer and models without a litter layer and for bedrock topography only a constant soil depth and a variable soil depth are compared (see also comment 1 and comment 5). These conclusions thus have to be rewritten.

The reviewer is right that we did not test the effects of evapotranspiration systematically. Evapotranspiration was modelled in all setups assuming a uniform distribution of roots and parameterized with literature values, which was not clearly explained in the discussion paper. The point we wanted to make in the discussion (P6492L26-29) was that evapotranspiration would probably have a larger influence on the water balance in long-term simulations than during the event timescale of our simulations.

For the litter layer, the view of the reviewer is not quite correct. We tested configurations without a litter layer and with a litter layer having a thickness of 2.5 and 7.5 cm, respectively. We feel that testing even thicker litter layers would not add realism to the model setups. Nevertheless, we think we could elaborate on the effect of a litter layer in the discussion of our results.

For the discussion on our conclusions on variable soil depth, we would like to refer to our response to general comment 4 / specific comment 1 above. In a revised manuscript, we would like to rewrite our conclusion in the light of this discussion.

Specific comment 11: P6498L8: The word 'similar' is not right here as the timing was quiet different. Rephrase this conclusion.

The solute transport simulations of the five runs found acceptable for water flow simulation (Fig. 4 of the discussion manuscript) yielded breakthrough times between 6.33 h and 6.92 h, and peak times between 8.25 h and 8.5 h, respectively (Table 4 of the discussion manuscript). This is quite similar in our view, especially when compared to the observed tracer breakthrough.

Anyway, we plan to rewrite the discussion of the solute transport results. In the meantime, we have revisited our code and introduced a factor representing the macroporous cross section into the calculation of the transport velocities from flux densities, as discussed in the first version of the manuscript. An error made in haste appeared to have thwarted an earlier attempt to consider this factor. This modification considerably accelerated tracer transport times in the models. Admittedly, the modification does not solve all issues related to solute transport. 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.

Minor comment 3: P6480 L6-7: Did you really measure the depths up to the mm?

No, we did not measure the depths up to the millimetre, but we measured the depths up to half a centimetre and that is why we are giving decimal values here. Principally, it is only necessary for the 12.5-cm depth, but in order to be consistent, we chose to keep the decimal format for the other figures as well.

Minor comment 4: P6487 L19: How far is the Heumoser station?

The Heumöser station is approximately 250 m to the northwest from the centre of the study hillslope. We will add this information to the description.

Minor comment 5: P6491 L9-10: Add a reference for this.

We have to apologize, but we do not fully understand this particular comment. We

wanted to describe that we conceptualised our structures as being less tortuous and more regularly distributed, compared to the earthworm burrows studied by Klaus and Zehe (2010). We are afraid that we have no idea which reference for this could be added. We could possibly clarify that we conceptualise the structures in this way because the structures observed in the field were, for example, vertically oriented desiccation cracks, which are less tortuous and more evenly distributed compared to earthworm burrows.

Minor comments 1, 2 + 6, and editorial suggestions

We will gladly consider all these suggestions during the revision of the manuscript.

In conclusion, we like to state that we appreciated the dedication of the reviewer very much, and we thank once more for all the comments.

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