Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-190-RC2, 2016

Hydrology and Earth System Sciences
Discussions

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

Anonymous Referee #2

Received and published: 15 June 2016

General comments:

The authors present a rather comprehensive overview of results on the fast hydrological response of a hillslope to natural and artificial precipitation obtained by a series of complementary methods. As commented already by the first referee, the topic of the paper should be of quite some interest to the scientific community in this field. Nevertheless, I have some reservations about how the results are presented in the manuscript. I listed some general aspects below and subsequently added more detailed comments.

No clear hypotheses: Overall, the manuscript is predominantly of the type `Look what we have found`. It is a lengthy compilation of many (interesting) data without

a clear hypothesis. Although the authors state clear research questions (L. 70 - 74), these questions are not consistently addressed. Neither the abstract nor the conclusions for example provide answers about the type of structures relevant for rapid subsurface flow and transport and how they can be identified. Simply stating that this transport takes place in *specific structures* (L. 593) does not really provide new insight. This deficiency is also obvious when reading the abstract, which mentions the challenges that have been faced and emphasizes the coherent combination of methods for identifying relevant structures and overall process understanding. What these structures and process understanding actually consist of is not made clear.

Lack of generality: It is also hard (based on the manuscript) to generalise the presented findings beyond the very specific conditions at the field site (this aspects is also related to the problem mentioned above). What can be learned *in general* about preferential flow structures at hillslopes? What are general lessons about instrumentation for elucidating such structures that go beyond the recommendation `Use several different methods at the same time on the same spot`? This may sound overly critical but given the large effort such field experiments require (I very much appreciate this!) one should aim at gaining as much general insight as possible from such experiments.

Site description: Although the authors have described the site in the companion paper a minimum of information is also required in this manuscript. Even by consulting the companion paper, I could neither figure out the spatial location of the dye tracing experiments nor the date of the experiments (sec. 2.2). I also miss information on where the infiltration capacity measurements were taken (L. 104 - 108). This makes it very hard to properly interpret the results. The authors have to provide a similar map as in the companion paper locating all experiments and measurements relevant for this manuscript.

Experimental limitations: There are two aspects of the experimental procedure that struck me because they seem to (strongly) reduce the sensitivity of the experiments. First, the hillslope irrigation experiment was carried out briefly after substantial rainfall had been recorded (43 mm). This of course reduces the contrast of experimentally induced water content signatures. Additionally, the irrigation water was rather similar in its isotope signal (L. 385) as the soil moisture because it was stream water. In the end, this leads to a situation where part of the hillslope has received different isotopic input (irrigation plus precipitation) while the rest had received less water and only one type of isotopic signal (only precipitation).

Fuzzy wording: The language is not very precise and often relies on a loose and vague use of words (see details below).

Detailed comments:

Title: The title is not very clear. What is meant by *beyond heterogeneity*?

L: 14: What is a coherent combination?

L. 28 - 37: What is about the use of GPR for characterising preferential flow paths? Given the topic of the manuscript I'd expect a short summary of which experiences have been obtained with regard to identifying flow paths and how they compare to results using different approaches.

L. 30: Flury *et al.*, 1994, were probably the first to very systematically use dye tracers under standardised conditions in the field but they were for sure not the first to use dye tracers for staining preferential flow paths and infiltration patterns (see for example Steenhuis *et al.*, 1990). Be careful when making absolute statements.

L. 31: Why are there strong assumptions involved when working with infiltration patterns caused by dye tracers? Be more explicit and precise.

L. 40: What are our *current theories*? What do they predict?

L. 47: What have these model studies shown in a successful manner? Be more clear what you mean.

L. 51 - 52: I am surprised by this statement. Many experiments have been carried out for studying preferential flow and the exchange with the surrounding matrix. Can you specify?

L. 61: This sentence evokes the impression that people mostly care about the numerical aspects of model quality. I don't think that is true.

L. 62 - 63: Which *ambiguity of measurements* do you refer to here?

L. 71: Here you conceptually switch from *fast (solute) transport* (= preferential flow) to *fast hydrological responses*, which do not necessarily imply preferential flow. You have to conceptually separate these phenomena and make this explicit throughout the entire manuscript. This has to start in the Introduction and go all the way to the results and discussion: Which of the approaches actually reveal fast transport, which may reflect also a fast discharge of pre-event water?

L. 80: What do you mean by *framing local heterogeneity*?

L. 80: What is *rather unknown*?

L. 82: What do you mean by *driven conditions*? I assume you refer to experimentally controlled boundary conditions, is this right?

L. 86: What do you mean by *coherently combining multiple methods*? How is this achieved and differs from incoherent combinations of methods?

L. 134: Why have two irrigation rates been used? What are the consequences? Discuss.

L. 213 - 214: In order to achieve what?

L. 220: One cannot see that in Fig. 7. Only Fig. 2 in the companion paper makes this clear.

L. 241: Sorry, it is not clear to me what was actually done here.

L. Sec. 2.3.5: This paragraph is confusing. Why did you decide to run the experiment so shortly after 43 mm of rain? This implies that the soil was already wet and not much additional change can be expected.

L. 306: What is meant by *strong reaction*?

L. 310: Which is plot XI? Please provide a proper map, where such information is provided.

L. 312: What are stronger interactions with the soil matrix?

Fig. 3: Please provide units for the grid. What is the physical meaning of negative volumetric water content? This makes only sense if the units represent change of water content over time. Please correct the units accordingly. From the pictures it seems that there was quite some microtopography affecting the infiltration patterns. How relevant is that for the subsequent interpretation?

L. 324: *Slight correlation*: please be quantitative and provide numbers.

L. 343: Please show results for plot X (possibly in the SI).

Fig. 395: How can you distinguish between event and irrigation water with GPR?

L. 409: So what was your expectation? You never mentioned this before? Did you not know that there was no strong subsurface reflector based on your previous (pedological) investigations?

L. 425 - 427: What are these *common assumptions* - that there are two flow domains that are completely separated? The fact that preferential flow paths end somewhere in the soil matrix is not new (just look at some of the infiltration patterns in Flury *et al.*, 1994, Kulli *et al.*, 2003, or Weiler (2005)).

References:

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Kulli, B., Stamm, C., Papritz, A. & Flühler, H. (2003) Discrimination of flow regions on the basis of stained infiltration in soil profiles. Vadose Zone Journal, 2, 338 - 48.

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