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Hydrology and Earth System Sciences Discussions

Interactive comment on "A novel explicit approach to model bromide and pesticide transport in soils containing macropores" *by* J. Klaus and E. Zehe

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First we would like to thank Mats Larsbo for the comprehensive review. In the following we will address the specific comments outlined in the review.

Mr. Larsbo raised some questions in relation to the choice of the dimensions of the 3D reality in the 2D model. We have chosen to represent the hillslope parallel to the tile drain for several reasons:

a) Since the tile-drained field site is not at a plane location, surface processes are of crucial importance. To account for different infiltration patterns on different parts of the slope we have chose to represent the slope parallel to the tile drain. This also allows surface redistribution of irrigation water along the hillslope. b) The representation

of the tile drain as explicit structure parallel to the hillslope allows that the water that reaches the right modelling boundary (tile drain outlet) can directly leave to system. This depends solely on the amount of water flow in the tile drain. Defining the tile drain as a point sink perpendicular to the hillslope does not allow this kind of handling. c) The objective of the study is to test the new modelling approach considering vertical preferential flow paths in an explicit way. The exact processes that lead to transport of water and solutes perpendicular to the tile drain are not clear yet, we can not explicitly account for them. Thus we have chosen the scaling factor that summarises the processes leading to lateral transport of water and solutes perpendicular to the tile drain (for a detailed description of this factor, pleases see Klaus and Zehe, 2010, Hydrological Processes, 24, p. 1595-1609). We think, after the study, that this scaling factor is an appropriate method in combination with the chosen dimension.

All over, we think the that representing the tile drain parallel to the hillslope is a valuable alternative to the perpendicular representation, also we are aware of the limitations (e.g. if simulating the log term leaching effect linked to ground water level). In the revision of the paper, we will explain our decision of the representation in a clearer way.

Mr. Larsbo stated that n-values larger one are not modelling adsorption anymore, and that this will thus not give information about the transport of IPU. The goal of using n-values exceeding 1, was to show that the major part of the transported IPU is transported without any retardation. We know that values as large as 5 are not accounting for retardation/adsorption anymore, since R is reduced to nearly one. Nevertheless or because of that, the modelling results are at least approaching the overall pesticide transport, in contrary to the modelling with n<1. Thus we want to include this part in the article, since it shows that considering adsorption processes in the event based leaching of reactive solutes does not improve the results, or even worsen them.

Mr.Larsbo suggested to include a discussion of the reasons for the small retardation of IPU during the field experiments, we think this is a very good suggestion, and also think that fits to the modelling results that show the small or non-existing retardation

during the experiment. We will improve this point for the revised paper, especially considering the various effects in the biopore coating and the differences to the soil matrix processes.

P993L24-P995L22: Mr. Larsbo points out the weakness of this passage. This passage is to summarise actually model approaches and to point at the limitation of existing approaches. We think that an explicit representation of preferential flow paths is one possible way out. In the revised version we will add a small summary at the end of this section to better link the references better to the objectives, and thus improve this part.

Mr. Larsbo raised a question about the use of the data supplied by 25 TDR probes. We mentioned the use of 25 TDR sensors to describe the performed experiment in a better way. In this study the results from the TDR are not explicitly used. They were used in the previous study (Klaus and Zehe, 2010) to describe the wet initial conditions of the hillslope of the upper soil. Since the sensors only covered the upper soil, we allowed different initial conditions for the soil moisture. It was found, that most of the best model runs followed the wet initial conditions, thus a better soil moisture observation could reduce the equifinality of the model setups. The best model runs from the 2010 paper, are used in this study. We could include a discussion about the different parameter setups, that lead to successful solute modelling results in this paper, e.g. wet initial conditions lead to better results. If this would improve understanding of the paper.

Response to the minor comments: We will include the various minor suggestions/typos in the revision. For some of the questions I will give a short answer that can be discussed.

P1000L4: The grid size of 30cm in lateral extend of a macropore and the related high amount of bromide that is stored in the upper worm burrows will – in principle – lead to a rapid initiation of the bromide transport in the macropores. This can be seen as some kind of mobilisation of the bromide directly at the surface. Nevertheless, this is clearly a point were the model parameterisation differs from the field conditions. The total effect

of these initial conditions is not observable, since the model is not reproducing the very fast first breakthrough of bromide in the experiment well (see Figure 3-5).

P1001L24-25: it's the hydraulic conductivity of the tile drain, we will change that.

P1001L14-15. The reason why we have chosen a fixed one is that we didn't want to add an additional parameter that has to be varied. But since this is an important point we will add a sensitivity model run that shows the impact of a slight change in the diffusion coefficient. The dispersion coefficient was used based on the value that led to successful reproduction of tracer infiltration in the study of Zehe and Blöschl (2004). This value is similar to the average value measured in a field study nearby (Klaus, 2011, yet unpublished), but this study also showed the variability of the parameter. Thus I think an exemplified sensitivity study may be a valuable add on. Is it better to include the citation for field measurements than the evaluation based on a modelling study?

P1003L6-9: We have no justification for this approach. There is no available data to distinguish between the Freundlich parameters of the different soil parts. Thus we used at first an approach that uses a homogeneous distribution to evaluate the effect of the different parameter values, and after we understand their relevance, we added the additional complexity be a heterogeneous distribution.

P1005L13-15. Maybe we can change 'lines from the soil surface down' to 'patterns of higher bromide concentrations along the preferential pathways'? Is this better explaining the bromide pattern in your opinion?

P1008L22-25: This is right. We don't expect the scaling factor to be the true width of the drained cross section. This parameter is more an integration of different processes that can be expressed as a drained cross section. This width depends on the initial conditions. With this sentence we wanted to suggest that the approach of scaling is valid to solve the 2D-3D problem, and not that we can measure the real width of the drained cross section. We will clarify this better in the revised version.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 991, 2011.