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Discussions

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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General Comments

This manuscript outlines the application of numerical flow and reactive transport modeling to four different geometries but with similar governing physics. The Darcian groundwater flow equation and advection-diffusion-dispersion transport equations for an inert (Br) and reactive tracer (O_2) were solved for (1) lugworm bio-irrigation under "neutral" conditions and (2) under ambient groundwater seepage, (3) stirred benthic chambers, and (4) rippled-induced flow, all in sandy sediments. The authors use COMSOL Multiphysics- a generic finite-element software. Their results illustrate the applicability

of numerical flow and transport simulations in understanding and quantifying coupled physical and biogeochemical processes occurring in permeable sandy sediments, and on how such methods can be used for integration of multiple processes into one interpretive and quantitative template. The paper is a novel demonstration of this, particularly with the breadth of scenarios and processes tackled. However, the paper is somewhat weaker in terms of major scientific contributions and there is no clear scientific goal. The authors do no pretense of doing so, and it is apparent that the intent of this manuscript is to illustrate the potential and robustness of their modeling approach. Related scientific discoveries have been presented elsewhere by the same group. Nonetheless, the work is timely and the audience of *Biogeosciences* will find the methods presented very useful and easily adaptable, especially that the authors have willingly shared their modeling scripts.

The authors should provide a more cautionary note to the readers, particularly with coupling water column flow with porous media flow. Their results for these cases are most likely at odds with reality and I suspect that it will not pass rigorous validation. Explicit statements about potential problems should therefore be included. The power of COMSOL coupled with unsurpassed user-friendliness (and other similar commercially-available products) is an exciting new tool for the community, and papers like this rightfully promote their increased application. I do feel that the very strength of the approach used here also opens it up to misuse and abuse. But this potential pitfall, albeit negative, needs equal billing in the paper.

Except for some minor errors, the paper reads well. Some readers might prefer that each case's description be followed by the corresponding results instead of going through the methods/ conditions for all four cases first and then followed by the results for all four.

Specific comments and technical corrections

Title: *Quantifying* may not be the best descriptor and could be misleading. Some readers might think that the paper introduces a new measurement method. Perhaps changing the first words of the title to *Quantitative modeling of-* would be better. In fact, the paper is solely about a novel modeling approach- this should be reflected in the title.

pg 1811, ln 3: *Rusch* is missing S.

pg 1812, ln 5: Reimers et al. is not in reference list

pg 1811, ln 26: References to Cardenas and Wilson (2006) includes my first name, plus the reference is not in the bibliography. Other related references are listed below.

pg 1813, ln 10: A reference to the papers by *Ren and Packman* on reactive co-transport s the sediment-water interface might be appropriate here.

Bottom of pg 1815 to top of pg 1816: Some authors also suggest that the Darcy-Brinkman equation is valid only for media with high porosity (>0.9) which is not typical of natural sands or even gravel (*Durlofsky and Brady,* 1987).

pg 1817 to pg 1818: For most people dealing with transport in groundwater systems, it is more customary to define a longitudinal and transverse dispersivity, α with units of length. α is determined from empirical studies relating the problem-scale to dispersivity (e.g., *Gelhar et al.*(1992)). The longitudinal and transverse dispersion coefficients are then defined as (*Bear*, 1972):

$D_{ij}=\alpha_T U\delta_{ij} +(\alpha_L-\alpha_T)u_i u_j/U$

where α_T and α_L are transverse and longitudinal dispersivities, U is the pore velocity magnitude, and δ_{ij} is the Kronecker delta function (i, j are indices), similar to equation (8) in the manuscript. The definition of the dispersion coefficients following equations (9) and (10) (as in Oelkers) is probably valid for the scale of laboratory columns, such as of this study.

pg 1818, ln 8: *-Peclet numbers in the range 1-100.* The dash became a "divided by"

symbol. *–(this was verified a posteriori)*. The *verification* should be described. What are the Pe's for the simulations?

pg 1818, ln 15: Insert "a" between *this is* and *clear topic*.

pg 1821, ln 25: I suggest repeating here that the "M-file" is the model script.

pg 1824, ln 23: *The advantage of modeling is that it is always applicable.* This is not necessarily true. Models can be very limited in application and may even be nowhere close to reality.

pg 1826 and 1827: We have now done models for turbulent flow and discussed the differences between laminar and turbulent water conditions, particularly on the effects on flow in the sediments. However, the papers are still in review. In the meantime, you can find the results in my dissertation which can be downloaded at:

http://infohost.nmt.edu/ cardenas/CardenasDissertationNMT.pdf

pg 1827, ln 25: A velocity of 10 cm/s is more likely to be fully-developed turbulent flow rather than laminar- the laminar simulation would not apply in this case. It is best if the authors emphasize that the results here are for demonstration purposes only.

pg 1830: Note that we have validated to experimental data both for turbulent flow above ripples- including flow parameters, turbulence quantities, and most critically- the pressure along the sediment-water interface. Perhaps more importantly, we have also been able to replicate the experiments of Elliott and Brooks.

pg 1835, ln 15: *this assumption was clearly invalid due to the limited volume of overlying water small.* I think there is a missing "was" between *water* and *small.*

pg 1836, ln 21: *Huttel* is missing an "e"

pg 1837: Note that this problem is directly analogous to the problem of injection/ extraction in aquifers with mean regional groundwater flow (*Javandel and Tsang,* 1986). In fact, there are analytical expressions describing the shape of the bell-region or in groundwater speak- the “capture zone”. Javandel and Tsang also show superimposed capture zones for multiple wells (or lug-worms)!

pg 1838: A more rigorous scientific study would require consideration for variable density flow when dealing with “saline” and “fresh” water. Note that we have also conducted similar experiments for ripple-induced flow under ambient downward and upward flow. It would be interesting to see results of a sensitivity analysis. At what groundwater discharge will the lugworm not be able to pump water or at least form a bell jar around it?

Section 5.4. This is where the major weakness of the paper lies. Both solutions are likely incorrect. The turbulent flow solution presents an eddy that is too large, as indicated by the pressure maximum close to the crest where the main in-flow point is located. Eddy reattachment points, and therefore the pressure maximum, are located at a horizontal distance that is 4*bedform height measured from the crest (*Engel,* 1981). We had the same difficulty with the k-epsilon model, particularly with the choice of and sensitivity to wall function parameters (eg, thickness of log layer). On the other hand, the laminar flow simulation is done at a velocity that is too high for laminar conditions. The authors should mention that no validation was done and that the correctness of the solutions cannot be ascertained. On the premise that this manuscript is a demonstration of the method, this is acceptable. The results may, however, be too weak and suspect for a rigorous scientific paper. Of course, this is easily corrected by doing simulations at much lower velocities- which is what we did (Cardenas and Wilson, 2006). However, in that case, the practical applicability of the results become very constrained and the exercise becomes academic more than anything else.

pg 1840: A brief discussion of the related work by *Rutherford et al.* (1995) on O_2 penetration through ripples would be relevant here.

Section 6.1: We found that the k-omega model is ideal for modeling recirculating flow near wall boundaries. The adequacy of the k-omega model for flow over dunes was first

presented in *Yoon and Patel* (1996). We have used the k-omega model for water and air flow for flow over different bedform shapes and are able to replicate three different experiments: *Vittal et al.* (1977), *van Mierlo and de Ruiter* (1988), and *Fehlman* (1985) which is also in Shen et al. (1990). Our papers on these are still in review but we have presented it at the recent AGU meeting (Cardenas and Wilson, 2006).

Section 6.2; Again, we have presented some model validations. Until our submitted manuscripts are finished with the review process, you can get the information from the dissertation which I mentioned above. Preprints are also available from me if people are interested in them.

Section 6.3: There have been several models of column experiments through sand coupled to reactive transport models, some have been done for field-scale applications in unconsolidated sand/gravel aquifers. What is presented here is the first for the specific cases considered.

pg 1846, ln 25: Missing “return” before Oelkers to start it on next line.

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