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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Behavior analysis by model slope experiment of artificial rainfall" *by* M. C. Park

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The paper presents a laboratory experiment and a seepage numerical model of a shallow failure obtained under controlled condition in the lab. I appreciate the paper, and I believe that the research is worth to be published on NHESS. Moreover, the paper is well written, with good English and correct terminology. And the quality of the presentation is high. However, I believe that the paper is still not rich enough to be accepted in the present form. The main problem is that a single experiment is not enough, also considering the fact that the experiment is cheap and can be repeated many times. With a single experiment, in fact, the research is affected by potential errors and singularities that other experiments could correct. On the other side, interesting behavior are not confirmed by other experiments and should be taken with a big caution. Hence, in my point of view, the research can be accepted for publication only if the author add

several other experiments. The fact that the experiment is single makes also difficult to evaluate the results and, lastly, to make a review. For instance, the anomalous behavior of tensiometer B in the experiment is due to a pipe? Why this pipe appear? Maybe a problem in the compaction of the material? Other experiments will allow to understand what is ultimately generalizable (that's the interest of scientist) and what not. Another issue that is critical is the fact that the soil never reach saturation, although the conductivity is relatively low (10-4). In fact, the seepage model reach saturation. Why this saturation does not happen? Is it due to compaction, preferential flow, cracks, pipes? Again, a single experiment does not say anything that is actually certain. According to the experimental results, I suspect that the conceptual model (homogeneous unsaturated flow) based on which the stability analysis is performed, is not correct, and it is clearly violated by the experiment. The experiment shows some heterogeneous behavior, which is not accounted for in the numerical model. Hence, also the numerical model needs to be improved to be more consistent with the actual behavior. Of course (again), this could be done only observing other experiments, when finally it will be possible to generalize the experimental behavior. The last point regards the interpretation of the soil parameters. The authors assign an air-entry value of 0.452, which is quite suspect. As far as I know, the air-entry value should be the point where air enters in the pore, i.e., the point where the curve deviates from the saturation. In the fitting of the data, this point is very close to the origin, which means that the material practically does not have any capillarity fringe (that is normal, indeed). Therefore, the air entry value should be low, almost 0, in my opinion. In addition, the residual water content to 0 is impossible.

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