Hydrol. Earth Syst. Sci. Discuss., 5, S1452–S1460, 2008
www.hydrol-earth-syst-sci-discuss.net/5/S1452/2008/

Hydrology and Earth System Sciences Discussions

HESSD

5, S1452–S1460, 2008

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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Received and published: 13 October 2008

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In response to the editor, Dr. Nunzio Romano, and the two referees we feel that we should structure our comment as an answer to the following three questions:

1) is our work and paper relevant and contributing to a better knowledge of lateral inflow into the river via the hyporheic zone?

2) is our work, notwithstanding the answer to question 1 and the above objectives, methodologically correct?

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3) what is the relation of the current manuscript to the ongoing experiments using temperature as a tracer?

1) Question 1: relevance of the paper

According to both referees the main weakness of our work is that our laboratory experiment and our analysis of the data by a soil physics approach is irrelevant to the hyporheic zone.

We quote the referees: Referee #1: "in the experiment, part of a J-column is referred as the hyporheic zone which in my opinion cannot be referred to as a hyporheic zone".

Referee #2: "I'm not sure that the proposed laboratory model can represent the situation"

Referee #1 tries to prove his point by attacking the classical physical model for convection dispersion. We quote literally referee #1: "Both in TSM and in other models, the governing equation gives the concentration, is different than the classical advection dispersion equation (ADE). Therefore, the classical advection equation should not be used". We should point out that this reasoning appears to be a "Reductio ad absurdum" argument. In our opinion such statement is not appropriate, moreover the use of "and other models" along with TSM in a contrast to the ADE is not a correct statement. Firstly, there is no such thing as an absolute truth (in this case the TSM is almost dogmatically declared by the referee #1 as an absolute truth and he throws in a lot references, almost all by the same author, to prove his point) which therefore in his opinion excludes other representations and models. Especially in hydrology where equi-finality is a well known phenomenon, such claims should never be made.

Secondly by stating that (we quote) "in TSM and in other models, the governing equation gives the concentration, is different" he appears to imply that the ADE does not give the concentration. This is not correct, the ADE has the concentration as variable.

It is appears that referee #1 feels attacked by our paper in his belief in the TSM. More-

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over, the ADE is different in the sense that the TSM is a perceptional and empirical model while the ADE is fully based physical model for flow in porous media and with a soil physical base and a more general validity. The TSM is also largely based on tracer experiments in longitudinal sections of rivers. Our setup starts from a different soil physical angle. Important is also to remark that by ADE we mean the full range of convection dispersion models. Both models have their own merits and it is in the interest of progress and open speech that no monopolies are declared.

Referee #1 in his point 4 also confuses our statement about flow-induced pressure differences. According to referee #1 we apparently overlook other causes and should have included Packman and Bencala (2000) in our reference list to have a more complete description. However, what we mean it that the main driving "force" (not in the pure physical sense) is the flow-induced pressure difference. We should point out the difference between causes and mechanisms. We do not deny other mechanisms and we should point out that the ADE includes the (according to referee #1) "missing" concentration gradient and the mass transport. The advection dispersion equation is a partial differential equation, which combines mass transport (by advection) with hydrodynamic dispersion (combination of diffusion due to concentration gradient and of mechanical dispersion). The ADE is derived from physical laws. HYDRUS can be considered as the most applied model for heat, mass and water transport in variably saturated media (see later under methodology). The ADE is a more complete and physically based model as compared to the perceptual and conceptual TSM.

Most striking about the review by referee #1 is that he refers to only one category of articles from one good US-journal WRR (Water Resources Research). He only proposes articles confirming the TSM. We tried in our literature review to come to a more balanced perspective. We indeed included a number of papers by Bencala. We wish to point out that there are some very interesting papers in other journals and that TSM is not the only approach.

Referee #2 states "the simulated process is a diffusive process". The word "diffusive" might have been accidently used, but we should point out that he should have written a "dispersive" process. "Diffusion" refers to molecular diffusion only, while "(hydrodynamic) dispersion" is the combined effect of mechanical mixing during convection (or in other words advection) and molecular diffusion. In most applications in which water flow is present, as in our experiment, the proportion between diffusion and dispersion can be characterized by the dimensionless grain Peclet number. Often the mechanical effect is orders of magnitude larger than the molecular diffusion. The mechanism identification is very important both to the TSM and to our approach. It means that with larger grain Peclet numbers the water fluxes in general are conveying the solutes with a dispersion on the microscopic pore water velocities and that the pure diffusive transport of solute by gradient differences is less important. With lower Peclet numbers both mechanisms are important or for Peclet numbers lower that a certain values the diffusion is predominant.

Referee #2 makes a very interesting comment; we quote: "the principal weakness of the TSM is the fact that the model neglects the wide range of residential times". We agree that this is one of the weaknesses of the TSM. The mechanical dispersion is caused by a variation of the microscopic pore water velocity in a porous medium. This variation in velocities (inversely related to travel times) causes indeed a variation in residential times. This is not neglected by the ADE (advection DISPERSION equation).

In our opinion the major weaknesses of the TSM are that firstly the hyporheic zone is conceptualized as an arbitrarily fixed volume below the river bed adjacent from the water in main river channel and secondly that the groundwater in (or out)-flow from the river is not represented well. The bulk of parameter fitting for the TSM is done on the basis of tracer studies in the river. The interaction with the surrounding aquifer is at best treated as a fitted parameter (like an assumed constant inflow).

We wish to remove the word "hyporheic zone" in our title and give the manuscript a new title "Laboratory study of hydrodynamic dispersion characteristics of lateral inflow into a river". Our work is a contribution towards building a meaningful numerical model

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(like HYDRUS) for a real-life complex layering but including the surrounding aquifer into the geometry. We selected for our laboratory experiments a homogeneous flow domain and not a complex layering for the very simple reason that with a complex layering we would not be able to unravel the structure of the quantitative relation of the dispersion coefficient (and dispersivity) in function of the flux and the water content. We also selected a J-shape and not a vertical tank in order to have as much as possible one-dimensional flow lines. With hindsight we could improve our set-up by including a horizontal section between our two bends. Our paper is relevant because it contributes to a better understanding of lateral inflow over a wide range of water content (saturated and unsaturated) and a flow-range. If we would have used a tank laboratory model with a complex (more realistic) layering we would not have been able to identify and derive the structure of our general relation. Rather in a 2D tank we could only have estimated the parameters by inverse modeling. Now we were able to confront inverse modelling to a transfer function method applied on individual segments.

We also wish to point out that others have using laboratory studies (Tonina and Buffington, 2007 and Elliott and Brooks, 1997) in the context of hyporheic zone. It is our belief that field studies should be predominant but that such studies can be supplemented by occasional laboratory studies. The two laboratory studies on hyporheic zone however were flume experiments without lateral inflow. So, our study concentrates on lateral inflow and aspect neglected in other studies.

The referees also criticized the use of homogenous sand in our experiment. It is to be recognized that the river bed just below the river and the surrounding aquifer can be of a different composition. Depending on the flow regime of the river the grain diameter of the immediate river bed is often rather coarse and similar to the sand in our experiment. In our opinion the sand we used is closer to riverbeds (in our Belgian conditions) and less representative for our surrounding aquifers. In the North of Belgium with sandy soils and flat topography the situation we created in our laboratory model is rather close to the reality. Important is to point out that our model study contains a laboratory

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and a numerical model aspect. It is not difficult to adjust the numerical model HYDRUS 2D/3D to a real-life geometry and layering.

It is fair to say that our study concentrates on lateral inflow and in its turn has not included longitudinal river flow. The processes in the hyporheic zone are complex and therefore we should unravel this problem in a systematic step-by-step manner. We make contribution to the aspect of lateral inflow.

2) Question 2: quality of the experimental and analytical methods used.

In our opinion both referees did not review our methods and analysis in sufficient detail. Referring to point 1 both referees clearly feel that our paper should be rejected outright because they dislike the laboratory setup and the use of ADE (and therefore also the numerical model HYDRUS) instead of the Transient Storage Model.

Referee #1 limits himself to a few queries on the equations, but does not present any fundamental issues. This is somewhat logical as he rejects the ADE.

Referee #2 says, we quote: "The experiments are interesting" and later ,we quote, "I think that the experiment is well done and the results are interesting."

The measurements of Electrical conductivity and water content by TDR are well established and our setup was properly calibrated. We already earlier pointed out that the transfer function method is described in 2 refereed papers (Mojid et al 2004 and Mojid et al 2006) in major soil science journals. The first paper presents and explains the transfer function method. The second paper shows that the determination of the tail of the response has a limited influence on the parameters determined by the transfer function method. If requested we can provide a short annex summarizing our transfer function method. As this method was published in the European Journal of Soil Science which has in 2007 an ISI Impact Factor of 2.730, which is the second highest one in the soil science category, we felt that we could refer a published method. However, if requested an annex can be added.

The criticism by a referee that the transfer function as in our equation 10 does not include space is wrong as space is indirectely present in Greek letter "Tou". The average travel time "Tou" is calculated by dividing the length of travel (or space) by the average pore velocity V.

The use of the HYDRUS 2D/3D model and inverse modeling is also an established methodology. In Google Scholar the search term "HYDRUS model" gave on 12 Oct 08 a total of 2010 hits. Interesting is that search term "TSM hyporheic" only gives 36 hits. Some of these hits however refer to total suspended matter (TSM) and not to the model and also this current HESSD added 2 hits for TSM. Important is to point out that at the same time a large number of recent papers have been published on the river-groundwater interaction which used HYDRUS. With "HYDRUS hyporheic" I got 21 hits but with "HYDRUS river" 610 hits Unfortunately "TSM river" gives mainly hits on total suspended matter and cannot be compared. If we refine to recent publications (2005 onwards) we keep 241 hits for "HYDRUS river". So, claiming that the ADE and a numerical model cannot be used for the hyporheic zone is contradicted by a large amount of literature.

Our original analysis contribution is twofold: very little studies have been made over wide range of variable saturation (from saturated to unsaturated) and we identified the dispersion characteristics by confronting two methods (inverse modeling and transfer function fitting). We also propose also to include "hydrodynamic dispersion characteristics" in our new title.

Personally we feel that our laboratory study is well done both in technical setup and in analysis method. As HYDRUS is a well-established model, a more detailed study of the hydrodynamic dispersion characteristics like our study is an appropriate and relevant contribution using a well established methodology. We would have loved to have a more detailed review on the method and analysis.

3) Question 3: the relation of the current manuscript to the ongoing experiments using

temperature as a tracer?

It is important to point out that the ongoing experiments are confirming the usefulness and relevance of our laboratory setup (point 1 of our final reply) but do not form part of our analysis as part in our current manuscript (point 2 of this reply). Therefore the validity of the discussion and conclusion does not depend on these experiments.

In the ongoing experiment, which is almost completed experimentally and also the analysis well advanced, we imposed the same steady state lateral inflow from the left. However we change the water level on the right (representing the river) by a step function. At the same time we change the water temperature on the right by a simultaneous step function in order to introduce pressure changes by changing water level in the river. The change temperature is used as a tracer. Thermocouples monitor the temperature changes inside the laboratory model. By this temperature tracer we can estimate how the water velocities reverse as caused by changing water level in the river under the river bed. In other words in our experiment we can mimic the "pumping" action of level changes in the river. Our initial simulations by HYDRUS (which includes water flow, solute transport and heat transfer) confirm that the ADE can indeed be used. However, this experiment is independent from the study of the hydrodynamic dispersion characteristics. Several field experiments use the natural variations in temperature of the river water as compared to the more stable temperature of the surrounding aquifer.

By mentioning the ongoing experiments we tried to demonstrate that the laboratory setup is worthwhile as an experiment and not an occupational therapy in a comfortable laboratory environment. It would be impossible to describe the two experiments in one paper as the analysis of the flux by using heat equation along the advection dispersion equation would too much material for one paper.

Conclusions

Our paper has to be seen as a contribution to a better understanding of the lateral inflow and how elements (like solutes, oxygen) in the water can be transported. We

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therefore propose to change the title into "Laboratory study of hydrodynamic dispersion characteristics of lateral inflow into a river". This title is more precise. We also propose to reformulate the introduction so that it is more focused to lateral inflow.

We tried to give a balanced view and not a narrow-minded confirmation of the TSM. Our paper describes a completed part of the research. The ongoing experiments confirm the usefulness of our laboratory setup and analysis method by HYDRUS but are not contributing or adding to the current manuscript.

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