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Hydrology and Earth System Sciences Discussions

Interactive comment on "Big and small: menisci in soil pores affect water pressures, dynamics of groundwater levels, and catchment-scale average matric potentials" *by* G. H. de Rooij

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Reviewer 1 starts with noting that the manuscript is confused and attributes this to the mixing of scales. As I will elaborate below, the combination of scales, as I prefer to call it, is intentional. The reviewer's implied (and at times explicit) call to stick to one scale reflects a difference in approach between this manuscript and hers/his preferences that has significant ramifications. The first part of this initial reply reflects on these ramifications and should be considered a dialogue (for which the HESSD forum is also intended) rather than a reply to the comments. The discussion of the various comments

follows after this reflection.

Scientists in general (including myself) appear to be most comfortable when reasoning about a system which operates at a particular spatial and temporal scale. We can then construct an orderly universe of hypotheses, governing equations, and, when the scales permit it, experimental verifications (the reviewer mentions rigor repeatedly and is then referring to such an approach). But when we try to understand the effects of the processes and phenomena at one particular scale on a different scale, order tends to break down rapidly and our understanding diminishes. Unfortunately for subsurface hydrology, this is a time in which we are forced to bridge scales of space and time at an unprecedented level. Catchment hydrologists are increasingly pushing to understand how stream water quality responds to rainfall. Climate modelers desire infiltration and evapotranspiration fluxes at the scale exceeding those of some countries (I was once asked about the hydraulic conductivity of the Netherlands and recently was asked of I could come up with a global groundwater model). National and supranational legislative bodies want reliable assessments of proposed and implemented measures to reduce pollution of soils, groundwater, and surface water for their vast jurisdictions. If all we can come up with are rigorous theoretical constructs of pore-scale and Darcy-scale flow processes, and somewhat less rigorous but at least practically applicable numerical models based solely on this (sub)Darcian scale understanding, we fall short of expectations. If we retreat to these scales exclusively, we will certainly increase our understanding but in a way that leaves our science sterile in the sense that it hampers cross-fertilization with related disciplines in the physical sciences. Our sister disciplines will be forced to claim the remaining scales (and evidence of this is already appearing in the literature), for better or worse, and will develop their own version of subsurface hydrology. The reviewer report repeatedly calls for a 'need to decide about the scale' and as such favors a retreat to the scales at which we feel confident. The manuscript represents the opposite tendency that advocates an expansion from the scales where we are confident to the scales where we are relevant. It accepts that this move towards scale crossovers is not as clear-cut as single-scale work. Still, clarity is improving, as

the improvements to an earlier iteration in this scale transfer demonstrate. I note for the record that none of these improvements are challenged in this review – in fact, only a single one of them is mentioned, but not more.

The referee's disdain for the lack of rigor is in a way at the core of not only this paper but also of the upscaling problem. Rigor at superDarcian scales exacts a high toll. As I discussed before (de Rooij, 2009), upscaling Darcy's Law by volume averaging has been done with impressive mathematical and physical agility by Quintard and Whitaker, Gray and coworkers, and others (see de Rooij, 2009, for full references). The elaborate procedures to solve the closure problem and other complications required such assumptions as cyclic media, absence of gravity, and limitations on the degree of heterogeneity (including hierarchical scales at which such heterogeneity manifests itself). Other problems involved the emergence of parameters for which not only a suitable sensor does not exist, but for which not even a measurement principle can be conceived of upon which to base a sensor that could possibly be built. Rigor and applicability therefore tend to be mutually exclusive at larger scales. The referee takes the stand that rigor should prevail and therefore does not accept any large scale approaches (the large scale sections of the paper are scarcely treated in the review), the manuscript on the other hand champions the opposite approach of gradually moving forward to explore what can be done given the existing limitations and learn how to improve on that as we go. I share the reviewer's appreciation for rigor but also take account of the current limitations of our observations and theoretical capabilities vis-à-vis the problems of societal relevance that we are confronted with.

Reply to comments

The reviewer considers the manuscript confusing because of the various scales involved. As I elucidated above, this was intentional. I have the impression (not only from this reviewer report but also from more informal discussions with peers) that much of the confusion largely stems from investigating the effects of processes on one scale on phenomena on larger scales. Nevertheless I think we will need to make such efforts to

face the problems we are confronted with.

My phrasing seems to have caused the impression that I advocate looking at individual menisci to infer the energy status of the subsurface water in a field or catchment. Upon re-reading the text in this light, I see the reviewer's point. I chose the wording to establish a clear connection between Zehe et al. (2006) and the current paper. I will reword the text to remove this source of confusion. Some of the formulations used by the reviewer are very good and will be quite helpful in this respect.

The reviewer states that 'many of the definitions' are incomplete. In fact, I did not introduce that many definitions (if any) because the paper relies heavily on what is available. The various methods of averaging and the associated discussion of the issue of energy conservation in the averaging operation come closest to a set of formal definitions, but the section containing them seems to be the only section of the paper that the reviewer appreciates. In the introduction I made an effort to carefully state the assumptions required for the analysis and the reasons for invoking them, but it is unclear to me from the reviewer's comments what is inadequate there. As a case in point the reviewer claims I presented a definition of the matric potential, but I did not - I find the available definition quite adequate. I really only mentioned some of the forces involved. The reviewer possibly would have liked me to be more complete – the most complete definition I am aware of appears in the Handbook of Soil Science and I will be happy to adopt that in the revised text. The reviewer particularly stresses issues with water films and hydraulic continuity, which lead me to believe that Van der Waals forces (between the solid surfaces and the water molecules) and H-bonds (between the water molecules, and between the solid surfaces and the water molecules), and osmotic forces inasmuch as they attract water molecules into diffuse double layers are forces s/he would like to see mentioned.

Still, I doubt if an extensive discussion of the various subtleties involved in defining the matric potential contributes meaningfully to the paper – the subject is interesting but outside the scope of this paper. Furthermore, several treatments are available in the

literature, and I am not sure if another one is needed at this time. In the manuscript I implicitly relied on the widely used definition which compares the potential energy of water to that of water at a reference state. Because of the importance of the phreatic level I also repeatedly stress the distinction between the matric potential and the pressure potential. This is all in line with the mainstream literature.

The comment on Eq. (3) is valid. I was too fast in using a differential expression, since this limits the validity of the equation to a single pore or a system of identical pores arranged in parallel, and which have a geometry that prohibits wetting or drying fronts advancing through Haynes jumps. I will revert to a difference form of the equation and clarify the text accordingly.

I do not understand to which part of the text the next sentence in the review refers:'The comments/discussion of motion of interfaces . . . is not qualified nor [sic] substantiated.'. It may relate to the part of the Introduction that explains for which conditions Darcy's Law and the Laplace-Young Law are valid. I use qualitative criteria there because they are based on the analysis by Hassanizadeh and Gray (1990), who provide plausible arguments for the breakdown of these two laws for non-equilibrium conditions, but give no indication of their range of validity under mild non-equilibrium. And the nature of their analysis is such that it is hard to derive such criteria. Still, Hassanizadeh and Gray (1990) offer an analysis that seems to be both 'qualified' and 'substantial', although not quantitative. See also my reply to reviewer 3 on this matter.

The next paragraph is confusing, in part because the term 'partially wet slug of water' seems to suggest that water is not entirely wet. I do not understand to which section this comment pertains. After this single-sentence remark follows a rather abrupt statement about instability that adds to the confusion. As I state repeatedly, the analysis cannot handle infiltration because of the rapid processes involved. Yet, the reviewer brings up wetting front instability (and elsewhere mentions viscous fingering). The paragraph ends in a personal attack with the suggestion that I 'decided to overlook' such processes, thus ignoring my repeated and explicit explanations of the limitations

of the presented analysis. The focus of the reviewer on the pore scale steers the discussion to a single phenomenon, the instability of the wetting front, instead of its broader consequences. We can argue that considerable areas can be prone to wetting front instability, but its reliance on dry conditions (even if the coatings on the soil grains are hydrophobic) makes it unlikely that wetting front instability will ever affect entire catchments. In pore-scale work (notably percolation theory) much effort is dedicated towards viscous fingering (and the reviewer explicitly refers to that phenomenon), but the truth of the matter is that even a very low initial water content stabilizes the wetting front. The most effective proponents of fingered flow are soil hydrophobicity and air entrapment underneath the wetting front.

Contrary to the reviewer's innuendo, I considered field-scale effects of wetting front instability in an exploratory fashion many years ago (de Rooij and de Vries, 1999, J. Environ. Qual.) and concluded that at that scale the acceleration of solute leaching to the groundwater was masked by the much larger effect of soil heterogeneity, for a considerable part because of the damping effect on the enhanced leaching of the diverging flow near the capillary fringe. This diverging flow was entirely ignored by the reviewer, who focused exlusively on the onset of instability at the pore scale as studied in percolation theory in set-ups that are smaller than the representative elementary volume, without accounting for the full flow pattern of converging, vertical, and diverging flow that develops in an unsaturated zone with an unstable wetting front. In fact, the instabilities observed in percolation experiments represent proto-fingers of which only a few will mature to much more widely spaced full preferential flow paths observed in natural soils. It is this full-scale preferential flow pattern that should be studied to clarify its significance in the field, not the onset of instabilities at the start of infiltration in a porous medium with a uniform, very low, initial water content. Preferential flow paths remain active for entire seasons and may even reappear in the same location over many years, as evidenced by the tongues of leached organic matter in podzols among other observable soil features. For such persistent structures that operate at scales of decimeters to meters, the onset of instability during the first minutes to hours of

infiltration in a pristine medium a few centimeters across is of limited relevance. I think preferential flow can be a dominant factor on the field and the catchment scale in a very different way and at much larger time scales by influencing vegetation patterns through the associated increased removal of water from the root zone by deep infiltration. This, however interesting, is speculative at this time and outside the scope of the paper.

In short, the reviewer made a clear choice for the small scale, which allows a confident and rigorous treatment. The price that was paid for this rigor was that it is focused on a problem that really is not all that important outside the laboratory.

More generally, the pore-scale works the reviewer alludes to in general report either on numerical experiments on pore-throat models, or on lab experiments with highly idealized porous media under conditions controlled to the point that the disconnect with field situations is nearly total. One can validly argue such an approach is needed to make hypotheses testable, keep the processes observable, and the mathematics tractable, i.e., one sacrifices the accuracy with which the natural conditions are represented in the lab or in the model for the sake of rigor. Still, I argue this approach is not as rigorous or sound as it appears from the surface. It amounts to adjusting the nature of the problems we study to our abilities, not to our needs. A very undesirable side effect is that the results obtained can hardly be put to work to solve actual problems. There is a real risk is that one ends up studying phenomena that are interesting as such but not all that relevant in the field.

This risk is illustrated by the example of viscous flow in soils, brought forward by the reviewer and to which I already alluded above. In the 1980s and 1990s, the relevance of wetting front instability was claimed to lie in the enhanced solute leaching it produced, in part illustrated by both esthetically pleasing and impressive imagery from experiments in well defined porous media not unlike those used in the papers referred to by the reviewer. Under field conditions, wetting fronts mostly become unstable in hydrophobic soils that are sufficiently dry. And even there, the soil is only water repellent to a certain depth. The flow below that depth diverges, which alleviates the

negative effect on solute leaching, as some of the later papers (some of them mine) in the voluminous body of literature on the subject demonstrate. At the end of the day, research abated without having resulted in policy measures to protect groundwater in areas with unstable wetting front or in including information about unstable wetting front in protocols for soil mapping or monitoring programs. The viscous fingering mentioned by the reviewer is different from wetting front instability owing to water repellency, and quite rare: I am not aware of any paper pinpointing viscous fingering as the cause of instability of an observed wetting front in a field soil. Therefore, rather than choosing to overlook this, as the reviewer states, I did not see the need to include viscous fingering as it is of little relevance in soil hydrology outside the realm of small laboratory setups – its main relevance probably is in oil extraction. In focusing on viscous fingering, the reviewer in effect made a scale leap from decimeter-scale laboratory settings to catchments while only considering the small scale processes and not the large scale consequences. I am doubtful about the potential for success of this methodology and would advocate a simultaneous look at both scales. The results may then be less glamorous (as this current work and that of Zehe el. (2006) show), but it lies bare the required assumptions, allows errors to be corrected and refinements to be argued (as some of the reviewer's other comments stimulate me to do), and showcases our still severely limited ability to make useful contributions at the largest scales.

Detailed comments

p. 6492 l. 14. The reviewer is correct. I was struggling with the proper formulation and I find that of the reviewer more adequate. I will modify the text accordingly.

p. 6492 l. 24. Interesting comment. I am inclined to adopt the suggestion.

p. 6495 l. 12. The matric potential is defined as it normally is, with respect to a reference level (I will include a reference to 'Soil Physics' by Jury et al., 1990, where this definition is discussed in some detail). In that definition, the matric potential is zero for water at atmospheric pressure (at the phreatic level), and less than zero for water in

the capillary fringe and in the unsaturated zone. Thus, 'negative' simply means 'smaller than zero'. Given the fact that the reviewer mentions the atmospheric pressure as the reference level in detailed comment p.6500 l.13, I am not sure where the unclarity originates from.

p. 6497 l.8 This equation will be converted to its difference form from its differential form (see above).

p. 6497 l. 15. I don't follow. This part of the text discusses ways to modify pressures in hypothetical vessels with a single non-capillary (large) or capillary (small) opening to the atmosphere. I read some of the papers by the mentioned authors but did not see how they related to this part of the text.

p. 6498 l. 10. The reviewer states that the drainage process may involve avalanches of emptying pores and refers to Aker et al., 2000, Europhys. Lett. 51:55-61.

There are a few issues with this reference. For the situation in Table 1 (wettable soil, 10 mm hr-1 rainfall, 15 degrees Centigrade, porosity of 0.40 or 0.60), the viscosity ratio M (nonwetting over wetting) equals 0.0159 and the capillary number is 1.08âĂć10-7 (porosity = 0.40) or 7.18âĂć10-8 (porosity = 0.60), indicating that capillary forces dominate over viscous forces. These capillary numbers are well outside the range of Aker et al. (2000), and many of the details of the drainage process were explored by Aker et al. (2000) for viscosity-matched fluids (M = 1). Given the gap between the simulated conditions in Aker et al. (2000) and field conditions, interpreting Aker et al.'s (2000) findings for realistic field conditions is not straightforward. Aker et al. (2000) also report an air viscosity that is about 16 times too large. This warrants care in interpreting their findings.

In view of the reviewer's repeated plea for rigor it is worth noticing that Aker et al., for nodal networks in only 2 dimensions with cylindrical connecting pores of only several hundreds of nodes (i.e., at best barely sufficient to reach the scale of a representative elementary volume) were already forced to sacrifice rigor: for flow rate calculations,

their tubes connecting the nodes are assumed cylindrical, but at the same time the pressure jump across a meniscus in such a tube is allowed to vary smoothly between zero an twice the value corresponding to the pore radius depending on the location of the meniscus inside the tube, reflecting an hour-glass shaped tube, as the authors phrase it. It leaves me with the feeling that double standards are being applied by the reviewer.

But if I focus on the substance of this comment I do not see how these small scale phenomena are at odds with my statement: the emptying of several pores instead of a single one still constitutes a small volume of water. I believe the reviewer considers a 'possibly very much smaller volume of liquid' a volume that is smaller that that of a single pore, but I purposefully used the relative clause 'very much smaller' to compare the volume of liquid added or removed to the total volume of liquid that experiences changes in pressure potential (which is the remaining liquid in the case of water removal). Thus, losing the equivalent of several cm of water (by evapotranspiration for instance) in a field or a much larger area affects the potential energy of all remaining water, which generally will be a much larger volume. Whether or not the air-water interface moved smoothly or by rapid emptying of clusters of pores is of limited importance at these much larger scales. I will clarify the text on this.

p.6498 l.28. The reviewer asks me if I can 'invent' a case that illustrates rapid drops in potential with small volumes of water removed. The answer is yes, although I would not call it an invention. Aquifers that are confined for most of their extent but have a phreatic intake area behave like that: if the intake area is small and the confined portion of the aquifer has a large horizontal cross-section, the response in the hydraulic head to pumping water from the aquifer is much stronger than in the opposite case. I will see if I can include this in the revision (with a figure), but must weigh this against the desire by the other reviewers to shorten the paper.

p.6500, l.13. The reviewer claims that particularly for the catchment scale, the atmospheric pressure can be assumed constant. I disagree. At this scale one can observe,

for instance, the effect of barometric pressure variations caused by the passage of areas of high and and low pressure due to atmospheric circulation. These variations and the delayed response they generate in aquifers is even used to flush out volatile contaminants by opening sealed wells at strategic times (the vapor then simply blows out). But even if it would be constant, my statement remains true: the value of the pressure exerted by the atmosphere on the liquid-gas interface is set by the atmosphere – had the Earth's atmosphere contained only half of the amount of gas, its pressure at sea level or another reference level would be correspondingly lower. Thus, I do not see a real disagreement between my statement and the reviewer's comment, but I do not subscribe to the point of view that the atmospheric pressure should be considered invariable for all cases.

p. 6501, l. 18. I meant here the vey slight difference in hydrostatic potential within the body of water of the pendular ring itself. I agree with the reviewer that the hydraulic continuity needed for the water in the ring to be subjected the hydrostatic force of water elsewhere in the pore space is not there. I will clarify the text.

p. 6507, l. 25. This text deals with a thin layer of water that wets the top few centimeters of a dry, wettable soil and then does not proceed any further. Thus, the water is stationary and the analysis can rely on hydrostatic equilibrium. The force balance invoked by the reviewer then automatically implies that the pressure in the water near the wetting front (the bottom of the wetted layer) must be higher than that near the soil surface, according to the pressure profile in a column of water. Consequently, for soil air at atmospheric pressure, the pressure jump across the gas-liquid interface at the wetting front is smaller than that across the interface near the soil surface. I don't see how this violates the force balance, let alone mass conservation.

p. 6507, l. 28. At the top of the paragraph I stated that the analysis holds for a well-sorted soil, which therefore is supposed to have a fairly uniform pore structure. I will improve the text. (Note incidentally, that the analysis would also hold for a bundle of tubes.)

p. 6510, l. 7. The reviewer seems to be unfamiliar with the term 'unit gradient flow' or perhaps overlooked it. I am not subjecting a stagnant body of water to a gravity field, but instead allow a constant flux of water to flow through the soil with gravity as the sole driving force. Thus, the vertical gradient of the matric potential is zero, and for uniform soils the water content and the hydraulic conductivity are constant with depth. The flux density is then equal to the hydraulic conductivity at that particular water content. Such conditions prevail for instance lower in the profile in soils where the groundwater is deep and there is a net rainfall surplus. The flux density then reflects the precipitation surplus averaged over the year. It is also a condition that is frequently imposed in the laboratory to measure unsaturated hydraulic conductivities and volumetric water contents as a function of matric potentials. The absence of vertical gradients in the three variables is obviously of great advantage.

p.6517. The reviewer asks if the analysis in the appendix would not give completely different results if the vertical capillaries were connected and could exchange water. This can be tentatively assessed by Fig. 1. Only when a horizontal throat connects a filled capillary to an empty one does it create an extra interface. The right-hand side figure represents a throat that is wider than one of the capillaries it connects, which would create an interface that is less curved than it could have been in that capillary. I find it hard to imagine this situation occurring in much more complex natural pore networks. The extra interface in the connecting throat in the left hand side of the figure would not occur in a bundle of isolated capillaries. Still, the population of throats in the natural pore space would hold water at low matric potentials and thus be visible in the water retention curve. Since the radii of the bundles are chosen such that they collectively represent the entire pore space, the interfaces present in these narrow throats are represented by tubes with small radii in the bundle model.

In short: I doubt if the capillary bundle representation would change very dramatically if the vertical capillaries were connected by throats, since only those throats that happen to connect two capillaries at an elevation where precisely one of them is water-filled

would add an interface. But I am not sure if the bundle model accurately represents the total number of interfaces in a soil, even if it does accurately reproduce the soil water characteristic.

But the point of the appendix was to test Zehe et al.'s (2006) hypothesis. The results indicate it does not hold for this special case. This strongly indicates it is unlikely to hold for a more general case, but verifying this by simulating the distribution of interfaces in a realistic pore space is not practically feasible. The pore-throat networks often used in percolation theory (including some of the papers mentioned by the reviewer) offer little help: they too are simplifications of the natural pore space and merely represent another special case.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 6491, 2010.

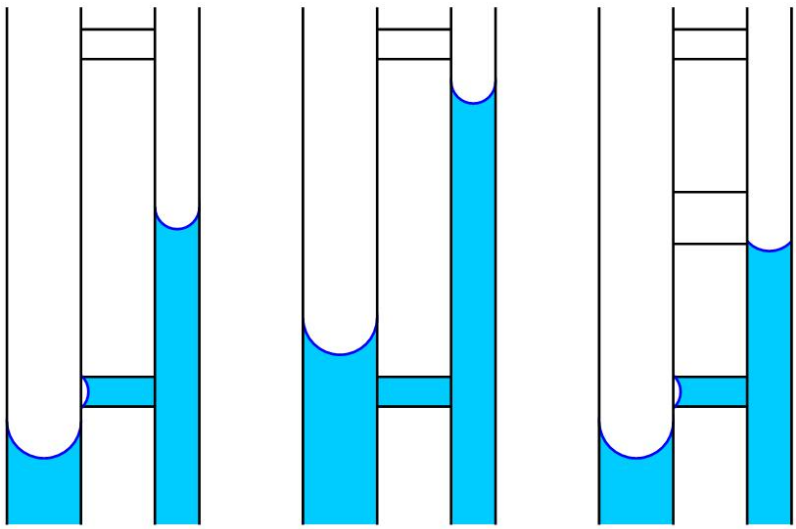

Fig. 1. Figure 1. Three tube systems consisting of cylindrical capillaries and throats. The middel throat in the RHS sketch has a radius between those of the vertical caillaries.