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Hydrology and Earth System Sciences Discussions

Interactive comment on "Quantifying heterogeneous transport of a tracer and a degradable contaminant in the field, under two infiltration rates" *by* D. Schotanus et al.

Anonymous Referee #1

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Comments to

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

by: Schotanus, D., van der Ploeg, M.J., and van der Zee S.E.A.T.M.

General Comments:

Schotanus et al. presented in the manuscripts (ms) a field experiment where they examined the transport behaviour of two contrasting substances (conservative tracer

and degradable de-icing liquid) under natural infiltration after a snowmelt event and artificial irrigation. In general, the ms is of high interest to the community due to the analysis of the local breakthrough of the substances. Especially, the measurement of persist preferential flow patterns has not been reported under field conditions yet. Therefore, the ms fits into the scope of HESS.

Nevertheless, the ms in its present form is returned to authors for major revisions for several reasons.

1. The introduction needs revision in terms of introducing the state of the art and in the line of argumentation (see also specific comments). 2. Some of the conclusions are not supported by the data presented or are not discussed properly

Specific Comments:

Abstract:

In general, the abstract needs revision in terms of wording and line of argumentation.

P1 L2: "of a non-degradable tracer" – per definition a tracer should be non-degradable and should not sorb. Better use "conservative tracer" P1 L3: should be:..field experiments were performed P1 L5: should be: During the second.... P1 L8: should be:..correlated, indicating that... P1 L8: here you argue that the flow path are stable between the seasons which will not be supported by the data presented. The only conclusion you can draw is that they are comparable between the two experiments. Later you argue that the preferential flow is not induced by macropores but by local differences in soil hydraulic properties. But if you assume that this will be the case, flow pattern will change in dependency of the flow rate imposed (or the actual water content). This has been already shown by Roth (1995). P1 P9-11: weak sentence P1 L11: should be: Therefore, ... P1 L13: What do you mean by clustering? P1 L13: the leached mass... - this should be the case if the substance is degradable and if the temperature is high enough. On the other hand, the second part is highly interesting

but due to the constant flow not surprising. Please discuss critically. P1 L15-16: Please rewrite the sentence and do not list up some findings without giving any interpretation or discussion here. P1 L18: weak sentence and line of argumentation

Introduction:

In general, the introduction needs strong revisions in terms of references used and the line of argumentation. For example, there is a nice review from Javis (European Journal of Soil Science, 2007) which should give some background information and should also be cited.

P2 L22: should be core, profile, and landscape scale P2 L25: when average parameters. . .- for the soil hydraulic properties this is not an average of all single properties but an effective set of parameters. For climatic parameters this is slightly different, because short time high flow will not be captured by the daily mean P2 L26: should be: hydrological models P2 L26-27: weak sentence: maybe like this: To account for preferential flow in the models additional parameters are needed. . .. P2 L28-29: weak sentence P2 L30: should be: . . .insight which. . . P2 L30: should be: Based on this knowledge. . . P2 L45: . . .is saturated. – close to saturation. Not necessarily at saturation P2 L50-52: what do you want to say? This seems logic if you will have a closer look at the conductivity function. In general, I do have the impression that you did not well capture the ideas presented by Roth (1995). In general, they presented a microscopic heterogeneity and not macroscopic large scale heterogeneity. Therefore, it would be would be hard to present K-values at this short distances. P3 L56: What do you mean? If the soil is dry you will not have any preferential flow at all. I believe you would like to state something different such as the flow path will change due to. . .. But if you do so please provide reasons why. P3 L56-57: Not necessarily. It depends on the conductivity in dependence of the actual water content. Please clarify. P3 L57-58: see comment above P3 L61: This might be the case here but if you will have swelling or if cracking might occur this might look differently. Maybe not in effective parameters such as velocity and dispersivity but maybe in the transport pathways. P3 L67: please

delete: when macropores exist than when only matrix flow occurs. Or reformulate P3 L68: biodegradation – this is not only restricted to biodegradation. Also thermal degradation and photodegradation might play a role. P3 L74: weak sentence. P3 L79: using wick samplers. . . or lysimeters, or porous plates as shown for example by Kasteel, R., Pütz, T., & Vereecken, H. (2006). An experimental and numerical study on flow and transport in a field soil using zero-tension lysimeters and suction plates. European Journal of Soil Science, 58(3), 632-645.) P3 L80: I do not see the problem here. The biggest problem is the limited range of pressure, and therefore, that water will not be sampled if the soil is not close to saturation. There is also a review available dealing with such instruments from Weihermüller (2007, JEQ)

P3 L82: To overcome these disadvantages Bloem et al. . .. P3 L83: Additionally, the spatial resolution of the MCS is high and fluxes through individual 3.15. . .. . . P3 L85-89: What do you want to say? P4 first paragraph: Please do not list up all information you can find. Better to put them into a nice order of argumentation. P4 L13: first point: I do not really believe that you can answer this question based on your given experimental setup. See also all detailed questions raised above.

Materials and Methods:

P4 L123: Reference should be at the end of the sentence. Or better state that details can be looked up at French et al. (1994). P5 L131: The pressure in the MCs was variable. – In space or time? P5 L131-134: These are general statements, and therefore, should be provided earlier. P5 L146: should be: the infiltration per day was calculated. P5 L156: Is this caused by differences in water content? This should be critically discussed. P5L 164: please rewrite sentence. P6 L166: drainage was collected in the trench. . . - I do not believe so. It was collected in some containers or bottles which might be located in the trench. P6 L186: all parameters should be in italic such as I and j. Discussion and Discussion:

P6 L197: Please explain what you mean by drainage depth. P7 L213: . . ..concentration

of bromide. – But maybe largest mass was transported here. Concentration is not always the best indicator for solute translocation. P7 L227: or there was only local infiltration due to heterogeneous snow melt water P7 L231ff: I would calculate a worst case scenario and see if the density differences realy play an important role. P8 L236: . . . by micro-organisms. Or any other mechanism (see comment above) P8 L237-239: You did not mentioned so far how you fitted the BTCs. This should be stated somehwre in the materials section. P8 L240 should be: mean soil water content P8 L240-242: how much do these mean SWC differ from those calculated from the solute transport parameters? P8 L246ff: all units are missing for the water contents. Please check entire ms carefully. P8 L249: should be. . ., and therefore, . . . P8 L249: maybe better: the coarser material is even higher conductive as during lower water contents. . . P8 L257: . . . concentrations, as plotted in Fig. 5 and 6, were. . .. P8 L261: in general, mixing is also time depending! P9 L276ff: does this argumentation contradict the entire argumentation you did before. Please discuss critically and consistently. P9 L280: should be times instead of time steps. P9 L290: why did you assigned it to day 2 if there is no leaching at all? Logically, they should either not be included or you can also include a bar at the end indicating all no-flow cells or flow cells without complete BTC P10 L309: The argumentation is OK but not fully explained. If you will look at classical transport theory, you will encounter that spreading generally increases with increasing travel time (or distance). Consequently, the peak decreases (if you do not lose any mass). Secondly, faster movement reduces mixing between different flow channels or flow pathways with different concentrations, leading consequently to larger concentrations in the observed single BTCs. P10 L311: Which should be clear if you follow up the argumentation given above. P10 L312: should be: in Fig. 10, the solute transport. . .. P10 L312-317: You should provide some more details in the materials section how you fitted the CDE. P10 L317: should be: back-transformed to units of. . .. P10 L318: Do not use alpha for dispersivity because you already used it as a MvG parameter. P10 L319: should be: As expected, for the fast. . .... P10 L320ff: In general, the discussion about the dispersivities is weak. I would like to see some hypothesis

and a critical discussion here. P10 L330: longer, and therefore, . . . P10 L340ff: I would not use only the peak concentration for the estimation of the degradation. Classically, the entire mass (or mass recovery or leached mass fraction) will be used. P11 L350: should be K_eff in italic as all other parameters. P11 L351ff: I do not understand why you need a full BTC. You can still fit the CDE to incomplete tracer breakthrough curves. P11 L357: This has been already shown by several other authors, therefore discuss in this context. A recent numerical study by Weihermüller et al. (2012, VZJ) indicates the same features. Additionally, they introduced (or better used) the concept of leached mass fraction which might be also of interest over here for a better and quicker interpretation of the data shown. P11 l367: see comment above how to handle this. P11 L375ff: I do not fully agree. Even if you will have large heterogeneity, the flow will be homogeneous at pressure heads where all conductivity curves cross. Please check again the paper by Roth (1995). P12 L392: I wonder. Is there a theory for that or do you refer to a bad experiment to excuse another bad experiment. There should be always close mass balance closure. Maybe you can find some hints in the paper by Kasteel et al. EJSS (2006) P12 L392ff: As long as you do not have mass balance close I would be careful with such statements P12 L402: should be: . . .cumulated as proposed by Quisenberry et al. (1994), Strock et al. (2001), and De Rooij and Stagnitti (2002). . . P12 L402: Do not start up with Doing so. And rewrite entire sentence. P12 L407: Please be consistent and use also percentage in the plots (Fig. 13) P12 L409ff: I do not understand at all. Please explain. Is this caused by changes in the water storage term during drainage? P13 L 419ff: I would not use the term line. P13 L426-427: That's a fairly general statement. But what do you want to say here? P13 L428: The drainage during. . . What do mean? Amount, timing, special occurrence? Please specify. P13 L430 . . .bromide leaching. . . same comment as above P13 L431-432: I do not understand what you want to say. P13 L435ff: Which should be clear. Do you expect something different? I would say no as long as you do not have any sorption.

Conclusion:

P13 L 470: I would not talk about any seasonal effect. See also the comments raised before. In general, the conclusion should be adapted to the comments raised above.

References:

In general, all Journal titles should be in capital such as: Journal of Hydrology. Please check entire reference list.

Tables:

Tab 2:Caption should be: Solute transport parameters with pore water velocity v, dispersion coefficient D, and dispersivity lambda, (equation) for fast, average, and slow cells, as well as for the entire sampler. Does it make sense to show also dispersion coefficient if you also show dispersivity? All units are missing in caption.

Tab. 3: Caption should be: . . . Moran's I, A with 1 indicates perfect spatial autocorrelation, and -0.01 no spatial autocorrelation

Figures:

Fig. 3: should be : daily mean air temperature. All units are missing.

Fig. 4: delete same remarks as. . . and add full figure description because it is not the same as in Fig. 3.

Fig. 5: Why don't you rescale the drainage in the legend instead of suing 10 by the power of -1? Caption: should be of the cumulative drainage since solute application. Consequently the last sentence can be deleted.

Fig.6: same as above

Fig.7: I would always sue full units or at least define it as volumetric water content. Otherwise it can be also gravimetric water content

Fig. 8: Delete "marked with ellipses"

Fig. 9: daynumber is somehow confusing.

Fig. 12: There is some duplication in the caption: What do you mean by parallel flow? Do you mean 1-D vertical flow?

Fig.13: Should be Normalized cumulative drainage. . . Delete the unit at the end of first sentence. Maybe better to express cumulative area in percentage as you did in the text. Should be drainage, bromide, or PG leaching.

Fig. 14: indicate the 1.1 line in the plots with 1:1. Again what do you mean by parallel flow?Delet the last sentence if you add in the 1:1 in the plots.

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