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Discussions

Interactive comment on "In situ investigation of rapid subsurface flow: Identification of relevant spatial structures beyond heterogeneity" *by* C. Jackisch et al.

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We thank all five reviewers for their critical and constructive comments on our manuscript. We will carefully address all issues in the revision. In the general reply we provide a revised outline and an overview about the upcoming changes concerning this part of the companion MS. It is followed by detailed replies to and discussions of the comments of each review.

1 Consistency and generality of the presented experiments:

The two companion MS deal with the in situ characterization of rapid subsurface flow. In order to highlight the methodological aspects of the study, we will refer to the general interplay of form and function. In our approach, we address this interplay through the detection of responses (MS1, function) and flow-relevant structures (MS2, form).

In accordance to MS1 a possible revised formulation for the title of this MS is: "Form and function in hillslope hydrology: In situ identification of flow-relevant structures".

We understand from the comments to both MS that the links between the different experiments, the measurements and the different aspects have not been conveyed as self-explanatory as anticipated. As suggested by the reviewers, we will revise both MS towards more clarity and self-consistency. The form – function reference shall help to clarify this.

Moreover, the lack of clear hypotheses caused substantial confusion. In accordance to the general frame, the MS at hand will primarily address form related issues along the following hypotheses:

H 2.1 Flow-relevant structures can be identified in the field under static conditions. (form described without function)

H 2.2 The characteristics of subsurface stormflow can be understood based on structural investigation. (form reveals function)

H 2.3 The localization of response patterns within the structural domain provides the missing link between form and function. (link form and function)

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2 Revised outline of the MS

Obviously, the structure of the MS requires revision in order to convey the key findings about the capabilities and limitations of multiple methods for subsurface structure identification at the pedon, plot and hillslope scale. Much confusion was caused by overlaps of process and structure related aspects. The revised MS will keep the focus on form-related aspects and leave most of the process interpretation to the companion MS. This clearer focus will also make room for details about the pedon-scale exploration, which has been too brief before.

The upcoming revised MS will be outlined as follows:

1. Introduction

1.1 Form-function relationship in hydrological sciences and subsurface flow

1.2 General introduction about the identification of flow-relevant structures in the subsurface

1.3 Specific introduction including a brief summary about the headwater under study, the hypotheses, and the overall aims of the study

2. Methods

2.1 Local exploration

2.1.1 Sampling Design: Point measurements along catenas in nested sets around observation clusters during one campaign.

2.1.2 Used methods and respective scope, scale and capability

2.1.3 3D GPR survey of the hillslope

2.2 Plot scale experiments

2.2.1 Design: 3 close-by plot irrigations as repetitions with different intensity.

2.2.2 Multi-method: Coherent use of dye and salt tracers, soil moisture monitoring, snap-shots of soil water composition regarding stable isotopes, time-lapse 3D GPR

2.3 Hillslope experiment

2.3.1 Design: Minimal replicate of plot scale setup + focus on lateral reaction on the hillslope. In conjunction with natural event to have an established connectivity and to reduce the effect of initial wetting and thus irrigation intensity and amount.

2.3.2 TDR network as spatially distributed reference. GPR transects as "virtual, non-invasive trenches".

3. Results

3.1.1 Point samples remain trapped in high heterogeneity but we can derive the mean integral plot properties (without its spatial organization).

3.1.2 3D GPR survey suggests a patchy layer of structures which later on prove to be not flow related. The driven case is needed.

3.2 Under dynamic conditions we see preferential flow and a lateral reaction in the deposit layer. But we cannot determine the hillslope response - especially as neither the share reaching the deposit layer nor the characteristics of the flow network can be determined.

3.3 TDR profiles are strongly limited in their spatial reference (resolution in time and space). GPR inferred trenches give a more comprehensive picture into the hillslope flow network as discrete, connected and leaky structures.

4. Discussion

4.1 Methodological discussions of the capabilities and limits of the used setups and methods

4.2 Conceptual discussion of heterogeneity versus structure

5. Conclusions

H 2.1: No, the driven case is needed to identify flow-relevant structures.

H 2.2: Structural investigations (including irrigation experiments) can provide crucial information to understand the processes. However, structure alone does not reveal function.

H 2.3: Form and function are mutually paired in the hydrological system. Overly strong assumptions about structures or processes can be avoided by the presented non-invasive GPR inferred trenching method, which can visualize and localize response patterns. This allows for more specific investigations and monitoring of subsurface processes.

3 Overview about main upcoming changes

In the specific replies to the reviews much more detail is given to all raised concerns. These changes will be embedded in the following revision schedule:

1. General story line and readability

1.1 Clarity about hypotheses, science questions and story line in abstract, intro, conclusions and rest of the MS.

1.2 Shortening where possible, extension where needed.

1.3 Clarity about the linkage between the methodological and case related aspects of the MS.

1.4 Final editing by a native English speaker.

2. Introduction

2.1 strong revisions to refer to more studies around the globe

2.2 highlight the methodological aspects and refer to such works more specifically

2.3 link more clearly to the conceptual aspects of inferring hillslope-scale process understanding from local or signal measurements

2.4 clarify the used terminology and sketch of the form-function concept

3. Methods

3.1 Add the WHY to each aspect and draw their connection clearly

3.2 Add sampling design (especially of local exploration).

3.3 Add more details on the hillslope experiment (although repetitive to the companion MS).

3.4 Outline again how the methods will answer the overarching question of exploration of a structured subsurface.

4. Results

4.1 more details on local exploration

4.2 split case-related results from method-related ones

5. Discussion

5.1 Shorten discussion on case results and process interpretation to a minimum showing the limits and novelties of the results at the case of the upper Colpach basin.

5.2 Give explicit frame to methodological discussion to streamline the narrative towards the main findings.

6. Conclusions with reference to the hypotheses

Many thanks again to all reviewers and Alex Zimmermann for your time and critique to substantially improve our MS.

4 Reply to Review 1

General: The manuscript should be of interest to a broad group of researchers working on the topic of hillslope runoff generation processes, and in particular of interest to those working on the topic of preferential flow processes as they relate to runoff generation. For the past three decades the challenge with respect to preferential flow has been to be able to verify the concepts developed with regard to modeling preferential flow processes. As in much of hydrology, progress has been made by starting with a black-box approach with predicting outputs for given inputs, and then this has evolved to the use of destructive morphological methods and the use of tracers, and now with more sophisticated instrumentation it being made possible to begin to look at preferential flows in real time non-destructively. The authors report their results of looking at the problem from three scales, the pedon scale, the plot scale, and then the hillslope transect scale. Only a few others have been successful at this, with one notable study by Guo et al. (2013). I believe I am correct to say that the authors state that without the larger scale measurements the local scale measurements would be able to explain the results found only by using some concept of heterogeneity.

Specific:

1. Text needs to be checked carefully for English grammar and sentence structure. I found much of the presentation and discussion to be difficult to follow. Perhaps because of the type of phrases used.

The MS will be fundamentally restructured which will already improve its readability. Moreover, we will consult a native English speaker for final editing and checking.

The statement that using local measurements one would explain the observations only by using a concept of heterogeneity and would not be able to explain it based on some network of preferential flow pathways. I found all this a bit awkward and would recommend it be rewritten.

The section about the local measurements will be strongly extended to include details about the sampling design, locations and data analysis. This will provide a better basis for the arguments we intend to make:

a) It is very difficult to distinguish heterogeneity and structure, regarding statistical heterogeneity as random small scale changes in hydrological soil properties and structure as spatially organized flow paths and their connectedness.

b) The scale of the flow-relevant structures (0.5 to 2 cm) is at least two orders of magnitude below the footprint of standard soil samples and hydrological measurements (0.25 to 1 liter).

In more general terms, heterogeneity can be seen as deviation of the found reality from the concept of quasi-homogeneous entities. If this deviation concerns only the apparent values of the same physical process, more samples are adequate to determine its stochastics. In cases (like here) where this deviation also means a shift in the physical processes, heterogeneity becomes a scale-problem. Here any measurement will consist of an unknown subset of connected or not connected flow paths. This makes it impossible to unravel the properties of the different flow domains without knowing the composition of the explored ensemble of each measurement. Hence the techniques cannot determine the sub-scale (with respect to the measurement footprint) organization of the domain and will only recover heterogeneity independent of the number of samples.

2. Not everything is well defined. For instance, the semblance attribute and the structural similarity attribute.

We will revise the MS to be fully stand alone and add details where required. For the similarity attribute we will change the too brief sentence P8L14 to:

Allroggen and Tronicke (2016) have shown, that a pixel-to-pixel comparison alone is not suitable for analyzing time-lapse GPR data. They propose a structural similarity at-

tribute, which is calculated as normalized local crosscorrelation between two GPR radargrams in a moving window.

In our study we calculate the structural similarity attribute using a local Gaussian window of 2 ns along the vertical axis and 0.06 m along the horizontal axis for the 2D data and 2.5 ns along the vertical axis and 0.1 m along the horizontal axes for the 3D data.

3. Some of the figures are difficult to decipher. They should be able to stand almost by themselves and therefore should be quite self-explanatory.

We admit that some figures are difficult to understand and will revise them. Figure 1 will be revised to contain more information about the positions of each data point. A map and details about the sampling design will be added. We will add a signature for gravel inhibiting further drilling and levels of assumed deposit layers in figure 2. We will include a figure of the general setup of the experiments. Figure 3 will be revised for clearer axis labels. Figure 4 will be dropped. Figure 5 & 6 will get more explanation. The piezometer locations will be removed in figure 7. Moreover, the legend of depth and semblance will be combined for more clarity. We will also add more detailed explanation to this rather complex figure. Figure 9 will be shifted to the companion MS. Figure 10 will get more explanation. Pls. see also below at our specific replies to your inline comments.

4. Why not discuss the results presented in comparison to the results presented by Guo et al. (2013). It seems that in their paper they have a very high resolution imaging of preferential flow structures, while in the present paper there seems to be some reservations about the imaging results. It would seem natural that you should compare the Guo et al. paper results to your own results.

We are well-aware of their studies and also cited their work. We suspect, that Guo et al. 2014 WRR is referred to, because their 2013 study is on root detection. However, there are substantial differences between their and our experiments and methods: For the hillslope experiment, the larger scale and the absence of specific reflectors confronts our study with the challenge, that we cannot trace a shift in travel time which then can be used to calculate soil moisture dynamics. This is the reason why we base our analysis on the similarity attribute. The disadvantage is that this allows only for a qualitative measure. The advantage is that the spatial organization of areas with changing reflection and transmission properties (which are attributed to changes in soil moisture) can be revealed.

Moreover, we did not discuss the work of Guo et al. here as it does not address severe methodological problems concerning the noise and the repeatability of GPR data. Especially their application of coordinate shifting (spatial migration in the third dimension) and the selection of flow path between distinct profiles causes major methodological concerns, which in our opinion result in a very questionable interpretation. Here we refer to Allroggen and Tronicke 2015, who showed, that a pixel to pixel difference calculation as performed by Gou et al. is a highly unstable calculation and can lead to a false interpretation of time-lapse GPR data. Furthermore, this migration and interpretation requires a very high spatial and temporal density of the measurements to be accurate – or very strong assumptions about the continuity of preferential flow paths. From the excavated stained soil profiles such a straight-forward propagation of the patterns even over a distance of 10 cm is in doubt. With respect to the large time lags and spatial distance between the transects in our hillslope experiment and the rather long acquisition time of each 3D cube in the plot experiments, such a migration approach is not feasible. As suggested, we will refer to their work more closely in the discussion section to highlight the methodological differences and the respective findings. This also suits the revision of the focus of the MS well. However, we believe that a detailed discussion of the methodological issues is subject to a more thorough revision in a geophysical journal. We will keep the focus on the hydrology-related aspects and refer to further studies, too.

5. Check your reference citations and reference list carefully. Upon casual perusal I found some mistakes. There may be others too.

We apologize and will carefully check this.

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Content related specific comments in the supplement:

P2L7: you refer to the 1982 article but that is not a resuming of the discussion but the beginning of the discussion.

This is a misunderstanding. The sentence will be changed to:

Beven and Germann initiated a discussion about macropores and preferential flow in 1982 and recently resumed in 2013 that the topic is still not given the attention appropriate to its significance in all areas of soil and catchment hydrology.

P2L16: this is actually 1996

Sorry for the error. We will correct this.

P5L15: not clear why it takes so long to do measurements at one location.

The formulation caused some confusion. One measurement takes only about 10 seconds but as the sensor needs to be manually lowered to each position in each access tube, each specific sampling point could only be observed in 5-10 minute intervals. To clarify the method this and the previous sentence will be changed to:

The sensor measured an integral of about 1.05L (depth increment of 18cm, mean signal penetration of 5.5cm). It was manually lowered in the tube to the respective depth for each reading. Each measurement took about 10 seconds which added up to 2-3 minutes per profile. As one probe was used to observe several access tubes, this results in 5-10 minutes measurement interval for each observation point.

P5L17: not clear on which way the excavation was done. It sounds as if the plot was dug up, but actually you were cutting a trench surface through it.

We agree that the method needs to be clarified. The sentence will be changed to:

The plot was excavated 24 h after irrigation for vertical and horizontal recovery of Brilliant Blue stains. This was done by successive digging of 3 profiles in the plot (aligned *with the slope line, 0.1 m distance starting from the lateral edge) and 5-7 horizontal cuts in different depth levels down to the first deposit layer (0.5x0.5 m in the center of the plot).*

P5L20: nothing was ever said about oven drying samples.

The analysis of the concentration of Bromide in soil samples requires a known volume of water. Thus the samples needed to be oven dried before being fully suspended in 150mL water. The sentence will be changed to:

This was done by oven drying the samples and consecutively suspending them in 150 mL de-ionised water.

P5L25: This is unclear.

The intended information is that the scaling of the sampled mass requires to assume a volume of affected soil (1m3). As the lateral flow is very difficult to estimate, we decided to neglect it. The approach also implies the assumption that the sampled soil is representative for the whole cube – which may be in question for the case of preferential flow in discrete pathways. To clarify this in the MS the sentence will be changed to:

A recovery coefficient is calculated as proportion of recovered mass of Br- in the soil samples scaled to the assumed affected soil volume (irrigated area times depth of the lowest sample). Through this we assume predominantly vertical tracer relocation and neglect lateral flow from the irrigation plot and further percolation beyond the lowest sample in the calculation. We also assume the samples to be representative for the entire affected soil volume.

P6L13: I did not find your explanation later on helpful. It is not clear to me how this attribute is computed. If it can be explained in a bit more detail so that one does not need to go to the artile by Allroggen and Tronicke (2016).

The attribute calculation will be added. Please see the reply to point 2.

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P7L4: you did not set up the model hypothetically, but you set up a hypothetical model of the site.

Thank you. This will be changed.

P7L25: it is not clear what you mean by 'resampling'.

The issue of interpolation and resampling arises from the necessity to synchronise the different singular measurements, which were taken at different times by the same probe. To do so, we generated an intermediate grid of high data density onto which linearly interpolated versions of the time series are projected. We then resample from the intermediate grid to derive a synchronised version of the records. We will clarify this in the revisions.

P9L15: is it high porosity? Or do you mean macroporosity?

In a strict sense it is macroporosity we are referring to. It will be changed.

P10L15: what is a deposit layer?

We meant to address the layer of periglacial deposits as "deposit layer". In some studies this is also called saprolite layer. Because chemical weathering is of minor control for the layer, we will change it to "periglacial deposit layer". A brief explanation will be given in a site description.

P11L1: where is the recovery coefficient defined? I do not see it discussed later.

It is described in P5L24 and caused confusion (see above). The revised formulation is more clear and defines how the recovery coefficient is derived. Moreover, we provide more detail on the assumptions related to this. Since we will extend the discussion about heterogeneity, these aspects will also be included there.

P12L18: It might be helpful to put some calculations into this explanation. You know the rate of applied water and I presume you know the amount of infiltration, so it would seem you could determine why the soil profile is not saturating so much. You can also include in the calculation an estimate of the percolation potential for the bottom layer so that one can see whether it is even possible to have ponded saturation on the surface of the bottom layer. It seems that the application rate should have been designed to be high enough to create ponded conditions.

Yes, we measured infiltration capacity at 40 locations (see figure 1a). The applied irrigation of 8.6x10-6 m/s is only slightly above the recorded minimum. As introduced in P7L4 (and as will be calcified in the revised MS) the experiment was designed to activate preferential flow paths at the hillslope. The low reaction of the piezometers is more related to the absence of a continuous subsurface water table and the existence of discrete lateral flow paths. As the piezometer reaction will be shifted to the companion MS with strong focus on processes, here we will rather highlight the issue of point observations in an unknown but structured domain more clearly.

P23L16: but is fill-and-spill even experienced at this site. It is not apparent from the observations that have been discussed.

The site shows fill-and-spill characteristics according to Fenicia et al., 2008 and Wrede et al., 2015. Moreover, surveys of bedrock topography at the slope of the plot-scale experiments (GPR and ERT measurements not subject to the MS at hand) hint to the existence of subsurface ponds. This hypothesis is also investigated in another (model) study on the same headwater (Loritz et al. 2016, HESSD). To clarify this, the sentence will be changed to: *The latter results in more deep percolation and thus potentially contributes to baseflow or fill-and-spill mechanisms at the bedrock interface (Fenicia et al., 2008, Wrede et al., 2015, and Loritz et al., 2016).*

P25L22: why not discuss the work of Guo et al. relative to what you have done here?

We will add a more detailed comparison to the discussion of the revised MS. Please see point 4 above for more details.

P27L19: what does this mean?

With exploration we meant the analysis of quasi-static or state-independent properties. We will clarify this in the revised MS. We show that without measuring the reaction of the subsurface flow, the flowpath structures cannot be distinguished from overall heterogeneity. Moreover, we show that point observations need to be set into context to avoid overly strong assumptions about domain and processes under investigation.

We will keep this message as one key finding also in the revision of the MS structure. A more focused presentation will also help to keep track of the methodological aspect of the story line.

Fig 2: what is the 'catena' of the plot experiments'? If someone asked me to find the catena I would not know what they mean. Also, the B is confusing because you have the B horizon and you have a B-nomenclature. It is not clear where there is bedrock anywhere in the site. The bedrock is discussed later, but I do not see where it is in the overall system.

The term 'catena' means the soil at the hillslope where the plot experiments were conducted. This will be rephrased. We will also provide a detailed description of the locations of all measurements together with the details on the local exploration.

For the results in figure 2 drilling was inhibited by increasing gravel content in the periglacial deposit layer. Therefore, we could not reach the bedrock interface. We will revise the figure with a symbol for the lower end of the bars. Moreover, we will include a position reference for the deposit layer from the GPR survey (data in fig. 7).

Fig 7: The colors shown here are difficult to interpret. I do not see the really dark tan or rose color that you show in the plot repesented by the color scale. Also, I do not see the semblance variable represented anywhere in the plot although you do have a gray-scale for it.

The revised MS will discuss the 3D GPR exploration in more detail. For the figure we will reduce its complexity by adding an overview of the hillslope irrigation experiment and by removing the piezometers. Moreover, the legend of depth and semblance attribute will be combined to one 2D colorbar to make the bivariate overlap more clear.

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