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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We thank Hannes Flühler for his review of our manuscript. The comments helped us to improve the manuscript.

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).
We included review papers from Flury (1996), Simunek et al (2003), Jarvis (2007), Feyen et al (1998), Weihermüller et al (2007), and Allaire et al (2009).
Furthermore, we streamlined the introduction for a better line of argumentation for our objectives.
We included snowmelt in the title, to emphasize the link between snow and hydrology.

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.
We mean that preferential flow is studied at one of the mentioned scales, not at all scales at the same time. We changed this sentence in 'Preferential flow processes are generally studied at either the core, profile or landscape scale'.

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?
We agree, we also expected that the snow pack would contribute to heterogeneous flow in the soil. However, this was not observed in the experiments. A preliminary experiment was done at the same field site and with the same instrumentation during the snowmelt of 2009. We compared the areas of the sampler in which most leaching occurred. These were the same for both years. However, we did not include the results from this experiment in the ms, because it was a preliminary experiment, with less elaborative measurements.
We added a sentence about the preliminary experiment to the results section: 'Results from a preliminary experiment, which was performed during the spring of 2009, confirm this conclusion. The areas with the highest leaching were similar for 2009 and 2010, which suggests that the effect the heterogeneous infiltration of meltwater is small compared to the effect of soil heterogeneity. '.

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.rproject.org/web/packages/ape/vignettes/MoranI.pdf. Why not explaining it in this text.
We agree that this would be easier for the reader. We added to the ms: 'This means that Wij is 2 for the cells in the corners, 3 for the cells at the borders, and 4 for the other cells.'.

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.
We did scale the values with the mass, not with the concentrations.

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.
That is a good suggestion. We changed the figure, in the new ms we use the pressure head instead of the calculated soil moisture content.

p.4837, l.10 ff: it did not "dry", it drained.
We changed this in the ms.

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.
We agree with the reviewer. Due to the limited cohesion of such coarse soils, prominent macropores such as burrows are less stable, and therefore usually less important.

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.
In this paragraph we explain why the drainage was used as a time axis. Furthermore, we explain how the velocity and the dispersion coefficient were transformed to regular units. We believe doing so, we do not tacitly use drainage as a time axis.

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).
This part has been completely rewritten. The discussion of the dispersivities is extended.

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.
The homogenized layer was 2 mm thick, not 2 cm. We assume this did not influence the results, because the layer is thin, and has a coarse texture and a high saturated conductivity.

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.
Part of the bromide might still leach, because it might be stored in less mobile regions, and the BTC was incomplete. However, a storage of more than 50% of the applied bromide in immobile regions would be too high. Part of the bromide may have been taken up by plants.

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.
We agree, we changed the figure.

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.

The homogenized layer was 2 mm thick. We assume this did not influence the results, because the layer is thin, and has a coarse texture and a high saturated conductivity.

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

We changed Fig. 7, in the new version the pressure head is shown instead of the soil moisture content. As a result, we do not use the soil hydraulic parameters anymore. Therefore, we added a sentence with the information from Table 1 to the materials section, and removed Table 1.

Table 2: One of the columns is redundant. I would just report v and □□

We removed the column with the dispersion coefficient from the table.

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

The cell size is not needed to calculate I, therefore it is not included in the caption. The cell size is given in the materials section, and in the captions of figures where it is needed.

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.

The experimental setup is described in the materials section (including the depth of installation and distance from the trench wall). We think that the pictures in combination with the description are more illustrative than a schematic drawing.

We added a sentence about the details of the sampler to the ms.

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

We agree, we removed this figure.

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

We agree, the caption might have been confusing. We changed the caption 'The daily infiltration is calculated from the sum of the change in the equivalent water depth of the snow cover in a day, and the precipitation per day.'.

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?

The soil was irrigated every second day. Therefore, we use bars instead of a curve, as a curve would look like bars as well. For the snowmelt and precipitation in Fig. 3 we have values for each day, therefore we use a curve.

We made the captions more clear.

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

We deleted total from the captions in Figs. 5 and 6. The figures do not refer to percentages but to mm and masses, therefore we did not add percentage to 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 ?

We changed the figure. In the new version the pressure head is shown instead of the soil moisture content. In all figures were a data is shown, we included the year 2010.

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?)

We changed parallel flow in uniform flow in the captions of Figs 14 and 12, which is generally used.