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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 3 offers some general editorial points without specific detail that I will take into consideration when improving the revised text. These comments do not need a reply in the discussion phase.

I do not see the contradiction between the statement that pore scale processes cannot be tracked beyond the Darcy scale and the importance of pore scale processes at larger scales. I cannot think of a sensor that would allow me to measure the interfacial area, its rate of change, and the principal radii of a large population of interfaces, and

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their rates of change. This is the kind of information required by the system of equations presented in Hassanizadeh and Gray (1990). Still, the fact that I am unable to observe a set of phenomena does not mean they will have no effect. As an example I present the increase in the matric potential in a fine-textured soil with its liquid-gas interfaces near the soil surface. The change of interfacial curvature at the onset of ponding creates a change in the phreatic level that is very real, and can readily be observed in the field. Reviewer 3 does 'not very much like the general idea that processes on a small scale are still dominant at a much larger scale' but sometimes this simply is the case and we have to deal with it.

Reviewer 3 states that the averaging of potential energies was already discussed by me in 2009. S/he is correct, but the material here adds to the equations of de Rooij (2009) to make the set of averaging equations complete – there is no overlap. Since this seems to be the only part of the paper that reviewer 1 likes I would like to keep it in: there apparently is an interest among the portion of the readership represented by reviewer 1, and the material has not been previously published.

Reviewer 3 suggests that the limitation I impose (slow flow) translates into near-zero gradients. This is only correct if the conductivity is high, i.e. for coarse-grained, nearly saturated porous media. The slow-flow limitation is based on the requirement that moving and deforming interfaces do not significantly contribute to changes in potential energy; according to Hassanizadeh and Gray (1990) that would make the conventional forms of Darcy's Law and Laplace-Young's Law invalid (as mentioned on p. 6494, l. 10-14). High gradients in poorly conducting soils (for instance in wet and dry clay soils or dry sands) are no problem. But wetting fronts (where the flow is truly Darcian only some distance behind the front) are, which complicates matters during rainfall on dry soils.

The slow-flow requirement is not limited to this paper – I only made it explicit. In this sense, reviewer 3 is too harsh: the critique levelled against this paper applies to all work relying on Darcy's Law for unsaturated flows. The point I wanted to make, and

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which I clearly need to make in a more crisp way, is that anybody who works with the conventional form of Darcy's Law at any scale (and that's most of us) implicitly subjects hers/his analysis and results to this requirement that the flow is slow enough to make the effect of the changes in the configuration of the interfaces negligible. Unfortunately, Hassanizadeh and Gray (1990) did not elaborate what the maximum allowable flow rates are under realistic circumstances before the conventional laws lose their validity, and in that sense the comment that the paper is mainly of academic interest rings true – I cannot give a quantitative measure. The fact that our practical models for water flow and solute movement in soils often give satisfactory results negates an overly pessimistic view on the validity of the Darcian approach, but the fact remains that infiltration into dry soils, with its strong infusion of interfacial processes, is notoriously difficult to model, even at the plot scale.

At larger scales it is worth noting that the satisfactory modeling results are largely limited to the field scale and for bulk solutes (salts, nutrients). At the catchment scale we are all but powerless but there is a very real demand to come up with accurate predictions at that scale too. This paper, Zehe et al. (2006), de Rooij (2009), and Roth (2008) all explore if and how concepts developed for the Darcian scale can be used at larger scales. The 2009 paper found that one cannot simply expect Darcy's law to hold for large volumes: the proportionality between the upscaled flux density and hydraulic potential gradient breaks down. Roth (2008) essentially found that it was fundamentally impossible to upscale processes with short characteristic time scales (such as infiltration), and here I find that the upscaling from the pore scale to the catchment scale holds promise only under rather restrictive conditions. Reviewer 3's desire to read about a Darcian approach that works at large scales can therefore not be fulfilled, I think: the overall conclusion from the set of papers mentioned above seems to be that we better look elsewhere for more viable alternatives. It would be great if I could already offer such an alternative, but I am sorry to say I have not found one yet. Still, in any debate about future directions, it is necessary and useful to point out the limitations of any approach, if only to help others avoid pursuing dead leads. The Darcian approach,

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which is the most widely used modeling approach at the plot and the field scale, was examined for its use at larger scales by de Rooij (2009) and Roth (2008), and found to be inadequate. In this paper I followed up on Zehe et al. (2006) to see if a connection to interface curvatures could at least improve the determination of the potential energy of large volumes of water. In the process, I corrected Zehe et al.'s (2006) hypothesis on the relation between average curvature and average matric potential, and provided conditions for such a relationship to exist. I will try to clarify this overall message, and strengthen the link between the cluster of papers mentioned above.

The point both Zehe et al. (2006) and I make, and which seems to be implicitly contested by reviewer 3, is that accepting the validity the Laplace-Young Law and Darcy's Law establishes a firm local connection between interface curvature and potential energy, but also a connection between the direction and the rate of flow, and the change of interface curvature in space. I was stimulated by Zehe et al. (2006) to see if these connections resulted in a relationship between the average rate of curvature and the average potential energy, and found a different relationship that that hypothesized by Zehe et al. (2006). Since the paper with that hypothesis has remained unchallenged for nearly five years I added in the appendix a detailed account for the most complicated case that still allowed analytical treatment. Reviewers 2 and 3 are correct in reading it as a comment to Zehe et al. (2006), but I need to think about their suggestion to publish it as a separate comment – doing so would perhaps suggest, even more strongly than I apparently have done unintentionally here, to go out and measure interface curvatures to estimate the potential energy of large bodies of subsurface water.

General response to all three reviews:

The paper received three reviews, with three widely varying opinions. The most critical review focused on the pore-scale processes and heavily relied on percolation theory and the physics of interfaces. In general, not all transfers between the pore scale and the very large scale were convincing, and reviewer 1 brought up a few criticisms that are valid, and others that are only valid if the target scale remains small – at or below the REV-scale (see my reply to reviewer 1). Generally, the explanations in the paper need to be improved. In doing so I will also try to deal with some of the scale transfers of the REV-scale to larger scales in the hope of taking away some of the concerns of reviewer 1. The valid points will of course lead to a modified content – I am working on that but cannot present anything firm yet.

Two reviewers have the impression I advocate pore scale analysis to analyze the energy status of water volumes at the catchment scale. This was unintentional and results form my struggle finding the right formulations while simultaneously trying to keep the link to Zehe et al.'s work (2006). Particularly reviewer 1 offers useful formulations that will help to clarify a revised text – the link with Zehe et al's (2006) work needs to be loosened then, but the clarity of the text is clearly more important.

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