Review of the manuscript hess-2012-172 entitled:

**Quantifying heterogeneous transport of a tracer and a degradable contaminant in the field, under two infiltration rates**

**submitted by Schotanus et al.**

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## General comments

This paper is one of the rare links between snow and soil hydrology, a feature which is not conveyed in the title of the manuscript. It reports relevant experimental evidence on soil water movement and solute transport in snowmelt periods. The data are scrutinized in various ways. Studying the figures in details, gives the reader a good impression about the different flow regimes under snowmelt and irrigation conditions, which is in principle confirming and reinforcing known knowledge.

The introduction is in part textbook information of advanced soil physics. It discusses heterogeneous water and transport in a rather wide sense. It should be more specific by addressing the focus of the study. This would make the task better doable. One focus is – to my understanding – the role of the input heterogeneity under a melting snow pack relative to that occurring in an irrigated field plot. This was probably the important feature of the experimental system and very likely the goal of the funded project, but it is barely addressed in the introduction. To adequately cite (and discuss) the findings in the field of preferential flow is by now a task beyond the scope of a single paper, as pointed out in several review papers, not only in a single overview paper (Allaire et al.). My recommendation is therefore: streamline the introduction, discuss only the dominant aspects, but those in depth. In contrast to the introduction, the abstract is concise and well focused.

The text needs careful editing (comma rules and other little details).

## Material and Methods

**p.4828, l.25:** Do you mean what you write? Generally, preferential flow is studied at one of the scales you mention as for instance in your case.

**p.4833, l.16 ff:** Applying a solution containing 10 g of Br and ~1 kg of PG per 2 l of water is probably inducing quite a flush of snow melt. Since the snow water equivalent was not measured within the sprayed area, infiltration was underestimated, as alluded to by the authors. The fact that the snow pack disappeared faster in the sprayed area, suggests that the reported infiltration data must be considered with care. In addition, spraying snow induces tremendous preferential flow in the snow pack, which very likely resulted in preferential ports of infiltration at the snow/soil interface. I doubt that this is irrelevant for the interpretation of the measured data, as stated by the authors? Is any information available on flow heterogeneity in the snow pack, especially in the bottom layer above the soil surface?

**p.4835, l.13:** When reading this text, the definition of $W_{i,j}$ was not clear to me. In Fig. 2 the number of neighboring cells is 4 except at the outer boundaries where it is 3. I guess that W refer to this number? The explanation is given in: http://cran.r-project.org/web/packages/ape/vignettes/MoranI.pdf. Why not explaining it in this text.

**p.4836, l.12/13:** Why do you scale these values with concentrations and not with the mass? The dilution by snow melt water would'nt matter in the latter case.

**p.4837, l.3 ff:** From Fig. 7 I take that the matric potential was oscillating between -7 and -40 cm. This is the plateau region of the water retention curve (capillary fringe), the region with a wide confidence interval of the water content. I see no reason to report water contents from PTF-guessed parameters of a non-unique function. Let the data speak, which were actually measured.

**p.4837, l.10 ff:** it did not "dry", it drained.

**p.4837, l.22:** I fully agree. Bio-macropores in such a gravelly, coarse-sandy soil are very likely less dominant compared with "pore space openings" between particles. In soils of this texture the pore size distribution is usually not bimodal.

**p.4841, l.4 & 7:** It is rather casual to tacitly express time in terms of the cumulated infiltration and used this in the dimensions of v and D.

**p.4841, l.12 ff:** The dispersivity expressed as a function of the soil water content goes through a minimum at a given degree of saturation. Check the papers by Hammel and Roth (1997?)

**p.4841, l.14/15:** This is the definition of the dispersitivty? It is also a measure for the characteristic length of the dominant structures in the flow region (structure in this sense refers to regions of similar transport properties).

**p.4843, l.23/24:** In absence of macropores the ratio of transverse/longitudinal dispersitivies is larger at slower velocities (lower water contents) (Forrer et al. 1999 WRR 35(10): 3049-3060), if matrix flow dominates, but at higher degrees of water saturation this dependence might be reversed when flow is impeded by structures perpendicular to the direction of main flow, as for instance the homogenized 2cm-layer immediately above the MSC-cells.

**p.4844, l.8:** Br might be stored in part in less mobile regions above the sampling depth and will be leached in a retarded way.

**p.4844, l.28:** On the x-Axis of Fig.13 it would be better to use % of the total sampler (MSC) area (rather than $m^2$) because this is what you are suggesting in the text.

**p.4845, l.1 ff:** The solutes being transported through the preferential flow stream tubes are getting laterally mixed when they reach reach the homogenized layer. In this transverse mixing process the solutes might be retarded. This would explain the observed difference of drainage and leaching in Fig. 13.

**Figures and Tables**

**Table 1:** The information content of this table is so little that it could easily be reported in a single sentence (materials and methods section). It does however no damage to document it visible in a separate table

**Table 2:** One of the columns is redundant. I would just report v and $\alpha$.

**Table 3:** Increment size of the spatial series ? Cell size?

**Fig.1:** A schematic of the experimental setup would probably more informative than pictures. Based on these pictures one can guess the spatial arrangement of the installations, though.

**Fig.2:** Also here, the information content is utmost minimal.

**Fig.3 (caption):** The snow water equivalent SWE is the storage term. $\Delta SWE/\Delta t$ is negative for snow melt. Hence, infiltration is the sum of P and $\Delta$SWE, not the difference. Both are **daily** rates.

**Fig.4:** Here, the variable I denotes the irrigation rate, in Fig. 3 it was infiltration (evidently, it was a period without rain due to the plastic cover). Why do you use a different way of plotting I in Fig.3 and 4?

**Fig.5:** Delete "total" and add % to the number in the legend.

**Fig.7:** Why do you plot soil water content when you measured soil matric potential? The relationship between the two state variables is not well defined in the wet range. In this Fig. you add the year 2010 and in the other figures you don't ?

**Fig.4:** You write *"… would have leached in each cell with perfect parallel flow".* You should substantiate in the Discussion Section what you mean with this scaling approach (I guess : each cell is assumed to be at the outlet of an isolated stream tube,… how did you calculated its average mobile water content?)