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Discussions

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

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The paper critiques current models and makes a case for developing models that are consistent across scales based on thermodynamic principles. The nature of the processes these models tackle is kept vague, but some hints suggest that models for subsurface water flow (soil water and groundwater) are the prime target. A theoretical treatment of the Laplace Law is developed to develop equations for microscale capillary pressures, which seems to refer to pressure jumps across fluid-fluid interfaces in single pores. These expressions are more general that the Laplace Law because they apply to equilibrium and non-equilibrium cases. Expressions for average intrinsic phase pressures are also presented.

An experiment is described in which a non-wetting gas phase (nitrogen gas) permeates a 0.5 by 0.5 mm two-dimensional porous medium saturated by a wetting fluid phase (decane). This process and similar ones with different initial and boundary conditions

are also modeled numerically.

Both the simulated and observed data are used to obtain the 3D equivalent of the decane retention function in which the degree of saturation is a function of both the average fluid pressure and the specific interface area.

Major comments

For a paper on scales I could not help noticing that the time scale is mentioned only once and that there is no clear definition of the spatial scales of interest (microscale and macroscale). No connection is established between these scales and the scale of the representative elementary volume.

The paper uses a few straw man arguments. It is claimed that in experiments, pressures are only measured (or set) at the boundary of the system of interest. With the increased use of microtensiometers this is no longer necessarily the case. In my experience (and with some support in the literature), the microtensiometers tend to confirm that the known pressure at a boundary can be used to calculate the pressure anywhere in the system as long as contact is good and equilibrium has been achieved. The reliance on boundary pressures is not as risky as the authors appear to believe. In the terminology of the analysis of the paper this implies that phase continuity in real-world porous media is often sufficient for the observed pressures to be valid.

The authors state that average phase pressures are convenient to work with. I have never read anything in support of this argument. There are no sensors to measure average pressures, so we cannot calibrate models on them, and I have not come across any work that used average pressures in lieu of local pressures and pressure gradients.

I have the impression that the analysis is valid for zero-gravity conditions. This is never stated explicitly, but three elements of the paper suggest it:

- the casual averaging of pressures without acknowledging the immense effect of the geometry of real-world fluid bodies on the average pressure when gravity is non-zero

- the implicit notion that fluid interfaces and common curves have a non-zero thickness and therefore mass, without the effect of this mass being discussed or even mentioned.

- the extremely small size of the porous medium used in the experiment that indeed makes the effect of gravity negligible. In a paper in which the introduction discusses the importance of consistency of scales for scale ranges that are many orders of magnitude larger and already in the abstract calls for models that are based on rigorous multiscale principles this severely limits the relevance of the paper.

The lack or relevance is further reduced by the experimental scale: 0.25 square millimeter is in the sub-Darcian scale for most soils and geologic materials. To call this scale the macroscale seems to betray a fundamental lack of understanding of the concepts of the continuum approach and the representative elementary volume that form the basis that most currently used models are founded on.

Section 4 'Approach' has a non-informative title. It can easily be split in a 'Theory' section (modify the title as desired) and a 'Materials and Methods' section, thereby making the paper conform to the established structure of scientific papers. The Results and Discussion section is already there.

Section 4 starts with a treatment of the Laplace Law. One of the authors published an extensive treatment of this law (Hassanizadeh and Gray, 1993, not quoted in the paper). I would like to see included in this paper an explanation of the added value of the current discussion in view of this earlier work, and how this treatment relates to that in the earlier work. There are marked distinctions in notation between the earlier and the current paper which made it hard for me to establish the relation.

The work culminates in a relationship between capillary pressure, degree of saturation, and specific interfacial area. As long as the latter cannot be measured on 3D samples, the work has no chance of becoming applicable.

I do not see a path for using this kind of work to arrive at the thermodynamically consis-

tent, scalable models for porous media found in nature, even though the authors claim that goal to be a main motivation for the paper.

Overall assessment

The paper has six objectives that claim to resolve several issues relating to capillary pressure at the micro- and the macroscale and expose limitations of conventional approaches.

The Introduction and its list of objectives raise high expectations about the impact and relevance of this paper for modeling of multiphase flows in soils, aquifers, oil deposits, etc. These expectations are in no way met, either by the theoretical analysis that adds only incrementally to an earlier paper and omits gravity, or by the experiment on 0.25 square mm of an artificial, two-dimensional porous medium with two fluids that have no relevance for hydrology. To make the contrast between this work and real-world hydrology even more glaring, the authors drop the name of Eric Wood, who has worked on continental and global hydrology.

The presentation of the material is messy:

- the Introduction dwells on subjects not at all covered by the paper and fails to inform the reader about the paper's focus and nature of the work.

- the list of objectives is too long, and vastly overstates what the paper actually delivers.

- the paper is not well structured - there is no Materials and Methods section, and the flow of thought is sometimes hard to follow. Some parts are well written, others much less so. A strict adherence to the established format of a scientific paper would help.

- not all variables and symbols are explained, and there are inconsistencies in the notation

- the description of the experiment and the computations (what should be the Materials and Methods section) is incomplete.

Detailed comments are given in the file accompanying this review.

Reference: Hassanizadeh, S.M., and W.G. Gray, Thermodynamic basis of capillary pressure in porous media, Water Resour. Res. 29:3389-3405, 1993.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-451/hess-2016-451-RC1-supplement.pdf

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