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Hydrology and Earth System Sciences Discussions

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Interactive Comment

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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We greatly appreciate the helpful comments on our manuscript given by the anonymous referee #2. In this response we answer to the questions from the referee. In the final version we will revise our manuscript according to the opinion of the referee.

Please also see response to referee #1.

General Comments

Referee #2 mentioned that he/she is not sure whether the proposed laboratory model can represent the situation in the examination of the hyporheic processes. Referee also points out the key role of the hyporheic processes is by the presence of bedform on the streambed, local hydrological and morphological features driven groundwater flow (S675).

As also responded to referee #1, we do not claim that the laboratory model and our analysis is the ultimate solution to the very complex reality of the hyporheic zone embedded in the river and the surrounding aquifer. Our general objective was to gain more insight into the lateral inflow into the hyporheic zone so that this particular aspect can be incorporated.

We fully agree that the hyporheic processes is governed by the flow-induced pressure differences over the riverbed, and the pressure changes can be due to riverbed irregularities or waves or flood hydrographs (P1569 L17-20). We have also pointed out that many studies have investigated the stream-subsurface interactions resulting from topographical features of the riverbed (P1569 L23-26) as the comment of the referee, however, usually the dynamics of lateral groundwater inflow are not considered (P1570 L5-7) in the previous studies.

As mentioned also in response to referee #1 we are currently investigating the effect of changes in pressures in the river by using water at different temperatures and data-logging thermocouples. Although our initial results are promising it was not yet appropriate to report this ongoing research in the current manuscript. We could of course add this as a recommendation for research. In our laboratory model the lateral groundwater inflow represents the flow coming from the adjacent aquifer across the hyporheic zone into the river. Our specific objective is to characterize the transport processes and parameters of dissolved material by hydrodynamic dispersion in the hyporheic zone in conjunction with the adjacent aquifer and soils (P1570 L16-18). Therefore, we proceeded in our study by using the J-shaped column which represents a schematic flow line in the simplified succession of unsaturated recharge zone, saturated zone (aquifer) and the river. This setup as compared a vertical tank has the advantage that classical identification techniques for columns can be used. A vertical tank would not allow this approach. 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 found that the data in the bend were still meaningful.

For the second part of the general comment from the anonymous referee, we reported that an anomaly occurred due to different packing of sand in the segment between probe No. 2 and 3, in which the transport was much slower than the adjacent segments (P1579 L22-26). We reported the improvement in inverse modelling was by changing the parameter alfa; in the soil water retention function for segment between probe No. 2 and 3, the shape of the soil water retention curve according to van Genuchten (1980) and the hydraulic conductivity was then modified (P1580 L18-22). Accordingly, the inverse modelling was performed by optimizing two parameters: the longitudinal dispersivity for the region before and after probe No. 2 and 3 with higher alfa value and the longitudinal dispersivity for the segment between probe No. 2 to 3 with lower alfa; value (P1580 L26-27 P1581 L1-2).

Specific Comments

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

Answer: The dimension of dispersivity is commonly reported in Length, whereby by definition n = 1. However, it is found empirically that coefficient n ranges between 1 and 2. In our research actually the coefficient 2 is found. We will be more explicit on this issue.

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.

Answer: Thanks for this helpful comment. We will integrate this viewpoint in the final revised version.

3) eq. (9): there is a bit of confusion in the typographical signs for the Laplace transform and it is necessary a better explanation about the time dependence of the subscript r(t) and in(t).

Answer: Thanks for this correction. The typographical signs for the Laplace transform

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will be revised accordingly.

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?

Answer: Thanks for this correction. Eq. (6) will be revised in the final version with z as the axial distance.

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.

Answer: The estimated response concentration (Cr.est) is predicted by convoluting the input with eq. (10). The mathematical procedure of determining transport parameters from solute breakthrough data in this study is based on the transfer function method published in Mojid et al., (2004) and Mojid et al., (2006). As both articles are published in international refereed journals and we therefore did not repeat our description. If wished by the editor we could add more detail regarding to the Laplace transform and the convolution integral. We feel that this would be appropiate as appendix in the revised version. Please note also that our method is different from the approach by Jury (1982).

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

Answer: The two outlier data are in the unsaturated section of segment between probe No. 1 and 2 and segment between probe No. 3 and 4 of flux = 2 cm hr-1. The measured dispersivities are relatively low if compared with the data of the same segment in other fluxes.

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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Answer: We originally wished to stress the difference in flux by using the same time scale. It will indeed be better use of paper to use two different temporal scales.

References

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