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Biogeosciences Discussions

Interactive comment on "Quantifying biologically and physically induced flow and tracer dynamics in permeable sediments" *by* F. J. R. Meysman et al.

F. J. R. Meysman et al.

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We thank the referee for his extensive, constructive and accurate comments and suggestions. These really have improved the manuscript. We apologize that we did not better acknowledge the body of work that is available in the engineering literature (of which the work by him and his coworkers forms a substantial contribution).

1. (a) We agree with the referee that import references are missing, particularly from the engineering literature, although it was certainly not our intention to overemphasize our own contribution. The literature on pore water transport spans widely different disciplines (engineering, hydraulics, geochemistry, ecology), and it is not easy to give a balanced review of this in just a couple of introductory lines. Unfortunately, but somehow understandably, the references in the previois version somehow reflected that part of the literature with which we are most familiar with (our home discipline of marine ecol-

ogy and sediment biogeochemistry). We have now rewritten the last two paragraphs of the introduction, adding references to have a better coverage of the engineering literature as well. (b) Somehow, there seems to be a misreading about what we claim and what we do not claim. The text did nowhere claim that the model development procedure and testing procedure is our own invention, nor that it is novel or unique. This would indeed be an impermissible neglect of prior work. We have now rewritten the last two paragraphs of the introduction to emphasize this even more.

2. The word 'generic' has been interpreted by the referee in a totally different way as intended. Our intended interpretation of the word 'generic' was to indicate that all four applications, that is both biological and physical induced pore water transport, can be modelled by the same equations (as the Title suggests, and as the Introduction elaborates upon). It was not our intention to distinguish our approach as different from the rest, or to say that previous approaches are non-general. To avoid such confusion, we have now deleted the word 'generic' and 'general' altogether throughout the whole manuscript.

3. The starting point to model the flow pattern in the sediment is indeed the classical volume-averaged momentum equation of multiphase physics. We have now made this explicit in the first sentence of the section. In essence one has to start somewhere, and this depends on the nature and scale of the problems examined. For a thermodynamicist whose daily pass-time is statistical mechanics, even the unaveraged equations of continuum mechanics may form an unacceptable crude description. But for the problems we are interested in (flow and transport in sandy sediment over the 10-100 cm depth scale), equation (1) is a good starting point. But the referee is right that we did not properly constrain to which problems are modelling applies. To make this clear we now explicitly describe the class of applications to which our model formulation applies upfront.

4. The homogeneity assumption was not only mentioned in section 2.4, but already stated in section 2.2 (p1816-L24). We have now reformulated the latter sentence to

BGD
3, S1085–S1088, 2007

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emphasize this even more. We were not aware of the (impressive) work by Salehin et al. (2004), and so we have now included it.

5. We agree completely with the referee. We have now added a cautionary note at the end, referring to Zhou and Mendoza (1993). However, the main focus of this manuscript remains an inter-comparison of transport at depth, rather than a detailed description of the interaction with the free flow over the sediment.

6. The reaction term R in Eq (6) can be spatially dependent, and hence, does not need to be homogeneous.

7. The referee has a different interpretation of the words 'generic' and 'general'. Our claim was definitely not that we have a new, general approach& to model transport in porous media, only that we have applied a single modelling approach to very different problem settings. And effectively, our modelling approach is exactly the one that is common use (and has also been used by the referee). To avoid this unfortunate confusion, we have deleted all references to the terms 'general' and 'generic'.

8. (a) The first point re-iterates the remark that our approach is not 'general' (see discussion above) (b) Second point: problems with the numerical implementation. We agree completely with the referee that k-epsilon model is too primitive, and that the k-omega model would be a better choice. We tried to convey this, but apparently did not succeed in getting this message across. So we have now rewritten this section to remediate this. Secondly, it would indeed be useful to examine periodic boundary conditions, and these are the simulations that we are presently working on (however, including this in the present paper is not feasible).

9. We were not aware of the work of Salehin et al. (2004), and we have now rewritten this section to give it proper credit. Furthermore, we have toned down our statement removing the back-of-the-envelope sentence. Still we stand by our point that the testing of model predictions to actual data remains in a stage of infancy, and should get far more attention.

BGD

3, S1085–S1088, 2007

10. We were note aware of the work of Rutherford et al. (very interesting indeed), and we have now fully acknowledged it.

11. The laminar versus turbulent problem may not be an issue in the engineering literature, but it is in the realm of marine biogeochemistry. The standard modelling approach here is to consider laminar flow field over the sediment (notably the work by Khalili and coworkers). Moreover, our intention was mostly to bring the problem to the table: simulations of tracer dynamics in the sediment are highly dependent on the actual model that is used to describe the free flow (laminar vs. turbulent; and in turbulent, k-epsilon vs. k-omega). Although this may be an obvious thing to engineering world, this issue is not as such conceived by the average biogeochemist.

12. The requests of the referee are justified and pertinent and have been implemented in the revision.

Interactive comment on Biogeosciences Discuss., 3, 1809, 2006.