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Hydrology and Earth System Sciences Discussions

Interactive comment on "Macropore flow of old water revisited: where does the mixing occur at the hillslope scale?" *by* J. Klaus et al.

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We would like to thank reviewer #2 for the comprehensive review pointing out some weakness in the current manuscript. Further we acknowledge the supplied references. In this response we will answer the main concerns of review#2.

"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'."

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Interactive Discussion

Discussion Paper

We are sorry that the reviewer thinks this. In fact, our series of three experiments involved more than one objective, some of these were linked with pesticide transport in agricultural soils. The experiments were designed to investigate the flow processes at the field site with its dominant vertical preferential flow paths. Characterizing flow processes is important for understanding the transport of pesticides, especially when you consider the type/source of water that is transported through the soil and ultimately reaches the stream via the tile drain. The current paper focuses on one particular aspect of these experimentsâĂŤthe source of the water flow in the macropores, in this case the macropore flow of old water. The design was not a moving target; rather a formation of key units that describe new and useful findings from the overall set of experimental findings. Nevertheless, the reviewer is right that the experiments are not exclusively designed for the investigation of old water flow in macropores, and that the design would have been different without the additional work of pesticide transport.

"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."

Response: Thank you for pointing out these inconsistencies. We agree that the way we introduced and discussed lateral preferential flow may be misleading. In this work, we used lateral preferential flow not in terms of the classical lateral preferential flow down a hillslope (e.g. pipeflow). Instead we meant a mixing zone in the upper soil layer that transports water towards the tile drain. At this field site we can find two types of earth worms: endogeic and anecic. - lateral preferential flow paths are created in the near subsurface by the endogeic worms, creating a mixing layer - vertical preferential paths are created by anceci worms, which cause rapid transport into the tile drain

Thus 'lateral preferential flow' is a flow perpendicular to the tile drain orientation, which will be made more clear in the revised manuscript. We agree with the reviewer that the citations in terms of lateral preferential flow used are not appropriate and we will include the suggested references instead.

“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.”

Response: We will make our ideas clearer in the revision, especially the important role of the networks of the two species of earthworms. Further, we never intended to solve the ‘holy grail’ of hydrology, but we think that our data shows a very interesting explanation/mechanism for old water flow in macropores. This is relevant for understanding both runoff generation processes and solute transport/leaching in agricultural field soils.

“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.”

Response: With this statement we were not intending to imply that there hasn’t been research dealing with old water flow in macropores carried out since 1990. We will cite more relevant papers to outline that. Additionally, we did not state that the work of McDonnell (1990) is the first paper to talk about old and new water, but rather that it is the first one that observed old water flow in macropores. Studies prior to McDonnell (1990) did not examine this issueâĂŤin fact, the McDonnell study was motivated by the conflicting findings of Pearce and Sklash and Mosley at the Maimai catchment. Since then, of course, several papers from several regions have explored such processes. There are studies dating back to the 1960s (e.g. Hubert et al., 1969; Dincer et al., 1970; and later work by Sklash) that talk about old/new water, but these focused on catchment scale and not macropore flow.

“No mention of soil texture and structure, although structure was alluded to in the Abstract. 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 slopeparallel preferential flow."

Response: For the purpose of this study, soil structure refers to the preferential flow structures in the study area soil. We are sorry if the use of structure was misleading. We clearly did not investigate the role of soil aggregates. We will give a more precise description of the field site, including a description of the preferential flow network and slope gradient. Slope parallel preferential flow can be possible on soil interfaces. Based on our data this statement remains speculative for this field site. At this study site we only observed slope parallel lateral preferential flow in the tile drain, since this was our main objective. Slope parallel flow in the top soil either leaves the field plot unobserved or enters the vertical preferential flow paths that connect it to the tile drain. Bromide data (not included in this paper) collected from the soil and the tile drain after and during the first experiment suggests a more or less closed mass balance. This indicates that there was no significant loss of tracer out of the experiment site.

"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."

Response: In section 2.2.3 and 2.2.4 we summarize the way irrigation was measured. We clarified that the irrigation sum is the average value from the precipitation sampler. A geostatistical relationship was not found. We admit that the setup is not a perfect solution, and we never expected that this system would generate a homogeneous irrigation pattern. In our opinion it is an acceptable trade-off between speed, the forcing of 400m2, and financial constraints. Furthermore, the variability in the second and third experiments is relatively low.

"... 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?”

Response: The bore holes were sealed with soil material after sampling, and the new holes were located between the tile drain and the ‘old’ holes. These ‘old’ holes might, as outlined by the reviewer, induce local vertical preferential flow. Nevertheless, we think that the total amount of irrigation and the possible redistribution of water in the upper soil would not lead to a clear reduction of the vertical preferential flow at the location of the new holes. If more preferential flow was induced in the ‘old’ holes, this would not influence our results, since these soil samples were only used to determine pre-event conditions.

“...justification is needed for using 10 and 45 days of antecedent rainfall”

Response: Indeed the 10 and 45 day antecedent rainfall was arbitrary: 10 days medium, 45 days long. The basic idea was to give a summary of the short term weather conditions before the experiment, and a summary over the usual dry summer weeks (45days). There is no clear evidence that this is a proxy for soil moisture.

“. . .is the instrumentation different for each experiment?”

Response: We increased the number of precipitation samplers due to the high SD from the first experiment. Additionally, we used different soil moisture observations after the first experiment because the use of the inserting tubes was not practicable in a field site under agricultural practice.

“Wouldn’t it be better to wait a few days after the disturbance of inserting these tubes to take meaningful soil moisture measurements?”

Response: Yes. Hindsight is 20:20. I would now wait weeks, not days, and would not use them at agricultural sites at all.

“Why did you decide to ‘improve’ measurements of soil moisture during this second experiment? This does not speak so well of the study design.”

Response: We improved the soil moisture approach in the second experiment for the same reasons stated above. The key question is: are the results of experiment 1 satisfactory given the earlier soil moisture design? The design was able to capture the overall dynamic of soil moisture on the field plot. E.g.: The saturation could be inferred be plateau values of the measurement device during the irrigation. Nevertheless, exact soil moisture values were not determined.

"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."

Response: The irrigation water contained the tracer for approximately 20 min during each experiment and was then tracer-free for the remainder of the sprinkling. For the hydrograph separation calculations we assume that the tracer mixes with the irrigation water on the ground and that the irrigation water (after the tracer impulse) still mixes with the previously applied tracer. This is illustrated in Equation 1. The concentration of the irrigation water is calculated based on the total amount of applied irrigation water at time t, the total amount of applied bromide at time t, and the total amount of bromide that left the system at time t. This resulted in uncertainty in our calculations. This was clarified in the manuscript.

"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 endmember mixing analysis which is fraught with difficulties due to inter-compartmental mixing phenomena."

Response: Yes, the model is neither new, nor does the simple application of it lead to new insights on its own. However, the behavior of the mixing within the soil profile together with the other applied methods lead to the overall picture of the flow process. The new insights relate to the involvement of stored soil water being expressed in the preferential flow paths. Our work clearly shows that stored soil water has a distinct isotope signalâĂŤa signal that we detect in the tile drain water. This goes beyond end-

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Interactive Comment

member verification and speaks to how water is / isn't mixed in the subsurface during and between events. We have expanded and clarified our Discussion to better outline the novelty of these findings, especially considering the structure of the preferential flow system.

"…it seems like the experimental design and irrigation timing was quite haphazard."

Response: We see this as a constant improvement of experimental design. Indeed, our experimental design "evolved" from experiment to experiment as we learned more about the system and the soil moisture measurements. Sticking with one design after discovering it's short-comings would not make sense. We will improve the text to better describe the sequence of changes and updates that were made through each experiment. They were all based on insights that were gained during the work.

"not clear what the differences in objectives were between the first and second experiments"

First, with two experiments the observed process could be proven to be a non-unique event, but an important process occurring frequently with our experimental conditions. Secondly, the two experiments had different initial conditions (Summer 2009 was drier than Summer 2008). Furthermore, we changed some settings related to pesticide transport, which is not an objective of this study.

"what unique results were you trying to obtain with these different experimental conditions?"

The intention was not to get unique results; the intention was to get repeatable results.

"Please see findings germane to this discussion in two papers published in Hydrol. Process. They are also the first papers to document 'networks' of preferential flow"

Response: Thank you for the additional suggestion of helpful references. We have improved our Discussion using the suggested references.

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“Finally you acknowledge the possibility of later preferential flow, but you still give no indication of the conditions that would support this”

Response: As outlined at the beginning of this response, our use of lateral preferential flow is not in the classic sense of pipe flow or lateral slope parallel flow. The idea is that lateral preferential flow (in the network of the endogeic earthworms) delivers water to the area were the vertical preferential flow paths connect to the tile drain. This is only conceptual, and the clear extent is not proven. Nevertheless, the recovery of bromide and water (outlined in the discussion) is higher than what could be generated from the area directly above the tile drain. The reworked manuscript makes this idea more clear.

“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.”

Thanks for pointing out these references, we will include them in the discussion. We think that our data explains the relevant processes for our field site, which is a rather smooth slope in an agricultural field and controlled by the activities of earthworms.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 4333, 2012.