Hydrol. Earth Syst. Sci. Discuss., 11, C941–C945, 2014
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Hydrology and Earth System Sciences
Discussions

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

Anonymous Referee #2

Received and published: 16 April 2014

Summary:

This study uses analytical models and field-based data to quantify the relationship between rainfall, pore pressure change, and the potential for landslides. First, the authors describe previous studies that have used similar data and models to quantify pore pressure change and slope stability. Next they develop a modified formulation of the commonly used pore pressure diffusion model. According to the authors, their formulation is more appropriate because past studies have used an improper parameterization and physically unrealistic model. The main difference between the model in this paper and previous papers is attributed to the diffusivity constant and the mechanism driving the pressure change. Here they state that the diffusion of energy imparted from rainfall propagates through the pore water to depth, while previous studies have discussed the

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increase in pressure driven by the infiltration of rainfall. Lastly, they combine their pore pressure diffusion model with the infinite slope stability model to understand how slope stability evolves over the course of a precipitation event.

Recommendation:

This study is thought provoking because it brings a well-established model into question and puts forth a modified version of this model. Overall this manuscript is well written and concise. There are several minor sentence structure issues, which I will address below.

I do not agree with their major claim that Iverson's (2000) diffusion model and other similar models are fundamentally flawed because they are derived from Richards equation. The pore pressure diffusion equation is derived from the physics of the poroelastic response of a compressible medium subject to transient pressure changes (i.e. rainfall). One way to derive this equation is by using Richards equation (as shown by Iverson, 2000), but it can be derived from more basic principles. It seems this aspect is not considered in this manuscript.

The main argument of this manuscript hinges on the author's statement that Richards equation is inappropriate to describe pore pressure diffusion because it relates the pressure head to soil moisture, which does not change once saturation is reached. They also suggest that the equation describes the diffusion of water mass (i.e. not pore pressure) through the soil matrix. Their major issue with this formulation is that once the soil becomes saturated the infiltration rate significantly decreases and therefore pressure change must occur without a change in soil moisture or water flux. Along with this point, they suggest that the hydraulic diffusivity (a widely used soil property) is inappropriate because it describes the diffusion of water through the soil matrix. They instead propose that energy imparted by the rapid rainfall diffuses through the saturated water column to depth.

I disagree with their main arguments. Richards equation is a suitable way to derive

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the pore pressure diffusion equation, as has been shown by numerous authors (e.g., Iverson, 2000; Berti and Simoni, 2010). Importantly, Iverson's formulation of Richards equation does not depend on soil water content (see equation 1 in Iverson 2000). It appears he has used the chain rule to describe Richards equation as a function of pressure head. Another important point that disagrees with the authors is that the hydraulic diffusivity does not describe the diffusion of water mass, it does describe the diffusion of pressure through the soil matrix.

Overall I am intrigued by the new conceptual approach to predicting the pressure response from rainfall energy and its relation to landsliding, however in it's current state I do not think this manuscript is ready for publication. I suggest the authors revisit the previous work on this subject. I also suggest the author's spend more time to develop their description of the energy imparted by the rainfall because it is confusing. Lastly, I am not sure their formulation is any more useful than previous formulations since their results and values (e.g., energy diffusivity is comparable in magnitude to hydraulic diffusivity) are essentially the same.

General Comments: 1) In this manuscript the authors state that for a saturated soil the infiltration of rainfall can be insignificant, however there is no discussion of overland flow. How do you consider overland flow in this model? Furthermore, shallow landslides often occur on steep slopes with highly permeable soils where overland flow is seldom observed. This suggests that rainfall is able to infiltrate throughout precipitation events.

2) One minor issue is that the model put forth in this manuscript is not "new" it is "modified" or "conceptually modified". I suggest referring to the model as the "modified diffusion model" rather than a "new diffusion model".

3) The energy diffusivity, d_e, is similar in magnitude to typical hydraulic diffusivities. There is no discussion in the text to explain why these values are so similar if the physical mechanism is so different. This should be addressed.

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4) The results from the slope stability analysis are a bit confusing. According to Figure 7 and Figure 8 there are several instances where slopes become unstable, yet there is no mention of actual landslides occurring. Also the depths of these instabilities are extremely shallow. I think if this manuscript seeks to describe landslide potential, there should be more discussion in the text about landslides in the field area that is being modeled.

Specific comments/technical corrections:

Page 2356 line 3-4: delete “within which the slide is triggered”. Change “of the storm” to “rainfall”.

line 6: change “period” to “rainfall”.

line 14-17: delete sentence starting with “Antecedent” and ending with “storm”.

Page 2357

line 4: There are more appropriate references that have developed the relationship between pore pressure and landsliding.

line 7: Add references at the end of “shallow landslides”.

line 12-28: These sentences read very much like a list and not like a paragraph. The phrasing and sentence structure is a bit awkward. I suggest rewriting this section to make it flow better.

Page 2358

line 1-19: These sentences also read very much like a list and not like a paragraph. The phrasing and sentence structure is a bit awkward. I suggest rewriting this section to make it flow better.

line 25: Iverson (2000) did not perform these experiments. They were performed by Iverson et al. (1997) and Reid et al. (1997) (see reference in Iverson, 2000).

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Page 2360

line 15-16: It is possible to define C(psi) where the water content is constant. It conforms to the saturated value, which corresponds to its minimum value.

Page 2362

line 1-5: The quantitative definition for hydraulic diffusivity is to describe the transmission of pressure through a porous media, not the diffusion of water through a porous media.

Page 2369

line 15: Change “landslides” to “landslide” in title.

line 23-24: Equation (15) is missing the unit weight of water in the numerator with the h_w and tan(phi) parameters.

Page 2371

line 15: I think there is a word missing after “Immediately”. Or if you meant it happened immediately then I suggest you rewrite this sentence more clearly.

Page 2373

line 5-10: Richards equation is able to describe unsaturated and saturated flow.

Figure 6: Are these values observed in the field or calculated using the model? They appear to be calculated, but it is unclear.

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