Replies to Interactive comments of Anonymous Referee #1
on
"Meta-analysis of the effects of soil properties, site factors and experimental conditions on preferential solute transport" by J. K. Koestel, J. Moeys and N. J. Jarvis.

The referee's comments are shown in blue.
Our replies are shown in black.

This paper presents a comprehensive collection of data derived from the transport experiments that were published in the literature. Such a compilation of information is essential to obtain a general understanding of transport processes in soils and the parameters and properties that control these processes. In contrast to previous studies, this study focuses on parameters that quantify preferential transport. I consider this study as highly relevant and would recommend its publication. However, I have a few major comments. First, I think that the authors should define what is actually meant by 'preferential transport'. That is, when is transport considered to be preferential and when is it not? What is for instance the difference between preferential transport and heterogeneous transport? Is transport with a high apparent dispersivity but perfectly described by a CDE preferential or not?

We agree. We have now explicitly what we mean when referring to "preferential transport":

See p. 5-6, l. 174-191 in the modified manuscript.

Second, it should be made clear why the parameters such as the p0.05 and the holdback factor are good measures for preferential transport and which typical characteristics they measure which are not captured by another often used parameter: the apparent dispersivity. Being very critical, one could question whether the p0.05 and the holdback factor are characteristics that are suited to quantify preferential transport. The dataset illustrates that they are both strongly correlated with the apparent dispersivity so that I wonder about their additional value.

We agree that the reviewer addresses an important point. Obviously, p0.05 and the holdback (H) are indicators for early tracer arrival. Furthermore, the study of *Koestel et al.* (2011) supports the empirical knowledge that early tracer arrival is strongly correlated with a long tailing. Both BTC features are associated with preferential transport in the literature (*Brusseau and Rao*, 1990). Furthermore, *Knudby and Carrera* (2005) found for their numerical experiments that the skewness of a BTC was not particularly suited as an indicator for preferential transport and that instead rather the p0.05 should be used. Another advantage of p0.05 and H is that they are more robust to BTC deconvolution (see reply below for explanation) than the apparent dispersivity (Koestel et al. 2011). In the same study, Koestel et al. show that the apparent dispersivity is well correlated with both, p0.05 and H, but also carries additional information on the transport process, i.e. because, in contrast to p0.05 and H, also the late tracer arrival times are used to calculate the apparent dispersivity.

All the above discussed points are now addressed in the modified version of the manuscript:

See p. 6-7, l. 168-191 as well as p. 8-10, l. 218-263 in the modified manuscript.

I would propose another characteristic: a normalised p0.05, normalized by the standard deviation of the (normalized) travel time, as a characteristic that contains additional information about the transport process, which is not captured by the apparent dispersivity. I suspect that this characteristic returns the same value independent of the apparent dispersivity length and the transport distance when the breakthrough can be described by a CDE model. Deviations of this CDE 'value' would then be a measure of anomalous transport.

We think that the reviewer's proposition is interesting. However, the proposed shape-measure does not own the main two advantages that p0.05 has. Firstly, it has not been investigated in another study before as p0.05 has been in (*Knudby and Carrera*, 2005) and (*Koestel et al.*, 2011); second, p0.05 has been shown to be rather insensitive to the choice of transfer-function for BTC de-convolution (*Koestel et al.*, 2011) whereas the standard deviation (of the normalized travel-time) was the rather sensitive to choice of transfer-function, i.e. the value if the standard deviation is stronger determined by the choice of the transfer function as by the data. Thus we think the proposed parameter will not be as insensitive to the choice of the transfer function as the $p_{0.05}$. Nevertheless, we encourage the reviewer undertake a study on potentials and limitations of the proposed shape-measure.

Third, when analyzing certain effects of certain parameters (e.g. transport distance, flow rate, texture, : : :) on the preferential flow characteristics, it should be avoided that observed effect is not due to other parameters that vary together with the parameter of which the effect is investigated. For instance, I suspect that the flow rate effect is also linked with the transport distance effect since I do not expect that high flow rates could be achieved easily in long soil column. The flow rate effect might also be linked to texture since high flow rates are more difficult to obtain in fine textured soils.

We generally agree. We are and were aware of this fact. We have taken care to check such mutual correlations and advise the reader in case it exists, e.g.:

See p.11, l.290 – 293 in the modified manuscript.

Note that the flow rate was neither correlated to the travel distance nor to the clay content in this here presented dataset (see Figure 3). To point this out explicitly, we have included the following passage:

See p.13-14, l. 370-375 in the modified manuscript.

detailed comments:

p 10011 I agree with the authors that it comparability can be improved if only flux concentrations are used. Comparing results from resident concentrations with results from flux

concentrations always requires a certain concept of the transport process. A drawback of focusing on flux concentrations is that flux concentrations can hardly be measured in field experiments.

We agree.

p 10012: I am critical of using results from measurements in tile drains. The largest part of the transport direction towards tile drains is horizontal whereas the vertical part of the trajectory through the soil is small. Therefore, breakthrough curves measured in tile drains lump transport in the soil with transport in the aquifer or perched groundwater layer on top of the confining layer. A paper by Radcliffe et al. (1996) illustrates the effect of this lateral transport on breakthrough curves measured in tile drains (Radcliffe, D.E., P.M. Tillotson, P.F. Hendrix, L.T.West, J.E. Box, and E.W. Tollner. 1996. Anion transport in a Piedmont Ultisol: I. Field-scale parameters. Soil Sci. Soc. Am. J. 60:755–761.). I think that the results from experiments in tile drained fields should be treated with care and flagged in the dataset. After reading the paper completely, I noticed that this was the only tile drained study included in the dataset.

We agree. The data of Radcliffe et al. has been removed from the study. The numbers on available BTCs discussed during the manuscript has also been changed accordingly. Also the References, Figures and Tables have been modified accordingly (also see comments below).

p 10012: Using the studies for which only CDE parameters were available could be critical. It could be the case that the CDE in these studies did not fit the data as well as the MIM would have fitted the data. If this is the case, then this would lead to a bias in the derived preferential flow indicators for this part of the dataset.

We agree but we suspect that the effect is rather minimal. However, we have re-checked the BTCs fitted with the CDE. We found that only the BTCs published in (*Green et al.*, 1995) could not be fitted well with the CDE and we therefore did not consider them for this here study. We have furthermore applied stricter rules for accepted datasets which are described now in the modified manuscript:

See p. 5-6, l. 148-153 in the modified manuscript

Note that this step reduces the investigated dataset from 793 to 733 but only marginally changes the results. All references, tables, figures and numbers in the text have been modified accordingly.

p10015, Eq. 9: The cumulative pdf, Fn should be a function of T, where T should be the upper limit of the integral in equation 9. For T is infinity, Fn =1.

We agree. We have changed eq. 9 accordingly. (note that Eq. 9 is Eq. 10 in the modified manuscript).

p10015: p0.05 is said to be negatively correlated with the degree of preferential transport. But how is the 'degree of preferential transport' defined. Transport with a high degree of dispersion would also lead to small p0.05 since p0.05 is negatively correlated with the dispersion coefficient. Is this also considered as 'preferential transport'? I would rather propose to use (1-p0.05) that is normalized by the standard deviation of the arrival times.

(see replies above)

p10015: Again, a large H may also be due to a large heterogeneity in the transport.

(see replies above)

p10015: What is meant by BTC-deconvolution? Calculation of H requires an integration of the BTC followed by an integration of the cumulative BTC. How is this related to a 'deconvolution'?

We have now included a passage explaining what is meant by 'BTC-deconvolution'.

See p.5, l.127-144 in the modified manuscript.

p10015: I think there is no place for belief in a scientific paper.

We have deleted the corresponding sentence from the manuscript.

p10016: The apparent dispersivity is said to be an indicator of the heterogeneity of the transport process. But what is fundamentally different from the measures p0.05 and H which are in fact also indicators of heterogeneous transport? It would be interesting to see what is the variation of p0.05 and H that are derived from CDE predicted BTCs using the apparent dispersivity. I am pretty sure that this variation will cover a large part of the variation of the H and p.005 that are derived from the MIM fits. From this point of view, one could equally well say that the p.005 and H are rather measures of transport heterogeneity. I think that this reflects the problem of not clearly defining what is meant by 'preferential transport' and how it differs from 'heterogeneous transport'.

(see replies above)

p10016: the authors should clarify why the apparent dispersivity is less robust to BTC deconvolution. They should also explain what is meant by 'BTC-deconvolution'.

(see replies above)

p10018: 'two of the three investigated indicators for preferential transport were strongly negatively correlated. 'Why is only this correlation mentioned? There is also a strong correlation of these indicators with the apparent dispersivity.

We now mention explicitly that p0.05 and H are both correlated to the apparent dispersivity:

See p. 10, l. 256 – 261 in the modified manuscript.

p10020: 'For a given apparent dispersivity, p0.05 increases with column length. This suggests that the strength of the preferential transport decreases with travel distance' p0.05 will be constant with travel distance when the coefficient of variation of the travel time remains constant with travel distance. This is the case for a stochastic convective transport process, i.e. when the apparent dispersivity increases linearly with travel distance. For a convective dispersive transport, the coefficient of variation of the travel time will decrease with travel distance, which would correspond with a decrease in p0.05 with travel distance. I think an interpretation of the behaviour of p0.05 in these terms could give an understanding of its dependence on travel distance. It could also be helpful to define what is actually meant by 'preferential transport'.

(see replies above)

p10020: 'Figure 7a–c shows that not only the medians of v and apparent dispersivity monotonously increase with the respective water flux class but also the strength of preferential transport (there is negative relationship between p0.05 and q).' The fact that the apparent dispersivity increases with flow rate, isn't that also a sign of preferential transport? I do not understand why the strength of preferential transport is only measured against p0.05. The increase of the apparent dispersivity with increasing flow rate is automatically related with a decrease in p0.05 with increasing flow rate, as long as the same column lengths are used for the high and low flow experiments. However, I would expect that the high flow rate experiments were mostly carried out in small cores whereas in the longer cores, experiments are mostly carried under lower flow rates. Therefore, the increase of p0.05 with decreasing flow rate could also be due to the fact that the length scale of the experiments increased with decreasing flow rate.

(see replies above)

p10021: I would propose to replace Figure 8 by box plots (similar as Figure 7). Form figure 8, I cannot derive that samples containing both top and subsoil have higher apparent dispersivities and lower p0.05 than samples from the top or subsoil only. This will become clearer in a box-plot.

We do not agree that figure 8 should be replaced by a box plot. We agree that the effect of sampling location on apparent dispersivity would be better discernible but then the interaction between p0.05 and the apparent dispersivity would not be illustrated. We furthermore do not agree that it is not possible to recognize that the samples containing both topsoil and subsoil exhibit a larger apparent dispersivity from figure 8.

p10021: Also to illustrate the effect of texture, box plots might be better.

We do not agree. We prefer the figure as it is (see reasoning above).

p10022: If Figure 10 illustrates the effect of texture clearer than figure 9, couldn't figure 9 be skipped then?

We agree that figure 10 illustrates the effect of texture better than figure 9. However, Figure 9 illustrates that the geometric mean diameter alone is not sufficient to explain the effect of texture on the shape-measures. This was the reason to include figure 9 into the manuscript and therefore we want to keep Figure 9 in the manuscript.

p1024: 'Anionic tracers exhibited larger apparent dispersivities than neutral tracers.' Were experimental conditions: soil types, flow rates, column lengths similar between the experiments that were carried out using the different tracers or could this also explain the differences?

We advise the reviewer that the corresponding flow rates and column lengths are shown (figure 5b) and their effect is on the dispersivity is discussed on p10019, l.23-29 in the original manuscript. Our conclusion ('Anionic tracers exhibited larger apparent dispersivities than neutral tracers.') is based on this discussion.

See p.13, l.344-351 in the modified manuscript.

Literature

Brusseau, M. L., and P. S. C. Rao (1990), MODELING SOLUTE TRANSPORT IN STRUCTURED SOILS - A REVIEW, *Geoderma*, *46*(1-3), 169-192.

Green, J. D., R. Horton, and J. L. Baker (1995), Crop residue effects on leaching of surface-applied chemicals, *J. Environ. Qual.*, *24*(2), 343-351.

Knudby, C., and J. Carrera (2005), On the relationship between indicators of geostatistical, flow and transport connectivity, *Advances in Water Resources*, *28*(4), 405-421.

Koestel, J., J. Moeys, and N. J. Jarvis (2011), Evaluation of Nonparametric Shape Measures for Solute Breakthrough Curves, *Vadose Zone J.*, *10*(4), 1261-1275.

Rose, D. A. (1973), Some aspects of hydrodynamic dispersion of solutes in porous materials, *Journal of Soil Science*, *24*(3), 285-295.

Vanderborght, J., and H. Vereecken (2007), Review of dispersivities for transport modeling in soils, *Vadose Zone J.*, *6*(1), 29-52.