

Comment on “Macropore flow of old water revisited: where does the mixing occur at the hillslope scale?” by J. Klaus et al.

This field-based experimental study by Klaus et al. presents some potentially interesting data on preferential flow mechanism; however, the rationale for the experimental design leaves one questioning whether the findings are more of an afterthought of the experiment rather than the product of a systematically executed investigation. In reading this paper, it almost seemed like the sequential design of this set of experiments was a ‘moving target’. Additionally, the issue of ‘orientation’ of preferential flow paths is confusing in your paper – up until Section 4.3, you seem to be referring strictly to vertical paths, but this is never clear. Additionally, you cite a lot of studies that focused on lateral preferential flow paths.

Given the rather lofty objectives of this study (and the title of the paper) – almost a promise to solve the ‘holy grail’ of subsurface hydrology related to preferential flow – I felt the findings and interpretations of this study fell short. I feel that the authors need to put considerable effort into this paper to bring it up to publishable standards. Furthermore, attention needs to be paid to the use of English composition in the paper, particularly concise writing – I feel that the paper could be shortened by at least 25%.

My comments appear below, with the most significant points shown in bold type:

P. 4334, L. 21 – I assume ‘were’ should be ‘where’

P. 4334, L. 25 – If you feel the need to cite the historical ‘origin’ of the ‘old’/‘new’ water concept, I believe the correct citation would be Skalsh and Farvolden’s (1979) paper in J. Hydrol.

P. 4335, L. 5-8 – earlier significant studies from Japan should be cited here

P. 4335, L. 8-9 – earlier paper linking contaminant transport to preferential flow paths was: R.C. Sidle, L.T. Kardos, and M.Th. van Genuchten. 1977. Heavy metals transport model in a sludge-treated soil. J. Environ. Qual. 6: 438-443.

P. 4335, L. 13 spell out ‘versus’

P. 4335, L. 16 ‘plot scale’

P. 4335, L. 15-21 But prior studies have elucidated how this inter-compartmental mixing may occur and why the simple concept of ‘old’ and ‘new’ water emerging at some point downslope or downstream may not always be the appropriate question to ask (or at least would have diminished inference). **As such the statement that no research from McDonnell (1990) to present is incorrect. Firstly, the 1990 work of McDonnell is not the first work that talks about old and new water.** Secondly there have been other papers that explored this idea of subsurface mixing.

P. 4336, L. 14 Sentence is not correct – delete ‘do’?

P. 4336, L. 15-24 No reference to the earlier work by Luxmoore and colleagues or the energy dissipation concept of Germann or the more recent field studies in Japan by Noguchi and Sidle.

P. 4336, L. 27 The concept of interconnectivity at the hillslope scale was more clearly articulated in subsequent papers:

R.C. Sidle, Y. Tsuboyama, S. Noguchi, I. Hosoda, M. Fujieda, and T. Shimizu. 2000. Stormflow generation in steep forested headwaters: a linked hydrogeomorphic paradigm. *Hydrol. Processes* 14: 369-385.

R.C. Sidle, S. Noguchi, Y. Tsuboyama, and K. Laursen. 2001. A conceptual model of preferential flow systems in forested hillslopes: evidence of self-organization. *Hydrol. Processes* 15: 1675-1692.

Section 2 – loess should not be capitalized.

P. 4337, L. 24 – ‘by reduced surface treatment’ is vague – do you mean no-till or minimum till agriculture? Also, total soil depth at different positions of the landscape is not given; this would seem to be important.

P. 4338, L. 16-28 **No mention of soil texture and structure, although structure was alluded to in the Abstract.** These data are important. You previously note the main vertical macropores are associated with earthworms – why not vertical soil ped faces in the structured as noted by Bouma and many others? **Also, there is absolutely no mention of the gradient of this 20 x 20 m plot – this is important because throughout the paper you seem to make the assumption that all of the preferential flow is vertical, but never talk about the possibility of slope-parallel preferential flow.** This could be induced if even a gentle gradient was present within the plot. Much of the literature you cite refers to slope-parallel preferential flow, but you never distinguish this in you paper or experiments.

P. 4339, L. 12 Spell out meteorological

P. 4339, L. 12-16 **It would seem as if the sprinkler experiment was not well designed given the level of variation in delivery amounts shown in Table 1** – was this checked by collecting total rainfall from different positions within the irrigated area? Please explain why the SD’s are so high. This could influence breakthrough results.

P. 4340, L. 6 spell out approximately; **could the proximity of soil water sampling to prior excavated holes have influenced vertical preferential flow at the sampled sites – i.e., maybe induced more preferential flow in the ‘old’ hole versus the sampled site?**

P. 4340, L. 10-21 Some **justification is needed for using 10 and 45 days of antecedent rainfall** as surrogates for soil moisture – i.e., why these time intervals?

P. 4340, L. 22-25 How were the macropores counted?

P. 4341, L. 3-5, 9-14 shouldn’t this information be placed in the earlier methods section or **is the instrumentation different for each experiment?** If it is different, why?

P. 4341, L. 9-14 **Wouldn’t it be better to wait a few days after the disturbance of inserting these tubes to take meaningful soil moisture measurements?**

P. 4342, L. 2-3 **Why did you decide to ‘improve’ measurements of soil moisture during this second experiment?** This does not speak so well of the study design.

P. 4342, L. 16 spell out approximately

P. 4343, L. 14 The case for using Brilliant Blue was never made.

P. 4345, L. 15 **You say that you “assumed that all irrigation water will mix with the applied tracer solution” – doesn’t the irrigation water CONTAIN the tracer? Please clarify.**

P. 4346, L. 11 Do you mean until 500 min into the experiment?

P. 4347, L. 4-5 How do you know that the macropores were empty?

P. 4347, L. 6-25+ **It seems that you describe the macropore-matrix interactions via conceptual models that exist – I was looking for new insights based on your field experiments. This is disappointing, and only seems to be focused on verifying the end-member mixing analysis which is fraught with difficulties due to inter-compartmental mixing phenomena.**

P. 4348, L. 5-8 delete – not needed in a scientific journal (unnecessary filler)

P. 4349, L. 9 reword – “... and the dynamic was closely linked to discharge dynamics” sounds strange

P. 4350, L. 4-8 – I noted this limitation earlier (this brings the veracity of this experiment into question); overall **it seems like the experimental design and irrigation timing was quite haphazard** (also see L. 15-16 on this page).

P. 4351, L. 11 to P. 4352, L. 18 – It is **not clear what the differences in objectives were between the first and second experiments** related to assessing preferential flow dynamics – the results are interesting, but the approach is haphazard.

P. 4353, L. 2-4 What exactly does this imply?

P. 4353, L. 15-22 Again, what is unique about this experiment? – i.e., **what unique results were you trying to obtain with these different experimental conditions?**

P. 4354, L. 12-19 **The effects from previous experiments (bromide) on the current experiment are noted, but not really resolved** (also see P. 4355, L. 2-7). This is definitely a problem with your experimental design.

P. 4355, L. 10-11 – Is this necessary? Such wordiness extends throughout the paper.

P. 4356, L. 18-22, But this error does not include analytical errors, so it could be misleading.

P. 4357 L. 23 to P. 4358 L. 4 **Please see findings germane to this discussion in two papers published in Hydrol. Process.** (Sidle et al. 2000, 2001) and one paper in Soil Sci. Soc. Am. J. (Noguchi et al. 1999) based on studies in Japan. These papers are also germane to the discussion later on this page (L. 15-23), and predate the cited references. **They are also the first papers to document ‘networks’ of preferential flow-** see P. 4360 L. 10-12.

P. 4359 L. 2-3 This is difficult to document and prove by the findings of your experiments.

Section 4.3 – **Finally you acknowledge the possibility of later preferential flow, but you still give no indication of the conditions that would support this** – e.g., slope gradient, lateral preferential flow channels, etc.

P. 4360 L. 25-27 (and material that precedes this) **I do not think this study quantified the mechanics of processes that lead to rapid mobilization;** I think the Japan studies alluded to

earlier described this mixing well and pointed out the shortcomings of simple end-member mixing models due to inter-compartmental exchange of water.