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Hydrology and Earth System Sciences Discussions

Interactive comment on "A simple two layer model for simulation of adsorbing and nonadsorbing solute transport through field soils" *by* M. S. Akhtar et al.

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Dear Editor We thank the referees for the effort they spent in reviewing our manuscript. We agree with most of their comments in particular the low quality of English language which in our opinion resulted in misleading statements. We will take specific care to that in our revised version.

One conclusion we draw from their comments is that we will revise our manuscript with focus on the analyses of the experimental data by the presented two layer model which is a further development of Steenhuis et al. (1994). The aim of our contribution is to

present a simple tool for analyzing the complex transport systems in unsaturated soils by use of the outflow concentrations of a soil column. This includes the description of the developed approach as well as the limits of application. We would leave the model comparison (CDE, two-region, simple preferential) model to another paper in which we might include also more sophisticated model comparison procedures as the editor mentioned previously. We will therefore not reply on the detailed comments to the descriptions and results related these models

We agree with referee 1 that a more sophisticated inverse parameter estimation procedure would be appropriate. Our aim was originally only to demonstrate the applicability of the further developed model. As we will discuss below we think that the fitting of the simple model will not be able to provide real system parameters with uncertainty because any obtained confidence interval will be biased strongly by the deviation of the processes represented by the model approach and those determining the transport in reality. On the other hand, confidence intervals provided by an inverse procedure might be useful to quantify that deviation. However, as the authors are not experts in this field to implement such a procedure will be a significant effort in regard to the revision of the manuscript. In case that referee 1 does not insist on such an implementation we would add a simple parameter sensitivity analyses by the simple model to demonstrate the model limitations.

We also want to make clear our aim of further development of the two-layer model with a pure mixing layer at the top and a pure transport layer below. We will leave the term conveyance layer. For the mixing layer no preferential transport was assumed as there was evidence from dye tracer experiments which we can proof by photographs. However, in the existing version the mixing layer depth was discussed mainly in the context of the experiment and not of the analyses by model fitting. Preferential flow was assigned only with the transport layer. Therefore, no exchange between matrix and preferential flow was considered in both layers. This could be an aspect for another further development. The major aspect of the further development was so far to represent the application phase in the way as it was defined in the experimental procedure. In the experiment the solutes were applied until a certain outflow concentration C2 was observed in relation to the applied concentration. This led to the need of reformulating the model boundary conditions. In this context will point out the meaning of the concentration C1(T0) and Eq. 12 more clearly. We agree with referee 1 to apply similar mass rather than to use the same ration C1/C0 in regard with the parameter study.

Referee 2 discussed the representation of the water content profile by the proposed model. As our aim was to apply a simple model and not a numerical model, such as MACRO or HYDRUS-1D, we were unable to resolve any water content profile within the two layers. Concentration profiles are not present in the mixing layer as the mixing is instantaneous within the whole layer depth. Since piston flow is assumed in the transport layer a profile can be found by routing of the outflow concentration C1 from the mixing layer. The application of numerical models with a large number of degrees of freedom would be contradictory to our aim to describe outflow concentrations by a simple model approach.

In the context of model approaches both referees discussed the meaning of the retardation coefficient. We agree totally with them that there is a fundamental distinction between the considerations of the retardation factors in the context of model approach or experimental observation. Our major aim to consider the retardation factor as analysis parameter was to use these distinct meanings to analyse the system properties. Since we did not conduct batch experiments with the soils we did not determine the partitioning coefficients in dependently. Unfortunately, the descriptions were not formulated properly to draw the possible conclusions clearly. We will also take specific care on this aspect in our revision.

We agree completely with referee 2 that the assumption of chemical hysteresis creates a difference in the mass balance. This difference is in our opinion much more related to kinetic effects rather than non-linear isotherms. Like the retardation coefficients

analysing the sorption processes by this approach could be a possibility to identify the existence of such features.

In agreement with referee 2 we also recognized some inconsistency of the experimental data with respect to the infiltration boundary conditions. However, as it is difficult to explain these and also the unclear hydraulic conditions at the seepage at the outflow without detailed information of the distributions of variables within the soil columns we did not discussed these aspects in detail enough. It might be useful to give more clear description of the transport phenomena that might occur under possible experimental conditions. Thank you M. S. Akhtatr

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