

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

**Anonymous Referee #2**

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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

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## Interactive comment

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. 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. 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. specific comments: 1. I wonder about some of the English (is the term microfluidic well used; 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. 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. 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. 2. Averaging (p.4) is older. For instance De Josselin de Jong (around 1955) 3. p.5 line 2: I would add: does not ONLY depend... 4. I do understand neither the notation nor the meaning of (2) or the term 'extent measure'. Please clarify. 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'. 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). One complication that may not be left undiscussed is that of boundary effects (at front and rear plates). In the same context,

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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. 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). 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.

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