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

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Summary comments

This paper presents an interesting set of simulations of various cases involving interfacial transport and flow in permeable sediments. This type of comparison is timely as there is currently considerable interest in the biological and biogeochemical processes induced in various sedimentary environments by advective pore water flows. However, I find that the authors greatly overstate the contribution of the work relative to the results presented. While the authors claim that they present a new, general approach to modeling this problem, I find that they follow well established procedures in fluid mechanics and employ essentially the same methods that many prior researchers have used to address the interfacial transport problem. Most tellingly, while the authors claim to use

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a general and basic modeling approach, ultimately they employ exactly the same types of assumptions to treat each individual case that have been used in developing prior solutions.

I think the authors need to do a more careful job of describing the work that has been done to date on this problem in order to provide a more appropriate context for their own work, and also do a better job of discussing the limitations imposed by the assumptions that they have made. And some of the assumptions the authors have used in developing the models are indeed highly limiting. Despite these problems, I think there is the essence of a good paper here - the basic comparison afforded by analyzing different cases with the same modeling package is useful - but the presentation simply must be made more balanced and better reflect the true state of the field.

Detailed Comments:

1. The background on pore water flows is considerably incomplete. Admittedly there is a lot of literature on this topic and it cannot all be cited, but when the authors claim to undertake a serious review of prior work then they must make this balanced. However, the examples given represent only a narrow cross-section - largely restricted to the prior works of the authors - and do not provide the necessary balanced view of the literature. Many key citations are missing. I view this as a serious deficiency of the paper, as the authors clearly either have not read all of the relevant prior work or choose to provide an unbalanced view so as to over-emphasize their own contribution. For example, the model development and testing procedure that the authors claim as their own invention (p. 1813) is exactly that advocated by Elliott and Brooks, and consistently adopted in many subsequent studies. And this was certainly not an invention even of Elliott and Brooks, but rather derivative of a long series of solute transport studies. There has also been considerable study of reactive particle and solute transport in pore water flows, including modeling efforts for oxygen utilization in ripples/dunes and for multi-phase reactive transport in pore waters (e.g., Rutherford et al., 1995; Ren and Packman, 2004). I do not understand why the authors choose only to cite their own

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work here.

Similarly, the approach to develop a general model framework and apply it to numerous cases as defined by differences in system geometry and boundary conditions is not unique to the authors. Indeed, this is the basic approach in fluid mechanics, and many of the prior studies on advective pore water transport have put their work in this context.

Despite this, I think the authors do a good job of summarizing the challenges in assessing pore water transport in sandy sediments (bottom of p. 1811 to top of p. 1812), and outline a good approach to address this problem. They simply need to make the introduction more balanced either by restricting themselves to general statements with a few key citations (not only from their own prior efforts!) or undertake a more serious review of the relevant literature.

2. Section 2.2 "Model development: a generic approach for pore water flow and reactive transport" is mis-described, as many elements of this are not truly generic. Using the same software and experimental procedures does not make the work general: it is still subject to exactly the same constraints as other modeling studies. I note specifically that the modeling approach described on the bottom of p. 1814 is a very basic one for fluid mechanics. While I appreciate the authors' careful treatment of the problem, there is no need to overemphasize this point. I think it is more fair to say that this study achieved a good inter-comparison of the specific transport processes investigated in this study, but not more than that.

3. I also disagree with the characterization of Eqn. 1 as a very "complete" or "general" equation for pore water flows. Any formulation based on the Darcy velocity already implies averaging of the basic fluid flow equations. Further, many pore-water flows are coupled with the overlying free flow, and so Eqn. 1 cannot be applied throughout the entire problem domain of interest in many applications. It might be useful for the authors to review the work of Zhou and Mendoza here. There is nothing wrong with Eqn. 1 per se, but it is important to recognize that this already represents a considerable

simplification of the basic fluid flow problem and it may not be applicable for all pore water flow problems (or more particularly, for all problems of coupling between free flows and pore flows). Again, the authors here need to be more balanced in providing the context for their work.

For these reasons, it would be better to combine this section with the following one and simply describe it as part of the development of the flow model. Further, the extensive discussion presented in the current Section 2.2 can be eliminated, as in the end the authors apply the same basic equations used in almost all of the previous studies of advective pore water flows in permeable sediments.

4. It is *very* important to note that this model only applies for a homogeneous, isotropic porous medium. More complex behavior needs to be considered in heterogeneous sediments, as shown by Salehin et al. (2004). While this is currently mentioned briefly at the beginning of Section 2.4, it needs to be addressed in the model formulation and not simply in the implementation.

5. The final statement of the current Section 2.2, "for practical modeling applications in sandy sediments, the Brinkman and other non-Darcian terms can be justifiably discarded" is also overstated, as this depends on the application of interest. A better statement would be that these terms can be neglected when the behavior of interest occurs at depth in the sediments (or throughout the depth). Problems involving surface behavior (e.g., very fast reaction rates, some deposition problems) would likely be influenced by the enhanced transport at the interface.

6. In Equation 6, it is also necessary to note that the reaction term in this form assumes that the reaction occurs homogeneously throughout the sediments. This is an important limitation.

7. In examining the model application to the various cases, I am surprised to find that very simplified approaches are used. Given the claims of the introduction regarding the development of a general modeling approach, I expected to see only minimal assump-

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tions applied to the analysis of the experimental cases. Instead I find that the analysis is treated with exactly the same types of assumptions that have been in common use for this type of problem for at least 10 years. For example, highly simplified geometries are assumed in all cases. Further, use of Equation 12 for the boundary pressure in the benthic chamber case exactly follows the approach Elliott and Brooks used for the case of flows induced under bedforms. This certainly does not provide a general solution or even a general approach for the solution of the interfacial transport problem.

8. The simulation of flow over/through ripples does include a new approach as it seeks to directly solve the flow-boundary interaction, but still suffers from significant limitations in the implementation of the numerical model. First, I note again that the model framework is not general, as numerous assumptions are made that restrict the application to a particular class of flows (e.g., overlying flow deep relative to roughness height, greatly sub-critical, and low rates of interfacial flux relative to the overlying flow). Second, I see two problems with the numerical implementation. Most importantly, the k-epsilon turbulence closure model is relatively primitive and probably represents too great a simplification to resolve the boundary interactions of interest here. We have done some of these simulations and found that the k-epsilon model does not do a good job of capturing the recirculation in the lee of the bedform or the distribution of pressure over the sediment-water interface. Instead, the k-omega model is a much better choice. Beyond this, including a flat-bottom inlet section followed by a series of ripples means that the authors are actually investigating the development of a boundary layer over the ripple forms, and these results are expected to differ from the case of a fully-developed flow over ripple forms. It would be preferable to include periodic boundary conditions in the numerical model.

9. In section 4.1 (p. 1830), the authors again greatly overstate their contribution. The use of tracers to probe pore water transport processes is very standard. There has been considerably more done with this than the "back-of-the-envelope" calculations the authors claim. Numerous studies have performed detailed quantitative analysis of

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tracer transport, including both evaluations of net tracer flux and pore water flow paths. Two prominent examples that use methods essentially identical to the authors' can be found in the work of Elliott and Brooks, 1997 (which the authors cite) and Salehin et al., 2004 (which they do not cite). In fact the comparison presented by the authors here is less stringent than that used by Salhein et al., as in that work we also examined the ability of the numerical pore water transport model to represent dye penetration patterns in the subsurface.

10. In terms of modeling oxygen dynamics, the authors essentially follow the approach of Rutherford et al., 1995, who included 0th- and 1st-order oxygen consumption in the bedform-induced advective pore water transport model of Elliott. This should be acknowledged in the manuscript.

11. I don't really understand why the authors include extensive discussion of the differences in the turbulent and laminar flow models. It is well known that turbulent flow over roughness elements such as ripples yields a flow separation and recirculation that produces strong boundary pressure gradients. The implications of this for interfacial transport were discussed in detail by Elliott and Brooks, and I believe were also mentioned by Thibodeaux and Boyle. Therefore the laminar flow model should be expected to significantly under-predict the interfacial flux, and this needs only a brief mention in the paper.

12. Despite the difficulties I have found with the claims made in the introduction and results, I still find that the conclusions of the paper are generally good and carry a valuable message for the research community. Some claims still need to be moderated here. Specifically: 1) this work does not really provide a general modeling approach, but rather represents implementation of standard methods in fluid mechanics to this problem, 2) in-depth comparisons of tracer transport in pore waters and model predications have indeed been carried out (e.g., we did this exactly in Salehin et al., 2004), 3) numerous reactive transport models have been developed for various applications involving permeable sediments (e.g., Rutherford et al., 1995; Ren and Packman, 2004).

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Some useful references:

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Rutherford, J.C., Boyle, J.D., Elliott, A.H., Hatherell, T.V.J., and Chiu, T.W., 1995. Modeling benthic oxygen-uptake by pumping, Journal of Environmental Engineering-ASCE, 121(1), 84-95.

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