Hydrol. Earth Syst. Sci. Discuss., 5, S674–S676, 2008
www.hydrol-earth-syst-sci-discuss.net/5/S674/2008/

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

Anonymous Referee #2

Received and published: 15 July 2008

The paper presents the results of some tests with a laboratory model that "investigates the lateral inflow processes from the vadose zone to the hyporheic zone then into the river (pg.1582)" and "in order to determine the hydrodynamic dispersion transport parameters in this variably satured environment, a laboratory J-shaped column model was designed (pg. 1582). To this purpose they "determined the transport parameters of the J-shaped column by fitting an analytical solution of the convective-dispersion equation on individual segments to the observed resident breakthrough curves, and by inverse modelling on the entire flow domain for every flux". The main result showed in the paper is that "under satured conditions the dispersivity was fairly constant and independent of the flux. In contrast, dispersivity under unsaturated conditions was flux dependent and increased at lower flux".

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Interactive Discussion

Discussion Paper

The experiments are interesting, but I'm not sure that the proposed laboratory model can represent the situation which it is possible to find in the examination of the hyporheic processes. It is well know, in fact that the hyporheic flux is very complicated, scale dependent and that a key role is played by the presence of bedform on the streambed, hydrological and morphological features like river curvature, pool-riffle sequences or topography-driven groundwater flow. Nevertheless the authors propose a J-shaped column that shows a simplified succession of unsatured zone, satured zone and "river zone" with the aim of studying the "lateral inflow in the hyporheic zone" as quoted by the title of the paper. The proposed model is not able to simulate the complexity of the hyporheic zone by a simply stratification of three zones and, above all, I'm not able to see the "lateral" flow simulated in the laboratory model. The exchange between the streams and the hyporheic zone occurs as a local flow system with water leaving the stream, moving through the subsurface and finally returning to the stream: the proposed experiment shows at the most the latter part of the hyporheic exchange. In other words, I think that the experiment is well done and the results interesting, even if quite expected, but the model doesn't represent the lateral inflow process that happens in the hyporheic zone. It is only possible to think that the simulated model is a river with a permeable bed and impermeable bank. Another question arises from the form of the proposed model. Why do the authors use a J shaped form? What is the physical significance of the curvature that the curves of the model introduce in the flow? This is the principal reason why I suggest a resubmission of the paper with a different title focusing the attention on the proposed experiment or, in alternative, a better and physically based explanation of how the proposed model can describe a "lateral hyporheic inflow".

Moreover there are some questions that arise from the fig. 2b (page 1592). The authors show the figure as an example of a "less good fit on the observed pulse-response..." but it is inapprehensible for me because there is an evident cut of the picture in the correspondence of the peak of the response pulse. It is evident (by the small sign near the top axes and by the different time steps for the signs near 200000 seconds) that probably there are some measurement results, not reported, showing a surprisingly increase of the peak height that the authors should justified, not only as a "less good fit" of the numerical model. The simulated process is a diffusive process, as stated by the used mathematical model, so more explanation is required for the peak increase. A similar problem is reported in fig. 8b where there is also a local peak growing in the experimental data while the numerical model correctly doesn't report it.

At the end there are some corrections that I suggest and some minor typo

1) eq. (2): the dimension of λ is L only if $n = 1$. Better explanation is required

2) page 1570: I think that the principal weakness of the TSM is the fact that the model neglects the wide range of residential times proper to the hyporheic phenomena

3) eq. (9): there's a bit of confusion in the typographical signs for the Laplace transform and it is necessary a better explanation about the time dependance of the subscript $r(t)$ and $in(t)$

4) eq. (6): there is x direction while eq. (1) shows a z direction. It is a typo or x and z are different directions?

5) Page 1574 and 1576: it is not clear the mathematical procedure; it should be better explained the passages regarding the Laplace transform and the convolution integral. After that it will be probably clear that the solution is not given "by convoluting the input with Eq. (8)". This latter is a boundary condition.

6) Fig. 7: is there a physical reason for the exclusion of the outliers?

7) Fig. 8a-8d. A unique temporal scale for the abscissas of the four pictures makes, in my opinion, the reading of the fitting between the measured and simulated values very difficult. I suggest a cut at 5 and 4 days for 8b and 8c/d respectively.

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