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Discussions

Interactive comment on "Modeling macropore seepage fluxes from soil water content time series by inversion of a dual permeability model" *by* Nicolas Dalla Valle et al.

Anonymous Referee #1

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The idea presented in this paper is good, but the execution is seriously, possibly fatally, flawed. The rationale for a simple, parsimonious model that can be calibrated using only data that can be obtained with relative ease is convincingly presented. The combination of a simplified macropore model with a matric flow model based on retention and diffusivity curves with few parameters is worth exploring.

But then the difficulties start:

An experiment was carried out in which a soil with low clay content was sieved, homogenized and mixed with fine gravel. If one wished to avoid the development of a macropore network, this was the way to go. Grass was sown, which was well watered.

Late in the paper it is stated that the roots were expected to create macropores, but they occupied these themselves. Dye tracer tests to verify after the experiment if there was preferential flow were not performed. The soil was kept moist so the chances of root shrinkage (opening up an air gap between the root and its macropore wall) were small. The shallow depth of the soil (20 cm) and the lower boundary with only a single outlet that presumably only permitted water to flow out at atmospheric pressure also ensured wet conditions throughout. Thus, the experiment allowed only a limited range of water content to be examined under the absence of macropores.

The macropore model has its own problems:

The expression for the flow velocity in the macropores is a factor 3 off. I presented an alternative derivation near Eq. (2). Eq. (4) is incorrect as well: the flux term in the mass balance is formulated incorrectly. I presented an improved version. The resulting version of the flow equation (the second part of Eq. (4)) is the same though. It appears that the error in the flow velocity and the mass balance term canceled out, perhaps that is why they went unnoticed.

The soil physics is also questionable.

The conductivity at the top soil is falsely interpreted as the maximum infiltration rate (section 2.4). It is better to state that the maximum infiltration rate is determined by the hydraulic gradient and the hydraulic conductivity at the tip of the wetting front. That is why the gradients in the wetted part of the soil during infiltration are often not very large. The wetted soil is so conductive compared to the dry soil at the tip of the wetting front that hardly any gradient is needed to transport the infiltrated water to the tip of the wetting front. Therefore, the conductivity at the soil surface is only important during the initial stage of wetting.

This is illustrated by the fact that that the infiltration capacity becomes lower as the wetting front moves down. At the start, the top fraction of a mm of the soil is saturated, and the gradient there can be in the order of 10000. As infiltration proceeds, the front becomes less sharp, and the flow through the wetted part of the soil causes a (relatively minor) drop in the matric potential as the water travels towards the wetting front. Both phenomena (the former often much more than the latter) lead to a reduction in the hydraulic gradient at the wetting front. With the soil at the tip of the wetting front being equally dry, this leads to a diminished infiltration capacity. When the wetting front arrives in a wetter part of the soil, the local conductivity increases a lot, and the infiltration capacity might well increase then.

All in all, approaching matric infiltration based only on the conductivity at the soil surface, and assuming that is somehow a maximum flow rate is not very good. There are several infiltration equations (by Green-Ampt, Philip, and others) that should work better, especially in the early stages (e.g., Hillel, 1998, pp. 391-391).

In section 4.1, unit gradient conditions are confused with hydrostatic equilibrium. The authors claim that d theta / dz = 0 indicates hydrostatic equilibrium. But this is the expression for unit gradient flow, not that for hydrostatic equilibrium.

Unit gradient flow: flow driven by gravity only; d psi/dz = 0, and therefore d theta/dz = 0.

Hydrostatic equilibrium: hydraulic gradient = 0: dH/dz = 0. This implies that the gradient in the matric potential cancels out the gradient in the gravitational potential. For z positive downward, as it is defined here: d psi/dz = 1. Therefore, d theta /dz <> 0.

The soil moisture profile can be derived from the soil water retention curve if the plane where psi = 0 (phreatic level) is known. The authors apply this technique later in the paper.

The casual statement in section 4 that ET had no effect even though a 20 cm closed container apparently was not at hydrostatic equilibrium (contrary to what the authors believe) also betrays a lack of understanding. Root water uptake tends to make soil moisture profiles in non-layered soils more uniform because the roots take up more

water where it is easier to get.

The explanation of Eq. (14) is simply false (and its sign is wrong). The expression for the matric potential follows directly from the definition of hydrostatic equilibrium (zero hydraulic gradient). Applying Eq. (8) for steady-state conditions is less specific (constant but non-zero flows are also steady state), and taking the derivative of the equation leads to additional terms that were conveniently ignored. In addition, the sign of the equation is only correct if z is defined positive upward, which contradicts Table 1.

In Equation 15, the sign is wrong (a consequence from the error in Eq. 14). Apparently it escaped the attention of the authors that they calculate positive matric potentials in unsaturated soils with this equation.

The exponential retention curves in Table 4 behave non-physically. The exponential terms in them go to 1 as theta goes to zero and to some small but finite value as theta goes to theta-s. If the exponent is made proportional to (theta-s minus theta) instead of theta they behave as they should.

In section 4, the Boltzmann transformation is applied (Eq. 19). This approach is only useful when capillarity dominates gravity (thereby ensuring the validity of Eq. 18, where the gravity term is omitted) and conditions far away from the infiltration front have no effect because the transformed equation is solved for a semi-infinite column with a prescribed water content at infinity.

The authors waited for 12 hours after rainfall and had only 20 cm long columns. The long wait diminished the matric potential gradients, thus ensuring that the gravitational gradient was no longer negligible. Also, the infiltration front had penetrated most if not all of the column by then, invalidating the semi-infinite approximation. The resulting values of the diffusivity are therefore meaningless. The fitted diffusivity parameters had an effect on the other parameters when in the last leg of the optimization process, all parameters were fitted simultaneously, but it is impossible to determine how detrimental they were. In any case, they cast doubt upon all fitted parameter values and the model runs based on these values.

Given that the paper suffers from both experimental and theoretical flaws I doubt if a revision can save it.

Hillel, D. 1998. Environmental soil physics. Academic Press, San Diego, U.S.A.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2017-336/hess-2017-336-RC1-supplement.pdf

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