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Hydrology and Earth System Sciences Discussions

Interactive comment on "Iterative approach to modeling subsurface stormflow based on nonlinear, hillslope-scale physics" *by* J. H. Spaaks et al.

Anonymous Referee #1

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This paper deals with the issue of how much complexity is needed to model runoff generation at the hillslope scale. The authors test different model formulations to simulate one major event at the well-known Panola hillslope site. The perceptual models being tested (Figure 3) present an interesting path from a simple representation towards a spatially varying and also dynamic representation of the hillslope. Having said this, I also have to say that I have major concerns with the paper as outlined below and therefore can not recommend publication of this paper as it is now.

1) The authors use a Darcy-law approach to compute lateral flow (Eq. 3), doesn't this

contradict to previous findings (including papers by McDonnell!), which highlighted the importance for macro-pore flow at this hillslope?

2) Even if we assume that flow can be simulated using Darcy law, I find it surprising that Ksat is assumed to be constant with depth. Many papers have shown that Ksat often decreases with depth, and I am a bit surprised that the authors do not test this. Looking at the hydrograph, I would guess that a different formulation for the Ksat-profile would have improved model 1 considerable. By not testing this, the question remains whether the more complex models are needed because of spatial heterogeneity (as discussed by the authors) or a wrong assumption on Ksat. (a similar argument can be made for the choice of g(t) in model 2))

3) I am not sure I understand the physical reasoning for the travel times in model 2. As I understand the authors, the motivation for g(t) is the delay of the water entering the soil storage (i.e., vertical flow). I would argue that for shallow gw-systems, such as in this study, delays are rather caused by lateral flow than by vertical flows.

4) Why do the authors limit themselves to simulating only one event? This is a severe limitation and limits the value of the results considerable. With only one event I don't think we can draw any general conclusions. Previous work at the Panola site present an impressive data set of many events (e.g. Freer or Tromp-van Meerveld), and I can't see any valid argument why the analysis presented in the present paper should be restricted to using only one event!

5) I must admit, I am a bit confused about the model setup. At first I thought that the simulations were done using a grid (e.g., P5213, 14: drainage direction in steepest gradient direction), but then I missed any information on, for instance, grid resolution or numerical schemes. Later I also got the impression that the first three models were lumped, since the authors describe the difference of model 4 as the hillslope being represented by 8 elements. Clarification is needed!

6) Basic information about the model application seems to lack. What is the time step

HESSD

6, C1967–C1969, 2009

for the simulations? How have initial conditions been specified?

Minor comments:

I would recommend not using terms like field capacity, which have a clear meaning in soil hydrology but are used in a different way in this study. The authors use, for instance, the term field capacity for a moisture content at which no further drainage occurs, but their value is independent of depth and height above groundwater level. In reality, the water content for the no-drainage condition obviously varies with depth and is related to the position of the groundwater table. Please note: my point is not that the approach used by the authors is wrong (we often have to use 'wrong' formulations as useful approximations) but that the terminology is misleading.

The structure of the paper with its jumping between methods, results and discussions is a bit awkward.

I think we should be a bit more careful with the term 'virtual experiment'. There are certainly cases where a modelling study can be called virtual experiment for good reasons, here I don't see why we need this fancy term. Different model structures are tested, but I don't see any 'virtual experiments'.

P 5218, 23-24: an efficiency of 0.85 is rather high, BUT of course here this value is computed for only one event. One might question whether the efficiency which is usually used for continuous simulations is appropriate for evaluating one event.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 5205, 2009.