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Hydrology and Earth System Sciences Discussions

Interactive comment on "The potential for material processing in hydrological systems – a novel classification approach" *by* C. E. Oldham et al.

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Thanks to the reviewers and the authors for their efforts on this paper. Obviously the topic is of great interest to a wide range of scientists, and I note the independent comments acknowledging the potential value of the paper.

It was my impression upon first reading the paper that: a) The topic is important, and b) The use of a dimensional analysis framework has the potential to be valuable, but c) The paper in its current state is challenging to interpret and the potential novelties of the approach are not clearly argued or illustrated by the examples presented.

This perspective has been, I think, largely confirmed by the reviewers who have offered

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constructive suggestions for refining the manuscript. My recommendation at this point is that the manuscript could become suitable for publication in HESS but to do so requires major revisions. I would like to see these revisions address the following 5 issues:

a) Improve the structure and ease of comprehension of the paper. Reviewer 2 has several useful suggestions in this regard. This issue needs to be confronted in a meaningful way - there are significant structural difficulties that impede reader understanding and obscure the arguments being made. Please revisit the structure and communication of the topics seriously.

b) Provide at least a preliminary treatment of non-diffusive transport problems & incomplete mixing problems. Reviewers 1 and 2 both indicated the significance of these issues; the theory presented needs to at least outline a path towards confronting them.

c) I personally was somewhat unconvinced by the treatment of the isolation timescale. The description presented seems a little incomplete (and I'm thinking here of the parallel to soil moisture dynamics a la the Laio et al 2001 papers). The full description, I would have imagined, depends on a set of 3 related timescales: the overall frequency of switching (from isolated to connected) and then the mean duration of either isolation or connection. I think this matters in that the emergent state of the system will be dependent not only on what happens during periods of disconnection but also on the relaxation from the disconnected state to the connected state. Having worked a little with some simple stochastic reactor models (again thinking about soil processes), the switching frequency (in this setting, determined by the rain event frequency) and the duration of wet periods (determined by both the rain frequency and the rain depth / evaporation rate) were both important to the overall dynamics of solute mobilization and discharge. This importance only escalated when stochastic source terms (in our application, fertilizer) were added into the picture. So, overall, i do question whether the treatment of fragmentation and its stochastic temporal properties is complete here, and whether there is not a second timescale that should be considered.

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d) As Reviewer 2 noted, the treatment of the case studies was not completely convincing as a way to illustrate the power and potential of the proposed framework. There is a need, I think, to make it explicit that the framework allows something new to be done that could not be done with pre-existing treatments (e.g. the classical treatment Reviewer 2 mentioned). If this case can be made convincingly it would be a valuable defense of the ideas in the paper.

e) Finally, there were some great suggestions for broadening the literature reviewed in the current manuscript. It would be (obviously I hope!) valuable to look at embracing those resources.

I intend to send any revisions out for a second review given the nature of the reviews received at this stage.

Yours truly

Sally Thompson

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 10487, 2012.