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Hydrology and Earth System Sciences Discussions



# *Interactive comment on* "Infiltration-soil moisture redistribution under natural conditions: experimental evidence as a guideline for realizing simulation models" *by* R. Morbidelli et al.

**Anonymous Referee #1**

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The paper makes a case for developing two-layered soil models with a thin upper crust that is formed in silty loam soils under rainfall events that exceed a modest intensity threshold. The crust also breaks up with freeze-thaw cycles. The authors contend that models seeking a realistic description of water distribution in such soils need to be cognizant of this crust formation and breakup. The authors support their arguments with numerous experiments involving soil moisture measurements (5-25cm deep into the soil) coupled with rainfall and weather-related experiments. Numerical results of approximate models for two-layered soils are also used to supplement the analysis and lend strength to the conclusions.



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Interactive Discussion

Discussion Paper

I feel that the authors have a strong and important message here regarding important hydrologic implications with crust formulation-at least they bring the problem to light and demonstrate its importance, as simulations under homogeneous soils fail to explain observations. I have the following questions that the authors could address to further polish their paper:

1. The authors assume an upper crust thickness of 5 cm based on some previous studies. My experience is that the crust is usually thinner – but that would be a function of soil type and rainfall history. What are the implications of this assumption of 5-cm crust thickness? Would it change the estimation of K1s for each event? A brief discussion would help put this aspect in proper perspective.

2. How was the initial pressure head (or initial soil water content) estimated for each event as listed in Table 2. Was this value calibrated for each event separately?

3. I agree with the authors that ET would perhaps not play a significant role for the short duration ($\sim$ 48 hrs) for each simulation, and would be more significant for continuous simulations. However, with the shallowest measurement made at the 5-cm depth, I am not sure if the significance of ET would be significantly captured from experiments.

4. The authors seem to imply that K1s should vary continuously with time (see page 6202, line 9, for e.g.). However, the value of K1s is kept constant during each event, but allowed to vary from event to event. This needs to be clarified in the abstract and introduction.

5. The authors leave us with the conclusion that K1s needs to be modified with time to represent crust formation. However, they do not offer a method for accomplishing this. Does it mean, that at present, we must calibrate for K1s for event-based models thus offering no predictive ability?

Overall, I think this is a good paper as it raises some fundamentally important questions and identifies future areas of research. The writing of the paper can be improved to



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Interactive Comment

make the message sharper, but perhaps that could be accomplished at the editorial level.

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