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Discussions



# *Interactive comment on* "A model of landslide triggering by transient pressure waves" *by* G. W. Waswa and S. A. Lorentz

**Anonymous Referee #1**

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The paper describes the development of an analytical model to predict the propagation of rainfall-triggered pressure waves in a saturated porous medium (soil). The manuscript is concise, well written, and properly illustrated. Moreover, objective and methods are clearly stated and the topic of the research is of relevant scientific interest.

However, I do not agree with the basic premise of the paper. The authors justify their work saying that the Richards' equation is not suitable to describe the propagation of pore pressure into a saturated soil, so they develop a new model specifically devoted to this purpose. This idea is repeated many times throughout the text, for instance:

p.6 "It should be emphasized that the Richards' equation was developed for the movement of water in the unsaturated soils, where a change in pressure potential can be



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expressed as a function of change in soil water content, a relationship that is defined by C( ). This implies that it may not be possible to define C( ), where the water content is constant, e.g., in a saturated zone."

p.9 "Iverson used the hydraulic diffusivity parameter to describe the transmission of pressure head based on the assumption that transmission of pressure head (energy) is synonymous to the transmission of water (mass)."

p.19 "A new diffusion model presented in this paper can predict the transmission of potential energy through a uniformly wetted soil profile and without the movement of water or change in water content."

I got the impression that the authors have misinterpreted the original work of Iverson (2000) and are (apparently) unaware of the salient literature addressing the hydrologic response of saturated soils (e.g. Keller et al., 1989; Haneberg, 1991; Berti and Simoni, 2010). The statements that in saturated conditions "the change in pressure potential occurs without the change in water content" (p.11) or that "the water content is constant" (p.6) are not true.

In fact, it is well known in soil physics (e.g. Bear, 1972) that even in a saturated soil there is a unique relationship between pressure head and water content: above the air-entry value, a change in pore pressure causes swelling or consolidation of the soil matrix (according to the principle of effective stress) and this changes the volumetric water content of the soil. The variation of water content is of course very little (hard to detect or measure) because of the low compressibility of the soil skeleton.

Therefore, in a saturated soil, the change in volumetric water content per unit change in pressure head C() is directly related to the coefficient of volume compressibility mv. In this condition the rainfall-induced pore pressure rapidly propagates downward with a diffusive process that can occur with or without much water flux (Iverson, 2000).

Reading the paper, it seems that the authors do not consider the possibility of pressure



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diffusion within a saturated, compressible medium. If this is the case, I strongly suggest the authors to refocus their work, shifting the emphasis from the methodological approach to the (interesting) field data used to test the model. If, instead, the authors are aware of this issue and I've missed the point.. please explain more clearly the novelty of your model.

In any case, I believe that the paper cannot be accepted in its present form. I strongly encourage the author to revise to revise their work and to resubmit it for further consideration.

References

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Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/11/C674/2014/hessd-11-C674-2014-supplement.pdf

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 2355, 2014.





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