Hydrol. Earth Syst. Sci. Discuss., 5, S989–S993, 2008
www.hydrol-earth-syst-sci-discuss.net/5/S989/2008/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Lateral inflow into the hyporheic zone tested by a laboratory model" *by* P. Y. Chou and G. Wyseure

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Received and published: 25 August 2008

We greatly appreciate the comments given by the anonymous referee #1. Probably we could have been more explicit and clear in our objectives in order to avoid the concerns by the referee #1. The observations by the referee #1 at the end of his/her section 1 are fully appropriate and correct. As the referee states the objective of our paper is to contribute to a better understanding of the linkage between the hyporheic zone and the aquifer surrounding the river.

General response

1) We had a great concern while selecting our title and after long deliberations we opted for a careful title which puts "lateral flow" upfront and also clearly indicates that this is a "laboratory" study. We hope that we do not raise any false claim whereby we

would have presented our work as the ultimate solution for the very complex problem of the hyporheic zone. In addition we do not reject the Transient Storage Model (TSM) as the referee seems to claim. Rather we pinpointed the neglected importance of the (bidirectional) lateral inflow from the aquifer to the river. We also do not claim that lateral inflow is the only process. Our specific and immediate objective in this paper is to contribute to a better understanding of the lateral inflow into the hyporheic zone. Our general and more long-term objective is to contribute to a better representation of lateral inflow.

2) When considering to setup a HYDRUS model for a real-life hyporheic zone it was clear that there was a big gap in our understanding of the hydrodynamic transport from the unsaturated zone, passing the surrounding aquifer into the river. The HYDRUS model is suitable to handle 2D/3D complex geometries and layering.

3) At this stage we do not have the technical (and financial) means to instrument a real-life hyporheic zone in such a fine detail that we can easily identify the inflow processes. Our instrumentation in the field leads often to studies which struggle hard with equifinality and uncertainty in terms of parameters and in terms of model structure. We therefore opted in a first approach for a laboratory study with as major objective to identify the lateral inflow processes and its parameters. The use of a numerical model allows at a later stage to incorporate the findings of the laboratory model into a real-life complex geometry and layering.

4) For the laboratory study we could indeed have opted for a vertical tank, which would have been more close to the real-life hyporheic zone. After careful consideration we have choosen a J-shaped laboratory model. This J-shaped model tries to combine a flow-line within the continuum unsaturated zone, aquifer and river with a column approach. The column approach reduces the geometry of the transport to a 1D, allowing analytical solutions for parameter identification. With hindsight the major improvement of our current setup would have been to include a horizontal section between the two bends. We, however, tested this set-up with the numerical HYDRUS 2D/3D model and

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found that the data in the bend were still meaningful.

5) We considered that it was as not appropriate to report this in this paper, but we are currently studying with our laboratory model the effect of fluctuating water levels in the river while maintaining a lateral inflow from the surrounding aquifer. In this study we use water of different temperatures and data log thermocouples at short distances to monitor the bidirectional flow in the hyporheic zone and depth of penetration into the riverbed. Results look very promising but our analysis is not yet completed.

6) Since the hyporheic zone connects stream water and groundwater, we have the conviction that a more complete characterization of the lateral inflow from the aquifer into the hyporheic zone will contribute to a better insight. Thus as we mentioned in the abstract of manuscript that the main purpose of this research is to identify the physical processes and characteristics needed for a numerical model, which includes the unsaturated recharge zone, the aquifer and the river bed (P1568, L5-7), and the hyporheic zone in conjunction with the adjacent aquifer is integrated in such model. We do, however, not see as referee #1 that the advection dispersion equation is an alternative and hence mutual excluding model to the TSM. The advection dispersion differential equation is a distributed model. It can be solved by a numerical model for complex geometries and layering. In contrast the TSM is a conceptual and lumped model, as mentioned by Packman and Bencala (2000) that the physical mechanics of transient storage are strictly idealized. TSM is indeed widely adopted to analyze the effect of solutes exchange between river water and bed sediment (P1569, L12-17) in the previous hyporheic zone studies. Both models have their merits and strengths. We believe that in the longer term the TSM should have a more explicit and dynamic representation of the lateral inflow. Our study does not attempt to compare the difference of tracer concentration between the stream water and the hyporheic zone, but tries rather to characterize the transport of dissolved material by hydrodynamic dispersion into or from the hyporheic zone in conjunction with the adjacent aquifer and soils (P1570, L16-18). We agree with the referee that we need to more clearly clarify in the manuscript

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our objectives.

Response to specific comments.

Thanks for the specific comments. We will improve our manuscript by taking these suggestions into account in the final revised version. And we will also review and add the recommended references into the final version of manuscript. Referee in his point 3 wishes to see more references. We can include theses references in our literature review.

Referee: Page 1573, equation (6)-(10): The equation (10) which is given as the solution of the advection-dispersion equation subject to initial and boundary conditions given in equation (6)-(8), has only time variable whereas the governing equation and initial and boundary conditions have also space variable which makes the solution inconsistent with the problem. Further, the response concentration Cr and Cr est are not clearly explained. Please clarify this issue in the paper.

Answer: Cr(t) represents the time-dependent measured response concentration of solute (M L-3) and Cr est represents the time-dependent estimated normalized response concentration of solute (M L-3).

Referee: Page 1573: The symbol Rf in equation (10) is not defined. In equation (9), the symbol inside the Laplace operator should be L[f(t)]. Line 17 reads "The impulse response in time domain becomes...". I think this should be the pulse response according to the boundary condition given in equation (7).

Answer: The symbol Rf represents the retardation factor (dimensionless).

Referee: Page 1576, section 3.3: It is not clear to me the identification of the beginning and the end of the response: How do you define the minimum slope for the start or the maximum duration to determine the end of the response? Please clarify this part of the section.

Answer: By using TDR the time series EC data are monitored for each probe. We

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measured the rising slope of adjacent EC data over the time step, and identified the start of pulse when the slope exceeds a minimum value. In order to avoid a fake start, the data of 3 time steps earlier before the moment of exceeding the minimum slope was taken in account. The EC-level just before the start was set as the background EC value. The end of a response was set either when the background EC was reached or after a maximum duration.

Referee: Page 1576, line 17:" The equation (8) as described by Mojid et al...". I believe this should be equation (10):

Answer: We will rectify the manuscript in the final version.

Referee: Page 1578, line 13: "Figure 2 illustrates an example of an excellent...good fit". I would recommend weakening this statement (and the caption of Figure 2a) a little bit by replacing "excellent" with "good" as I do not see an excellent fit in Figure 2a especially in the tail portion of the curve:

Answer: We will revise the manuscript in the final version.

References

Packman, A.I., and Bencala, K.E.: Modeling surface-subsurface hydrologic interactions. in Jones, J.B. and P.J. Mulholland eds. Streams and Ground Waters. Academic Press, San Diego, CA, 45-80. 2000.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 1567, 2008.