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

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In this response, C marks the beginning of a comment and R marks the start of the response to the preceding comment.

Dear authors

C1:

thank you very much (and also the reviewers) for the in-depth discussion on your manuscript. Because the complexity of the article and discussion and the disagreement between reviewers and authors, I needed quite some time for the editorial decision. Your article is indeed thought provoking and obliged all persons involved to think

about the basic principles again. It is intriguing to see that your newly derived model is basically similar to the Iverson model except with another definition of the diffusivity coefficient. Does this not hint to only a (semantic?) definition question? (see also comment referee 2 C943 point 3)).

R1:

Hydraulic diffusivity coefficient in Iverson's (2000) paper and Pressure head diffusivity coefficient in our manuscript may appear as a matter of semantics, but they do not describe the same physical process, as indicated by referee 2. The former describes the transmission of water through soil, while the latter describes the transmission of pressure head through pore water (or even pure water, not in a poroud media) . This is also reflected in the respective boundary conditions for which the models are solved. While our model is solved for pressure head condition from the kinetic energy of an intense rainfall at the ground surface (Waswa and Lorentz, 2014: page 2368, Eq. 11), Iverson's model is solved for pressure head condition from infiltration of rainwater at the ground surface (Iverson, 2000: page 1901, Eq. 25c).

In our model we demonstrate that transmission and change in pressure head in deep soil profile is without, or with very little, water fluxes. On the other hand, Iverson demonstrates that after infiltration of rainwater, the change in pressure head in deep soil profile is as a result of transmission of water (Iverson, 2000: page 1902). This is clearly indicated in a statement that: "The lower boundary condition (Eq. 25b) states that at great depths below the water table, transient vertical groundwater flux decays to zero and steady state pressures described by (Eq. 10a) persist" (Iverson, 2000: page 1902).

C2:

The paper consists of two parts: a theoretical derivations and the measured data and calculations. Most attention is given to the physical incorrectness of the Iverson model. Both referees do not agree. You disagree with the referees. The minimum that is required in such case is a full analysis (both conceptual and mathematical) where Iverson

and the reviewers 'misjudge' the physical processes. I ask the authors to provide this in their new version of the article (you can also include examples like you did in the final reply). This will go in full review again.

R2:

This is a very important comment. Iverson arrived at his diffusion equation from what he refers to as 'the standard Richards equation for vertical infiltration' (Iverson, 2000: page 1901 – see also our Response R1) by assuming that when soils are sufficiently wet the water flux, due to gravity force, can be neglected and that when the soils are sufficiently dry the water flux, due to capillary pressure, can be neglected. We do not agree with these assumptions, because we think that they contradict the physics of infiltration. This error (according to us) and others (e.g. use of infiltration in defining pressure head on an upper boundary of a saturated soil profile – see also our Response R1), are described in our manuscript (Page 2361 - Page 2363). We have even given illustrations (similar to the ones in our last reply) in our manuscript (Page 2362).

C3:

In attached file I include some of my own comments summarized for the review. I thank you for your patience and I look forward to the new submission. Kind regards Thom Bogaard

R3:

Thank you very much for these specific comments.

Some summarizing remarks:

C4:

One main point is that the Richards equation does not hold in saturated condition (and therefore the Iverson model is physically incorrect). One of the main concerns of the reviewers boils down to soil compressibility. Referee 1 is correct that that there does

exist a unique relationship between pressure head and water content within a REV, also in saturation. In my view you used an incomplete description of the Richards equation. The Richards equation combines the energy equation (potential gradient water flow: Darcy) and the water balance. Your equation (1) assume an incompressible water and soil. The full version also contains soil compressibility (in this form the equation is used in geotechnical world). Therefore there still is a unique relation between pressure and water content and as such the Richards equation can be applied as pointed out by both referees. Your statement that Richards equation cannot be used with the argument of the relationship between pressure and water content seems not to hold, or should be proven by taking the full Richards equation (including water storage in a soil by compressibility). This could also influence your model derivation (Appendix A). Here you assume a rigid and non-deformable porous medium. How does this assumption influences your model?

R4:

We imagine that the Richards' equation presented in our manuscript is complete, only that it is what is commonly referred to us the 'mass picture' Richards' equation.

While I agree that soils can be compressible, I disagree that the modified Richards' equation that considers compressibility can appear as expressed by Iverson (2000). Especially for a saturated soil, I imagine that, compressibility cannot just be limited to the porous structure (solid), but the compressibility effects should also be extended to the mass balance of pore water. It has been argued that this compressibility, or consolidation, of the soil accounts for the increased pore water pressure in landslides (e.g. referee 1 and Berti and Simoni, 2010). This argument may be physically correct, but is not the case in landslides. In landslide occurrence, it is the increase in pore water pressure that drives the effective stress of the soil (resulting in swelling) and, consequently, we expect the soil to expand/swell (see Response R9) and not to consolidate.

In addition, a compressible soil means a dynamic porosity. Consequently, all the soil

parameters that depend on porosity, e.g. hydraulic conductivity and soil water content, do not remain constant. In the derivation of our model we assume an incompressible soil. However, compressible media means a dynamic water content (see Response R5) and hence a dynamic pressure head.

C5:

If I look at your mind model (page 2362 Line 15 onwards) you say that change in water pressure is neither the result of change in water content nor arrival of new water. Of course and the arrival of energy is instantaneous if the medium is rigid (like a bucket of water where you add a layer of water, the pressure increase is felt directly at the bottom of the bucket. However, if the soil is very compressible the arrival of the increased water pressure at the top of the water body will take some time depending on soil compressibility.

R5:

Even in a bucket of water (without a porous media), pressure head is transmitted only relatively faster, but not instantaneously. This transmission of pressure head depends on properties of the fluid. For instance, transmission of pressure head through water would be quite different from transmission of pressure head through oil. Note that this transmission of pressure head cannot be described by the hydraulic conductivity, but by our proposed pressure head diffusivity coefficient.

Now, when you introduce a porous media and this porous media is non-deformable (i.e. rigid or incompressible), and saturated, the pressure head will still be transmitted through pore fluid (pore water). Again, this transmission of pressure head is the same as that in pure water, except that in this case the water through which pressure head is being transmitted is in a porous media. Note that the added pressure head does not have to necessarily come from a fluid of the same nature as that in the porous media. An example of this is the Lisse effect, where an additional pressure head (being transmitted through pore water) is from pressurized air in an unsaturated zone

(Waswa and Lorentz, in press). The transmission of an added pressure head, across a saturated porous media, will be relatively less instantaneous than in pure fluid, because of the reduced/narrow flow path and the transmission of pressure head is a function of soil water content (Marui, 1993: page 469). It is worthy emphasizing here that the medium of transmission of the pressure head is the pore fluid, not the pore.

C6:

Another point that should be addressed extensively by the authors is the discussion on the correctness of the Iverson model. If you want to make this point so strongly, I think you should elaborate on this in your paper. You disapprove with Iverson's approach but end up with a mathematically identical model. This needs a more in-depth analysis where you think Iverson paper goes wrong (conceptually and mathematically). For example, in your work and replies (see e.g. the final reply, page 2) you write that Iverson neglected the gravity component. In the original paper (page 1901 between eq 18 and 19) it is written: "the gravity flux term involving Iz/Kz can be neglected". Do we talk about the same here? (or is it a bit awkward formulation to say that the derivate with z is not changing (equal water content with depth. . .) for the gravity flux term?).

R6:

My problem is the way that Iverson's equation derived. The derivation appears to violate the physical processes that might be occurring in the field, resulting/during an intense rainfall triggered shallow landslide. All these flaws (according to our understanding) are pointed out in our manuscript.

C7:

In your last reply (page 5 last part) you say that Iverson does not write about poroelasticity. Maybe not in words but in his definition of C0 (equation 3) he writes that this is minimum when the soil gets saturated. This definition includes soil compressibility (Like referee 1 points out).

R7:

We imagine that the "change in water content per unit change in pressure head", i.e. specific water capacity, becomes minimum at both ends of the soil water content (i.e., in the dry state and in the saturated state of the soil). This can be seen from a water retention curve of a soil (even an incompressible porous media). Therefore, we think that it is pure speculation (without proof) to say that the minimum value of the specific water capacity (Co) in Iverson's work includes compressibility.

Furthermore, if the soils considered by Iverson were compressible, we would expect to see parameters like coefficient of compressibility/consolidation (as suggested by referee 1), and even a dynamic hydraulic conductivity (because a compressible soil/porous media means a dynamic porosity, and, hence, dynamic porosity-dependent parameters, e.g. hydraulic conductivity). We do not see such parameters and processes in Iverson's paper.

Lastly, Iverson has clearly indicated that the change in pressure head in the soil profile is as a result of groundwater fluxes (see also our Response R1). In describing the upper boundary condition for his model (Iverson, 2000: Eq. 25c), Iverson states that pressure head is because of infiltration (also reflected in the title of his paper). In describing the pressure head at the lower boundary (Iverson, 2000: Eq. 25b), Iverson states that pressure head at this boundary does not change, because groundwater fluxes decays to zero. From these definition, it is very clear that compressibility does not play any role in the transmission and change of pressure head. Furthermore, what could be the physical connection (physical processes) between the generation of pressure head by infiltration at the upper boundary and the transmission of pressure head as a result of compressibility of the media?. Where and how do the process occur?.

C8:

Concerning the kinetic energy of rainfall on a surface. If it falls on a rigid surface the

energy will stay in the soil skeleton and that does not influence the effective stress (or increase the weight as it is a kind of external pressure on the skeleton.). If the rainfall is fully absorbed by the water layer only it will add energy to the system and thus to the pore pressure in the subsurface.

R8:

We wish to clarify some issues here. The kinetic energy from rainfall is imparted into the pore water at the point of contact of the raindrops and the pore water. This happens at the ground surface. It is for this reason that the requirement of the extension of the tension saturation (the pore water, under tension, that completely fills the pores) to the ground surface (Gillham, 1984) is important. The emphasis here is that the kinetic energy is not introduced into the pore water via the soil solids (particles) at the ground surface. There are two ways, therefore, that the pressure head in the existing soil pore water can be elevated. First, is when the soil is dry, or unsaturated, allowing infiltration to occur. This is the way described in this comment C8, and is not the only way rainfall can add energy (pressure head). The second/other way, is where the saturation (or near saturation) condition extends to the ground surface. This condition is described and simulated by Iverson (2000: page 1904) and Rasmussen et al. (2000). Under these conditions, the infiltration of rainfall is too minimal to account for the increased pressure head that triggers landslide. Waswa et al. (2013) found that under this condition, an intense rainfall at the ground surface, is capable of introducing a proportionate amount of energy into pore water, without infiltration.

C9:

In erosion studies a lot of measurements have been done on the kinetic energy of rain on a surface (for splash erosion) and also on energy dissipation on a surface or on a small water layer. How does this relates to your findings? Or can it not be compared?

R9:

After my findings, I have, on several occasions, imagined of a relationship between the initial moisture content of the soil and erosion, under the action of rainfall. The soil particles are usually more firmly bound to each other, if the water film between them is under tension. Nevertheless, the bond between the particles becomes weak as the tension in the water film decreases, or rather when the pressure within the pore water tends towards positive). An illustration of this has also been reported by Ranjan and Rao (2000: page 136) that:

"it is well understood that the most important consequence of the increase in capillary pressure (tension) is an increase in shearing resistance of the soil mass. This is the reason why one can walk comfortably on a sand beach adjacent to the sea, a patch falling within the capillary zone but not on the dry sand above the capillary zone some further distance away from the sea where the soil exhibits much less bearing power in the absence of capillary pressures. Again, when the sea water breaks the capillary menisci during tides, the temporarily induced shearing strength is lost. Anyone walking on this surface at this time can virtually feel the ground sinking under his feet. Yet, in all these different states of the beach zone, the relative density of the soil remains more or less the same. It is the presence or absence of capillary moisture that makes all the difference.

"Capillary moisture in fine sands and silts allows excavation to be made because of the stability it provides by virtue of the induced strength. Such excavations would be impossible under dry soil conditions or under water table conditions.

"Bulking in most sands is another manifestation of the capillary moisture. The capillary menisci surrounding the soil grains produce apparent cohesion, which holds the particles together, enclosing honey combs." This paragraph supports our argument Response R4, that an increase in pore water causes swelling of the soil and not consolidation.

From the above illustration, we imagine that the soil particles at the ground surface are

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bound to the soil mass below. But the bond depends on the capillary pressure in the film of water between the soil particles and this will influence the amount of energy required for the soil particles to be detached.

These are only imaginations and it would be interesting to verify/investigate them. However, it is along such kind of imaginations that I would like to re-state that an increase in pore water pressure drives the effective stress in soils, and an increase in pore water along the potential failure plane pushes the soil particles apart, increasing the potential of the upper layer to slide down the slope. It is not the effective stress (consolidation) that results in an increase in pore water pressure.

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