Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-451-AC2, 2016

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "On the Consistency of Scale Among Experiments, Theory, and Simulation" *by* J. McClure et al.

J. McClure et al.

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Received and published: 6 November 2016

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Response to Review #2 of
On the Consistency of Scale Among Experiments, Theory, and Simulation
J.E. McClure, A.L. Dye, W. G. Gray, and C. T. Miller

hess-2016-451

1 General

We respond to the comments from Referee #2 beneath comments made. The authors' response is shown in red.

2 Referee #2

In this manuscript, physically based upscaling of two phase fluid flow in a porous medium is considered by presenting definitions of microscopic and (macroscopic) averaged properties, and investigating this system with experiments and simulations. The manuscript provides nice illustrations of how different experimentally determined pressure differences and local values of capillary pressure are. This is done by a blend of experiments and numerical simulations. While I have no problem with the basic message of the manuscript, the presentation is not as may be expected. Quite some space is reserved for the objectives, a literature overview in the background section, and the presentation of eqs. (1)-(19), which are basically definitions. What remains underexposed, though, is a clear identification of what is new. Certainly, averaging is not, and neither is it for two fluid systems in porous material. Therefore, I propose that this is explicitly mentioned on these sections 2-4.1, as I am not convinced that these sections should be maintained in this manuscript. The aspect of connectivity is given some emphasis (e.g. p.8) and reference is made to McClure et al. Again, I propose that it is clearly identified whether and what is new in this work, as the current text is not clarifying this. Later on, again the experimental and simulation parts appear to be based on work of McClure et al. and it is apparent that this work may duplicate that earlier work. Though the present manuscript is illustrative, I would consider it not fit for publication, if in essence the material is a duplication of earlier work.

AU: The introduction was written to put this work into a broader context associated with the special issue in honor of Professor Wood. Since this may well be the only manuscript on porous media in the issue, it seems some effort should be expended to make the connection with approaches to other hydrological problems. The objectives are brief, and we don't see anything to cut here. The formulation is included because the focus on individual regions within the porous medium is needed to clearly explain the issues involved with disconnected phases.

There are a number of important differences between the McClure et al. manuscript published in *Physical Review E* and this manuscript. Specifically, the most significant differences with the PRE paper are

1. **this manuscript includes data from drainage in addition to randomly initialized configurations allowing a comparison of a new approach with a traditional approach;**

2. **this manuscript considers a system where there is experimental support; and**

3. **this work focuses on wetting-phase connectivity rather than non-wetting phase connectivity.**

Reference to McClure et. al is necessary to provide additional theoretical details on the random phase initialization, but there is not really a significant topical overlap between the two papers. A sentence or two can be inserted to clearly assert what is the new contribution of the current effort.

One of the issues that is quite central to this manuscript is that equilibrium is achieved. Considering the small size of the apparatus, I wonder how this is checked.

AU: We are willing to add an experimental methods section to explain how these data were collected and how we became convinced that equilibrium conditions existed. We do not think this is necessary, though. We can add a statement that indicates how we used experiments in conjunction with the LB simulations to assure equilibrium. In short, we imaged the cell with a microsope and monitored the curvatures until no change was observed and compared experimental observations with LBM simulations, which showed virtually identical agreement.

On several other statements I also wonder what their justification is. Presumably, this is indicated in the cited references, but as a stand-alone manuscript, important statements need to be justified here.

AU: Not sure what statements this comment refers to.

specific comments: 1. I wonder about some of the English (is the term microfluidic well used;

AU: Yes this is standard terminology that is broadly used. A Google search on "microfluidic" provides over 4M hits (including both microfluidic and microfluidics)

abstract; these instances on p.4 line 11). The abstract contains quite some text, which I would rate as context, that is not necessary for an abstract and must be deleted: lines 1-8 or even 1-11.

AU: The abstract could indeed be shortened, but we wanted to ensure, given the special nature of this issue in honor of Eric Wood, that the abstract is sufficiently informative. We would do as the editor wished with regard to this point, but our preference for the reason cited is to leave this longer version pretty much as is.

In addition, the reduction of water content to below the irreducible saturation is mentioned: As the authors make a call for rigorous definitions, I think this contradiction in the text is inappropriate.

AU: We will make it clear in a revised version that the standard terminology is indeed a misnomer.

Of course, in a special issue focused on Eric Wood, there is a temptation to give some thoughts on his career. However, in this manuscript, those thoughts look quite artificial and unnatural. I would omit those parts of the text.

AU: The fact that this paper is intended to be part of a special issue in honor of Eric Wood was our motivation for a broad introduction that sought commonalities between Professor Wood's work and this particular, focused piece of work. Although one might argue that the contexts of upscaling in this work and in Prof. Wood's work are different, we assert that the upscaling techniques that can be used are the same. The fact that experimental and computational efforts to support the theoretical results in the two contexts will be different does not detract from the fact that upscaling, in any context, requires support. Again, we are willing to respond to the editor's recommendation on this point, but we prefer to leave this portion in tact in light of the nature of the special issue.

2. Averaging (p.4) is older. For instance De Josselin de Jong (around 1955)

AU: We do not know of another source in which the averages computed in this work, and necessary to make the points about the role of connectivity, have been formulated. This material should stay in our view and is necessary for understanding.

3. p.5 line 2: I would add: does not ONLY depend....

AU: OK.

4. I do understand neither the notation nor the meaning of (2) or the term "extent measure". Please clarify.

AU: We find this comment confusing. Previously, the reviewer opined that this was a standard formulation that wasn't new, and now she/he seems confused about fundamental components of modern averaging theory. We can add some additional references here to help readers who lack the appropriate background but are trying to understand the details of what is written. The indicator function is identified and described in Eqn (2) concisely and correctly. Similarly, the meaning of extent measures is defined right beneath their formulation in Eqn(3).

5. page 8: the term averaged phase pressures is used. I think that it is not appropriately, especially for this manuscript, to be vague about "over what is averaged".

AU: The purpose of including the formulation is to define precisely every quantity that is used. There is no ambiguity as every symbol is defined completely and in detail, with averages explicitly denoted in terms of their smaller scale precursors.

6. in Fig.1, the black circles represent solid phase particles. Are these in fact porous cylinders as I understand from p.9 line 15? I think this info should be made very explicit, to address whether or not this experiment is true 2D or in fact 3D (with additional complications that will be obvious).

AU: We could add a brief experimental methods section to make the experimental work clearer rather than just referring to other sources. The reviewer has identified correctly that fact that the black circles are the solid, but they are not porous. The porosity is the space between the cylinders.

One complication that may not be left undiscussed is that of boundary effects (at front and rear plates). In the same context, I do not understand p.11 line 4-5: why the "depth" (in Fig.1: vertical, horizontal,...) of the real apparatus and of the model differ.

AU: The two principal radii of curvature are R_1 and R_2. Since the depth of the micromodel sample is fixed, R_2 is fixed, and variations in the mean curvature are due solely to changes in R_1 . The depth of the simulation domain was increased

to improve the numerical accuracy, noting that this approach was sufficient to resolve the behavior of R_1.

7. Is the instrument new? I ask because it is not clear whether the experiments, their interpretation and such are new and in what sense (see p.10 line 17-18).

AU: The experimental work reported here is new.

8. you create random initial conditions below irreducible saturation (p.11). Only now, it is indicated clearly what makes it irreducible: because it is not connected to the wetting phase reservoir. I think that this needs to be mentioned earlier. Also, explain why it is relevant: these situations cannot develop in reality (for the experimental set up) as it is a state below irreducible. You mention (p.11) that below irreducible saturation, where sub-regions are unconnected, this leads to history dependence. I would think that the same is true in the random initial conditions simulations. Where you inject your "blocks", is simulating history.

AU: We agree that the use of the expression "irreducible saturation" is a bad historical misnomer. This name arose from the form of $p^c - s^{\overline{\overline{w}}}$ curves and the experimental methods used to obtain them. In fact, saturations below the "irreducible saturation" exist and can be achieved experimentally. Encouraging abandonment of this unfortunate, yet deeply ingrained, terminology is a huge task. The term, "history dependent" is employed, perhaps in a traditionally inappropriate way, to indicate that the microscale state of the system cannot be characterized by the macroscale variables p^c and $s^{\overline{\overline{w}}}$. If we consider the relationship $p^c(s^{\overline{\overline{w}}})$, we observe "history dependence," as it is traditionally explained, is a consequence of the fact that for a given s^w there are many possible microstates; and all of these do not produce the same value of p^c. These microstates can be achieved by operating an experimental apparatus under different scenarios changes of the boundary conditions. However, if we include interfacial area, ϵ^{wn}, in the theoretical construct, then the relationship $p^c(s^{\overline{\overline{w}}}, \epsilon^{wn})$ is able to characterize the possible microstates more effectively, independent of experimental operating strategies and histories, which does remove "history dependence."