Review of “Modelling the control of groundwater on landslides triggering: the respective role of atmosphere and rainfall during typhoons”

Summary:

In this contribution, Pelascini et al. examined the effects of pore pressure changes due to atmospheric pressure and rainfall infiltration on the stability of hillslopes of finite length. Here the Mohr-Coloumb failure criterion was the conceptual basis of stability, though throughout the paper, emphasis was placed on pore pressure components of the effective stress rather than the failure criterion. Time-evolution of pore pressure at the hillslope crest and toe were calculated by convolving analytical solutions for groundwater flow and diffusion of pore pressure with synthetic and real timeseries of rainfall and atmospheric pressure. The results showed that the importance of the two dynamic pore pressure-generating mechanisms (atmospheric pressure and infiltration) varied in space on the slope, largely driven by differences in depth to the water table, and in time depending on the mechanism’s response timescale. The results suggest that more attention should be paid to slope stability effects of atmospheric pressure fluctuations in large storms, and that estimates of landslide timing in relation to pressure fluctuations and precipitation could help distinguish drivers of landslides.

My experience makes me most suited to comment on the groundwater hydrology aspects of this paper, rather than the landslide hazard component. In this respect I have a few concerns.

1. The hydrological model used in this paper is a combination of a Dupuit-Forchheimer (D-F) aquifer model and a one-dimensional infiltration model based on the Richards equation. After reading the paper I was left unclear on how exactly these two models interact, and what effects the transient component of the water table response has on their results.
2. There needs to be more careful attention paid to the relationship between this hydrological model and the expected groundwater dynamics of the landscapes the model intends to capture. (Steep landscape hillslope hydrology see e.g. Montgomery et al. (1997)). The linearized, horizontal-based form of the D-F model may be appropriate in low relief settings or for deep aquifers that respond slowly to recharge, but the landscapes considered here are steep, and recharge here is assumed to infiltrate instantaneously to the water table.
3. The two hydrological models operating together contain potentially contradictory information on the pore water pressure below the water table.

While I recognize that issues 2 and 3 are acknowledged in Discussion section 5.1, it seems that most of the paper does not meaningfully engage with these limitations. If the authors retain the current hydrological model, rationale and limitations of the model need to be more clearly stated up front in the introduction and methods sections. While I cannot comment on the novelty of the landslide hazards component of this paper, I was left with the impression that there is merit to exploring the processes they consider here, though I think the hydrological basis of this work could use more thought. I’ve added more details in the line-by-line notes below.

Line by line:

Title:

Title feels a little unspecific – what about the atmosphere, and what about groundwater? Could I suggest something along the lines of: “Finite-hillslope analysis of landslides triggered by excess pore water pressure: the roles of atmospheric pressure and rainfall infiltration during typhoons”

Abstract:

Two things in the abstract seem contradictory to me. Please reconcile or clarify the following:
Lines 10-11 you state that “atmospheric pressure changes and rainfall induced groundwater level change can generate pore pressure changes with similar amplitude,” but then in line 17-18, you say they differ by perhaps several orders of magnitude.
Lines 14-15 you state that “rainfall infiltration and atmospheric pressure variations” are described by diffusion equations, but then in line 18 you say the effects of atmospheric pressure are instantaneous. This may be a matter of the phrasing, but it is confusing.

Introduction:

Line 31: “cumulated rainfall” -> “groundwater recharge”
Line 38: “Little attention has been by received by this potential slope destabilisation factor” -> “This slope destabilisation factor has received little attention.”
Line 40: “…modifying slope stability.” Citation?
Line 55-56: “As both rainfall and atmospheric effects implies pore pressure diffusion in groundwater, the link to slope stability requires a specific model.” This sentence seems vague to me.
Line 59: “allows *us* to define”
Line 62: remove “about”

Methods:

Line 65: Not sure what “homogenous half space” means and it is not mentioned anywhere else in the text.
Line 90: “Under rainfall constrain” ?
Line 101: “hydrogeological model” usually refers to a model of the characteristics of an aquifer – it’s permeability, porosity, stratigraphy and composition (e.g., Condon et al. 2021 5.1). Maybe hydrological model would be better?
Line 101: I would use “slope” or “topographic slope” over “dip,” because dip has a different geologic meaning.
Line 104: Interesting, I have not seen this called the diffusivity equation before. Looking around online, it seems this term is more commonly applied in the petroleum industry to other fluids? In hydrology I see this called the Boussinesq equation (e.g. Troch 2013, paragraph 9, Boussinesq equation for horizontal aquifers) or simply the Dupuit-Forchheimer equation.
Line 110: “storage” -> “storage coefficient”
Line 112: “in term of” -> “in terms of”
Line 119, 130: “in function of” -> “as a function of”
Line 131: It's unclear to me whether you use this solution, given the discussion in 2.3, where it seems that only h_s matters. Does the static pressure head in response to recharge come into effect in your model?
Line 135: This sounds you are disregarding the transient component of the water table variation in the Dupuit-Forchheimer model? Or are you only disregarding its affect on pressure head and not on water table position?
Lines 137-138: It is not necessarily described by diffusion. In Iverson (2000) there are extensive assumptions and conditions required to reduce the Richards equation to this particular 1D diffusion form. These need to be identified and discussed.
Line 138: “characterise” -> “characterised”
Line 139: “model considered a 2D mode” Model? Consider rephrasing to avoid repetition.
Line 141: “one-dimension” -> “one-dimensional”

Lines 148-149: Could you more clearly state the boundary conditions to arrive at this solution? The constant loading gives the surface boundary condition, what is the condition at depth? Seems like this solution is not accounting for the water table depth?
Line 152: tc = z2/D should this be \hat{D}?
Line 154: “convoluted” -> “convolved”
Line 167: Again more clearly state lower boundary condition.
Line 168: change citation type to “Carslaw and Jaeger (1959)”

Figure 1:
Can you label hydraulic head *h*?

Results – Synthetic:
Line 176: “toe of and at the very top of” I would call the top either *crest* or *ridge.*
Line 177: I think some more elaboration of this consideration of the slope at yield is needed. It seems critical to how your are interpreting the results.
Line 183: Don’t need the figure description in parenthesis.
Line 185: “an 86.4 mm cumulated rainfall” -> “86.4 mm of accumulated rainfall”
Line 197: Underestimation of tc… Can you provide more insight into the physical meaning of tc here? Semantically, it also seems to me less that tc is underestimated, and more that it may not be the right quantity for comparison with the timescale estimated.
Line 200: “slop” -> “slope”
Line 214: Still unclear exactly how the water table rise during event is incorporated into your model.

Results – Application:
Line 247: “east of Taiwan” -> “eastern Taiwan” or “the east of Taiwan”
Line 251: “inferior to” -> less than
Line 255: “contrasted” -> contrasting
Line 257: remove “has”

Figure 5: Great plot on the left – I like how your selected storms are a kind of envelope around the extreme events.
Section 4.2:
- How did you select hillslope length? How sensitive are results to hillslope length? Can you use your analytical solutions to show something about this?
- Is there evidence in the literature or in published well/piezometer data that hillslopes fully saturate during typhoons?
- Provide the equation for the infinite slope model used for comparison
- Can you say anything from your model about which storms cause landslides and which ones don’t?

Line 278: you say “amount of rainfall” which to me implies rainfall depths, but rates are given. I would make these agree.
Line 290: “caps off” colloquial language, consider replacing
Line 296: “in function of the event” what does this mean?

Figure 6: The use of black and blue together in plots b-i is difficult to read. I would choose a better color contrast.

Discussion:

Line 314: “models limitations” -> “model limitations”
Line 315: “considered in this study consider” rephrase
Line 355: “has been” -> was
Line 365: Worth mentioning in this section that the diffusivity in the 1D model is Iverson (2000)’s maximum hydraulic diffusivity, derived for conditions near saturation.
Line 385: Can you provide some more physical insight here on why diffusivity affects these in opposite ways?
Line 392: When considering only these two effects. Do you think atmospheric pressure effects could be more important than other mechanisms going on when hillslopes fully saturate, like seepage? (Found this, line 455-456)
Lines 399: When you say that the response of psi_air is instantaneous, I think that then the pore pressure response to a gate function should just look like the gate function. But it seems like what you’re implying is that there is no delay in the beginning of the response, even though there still is a decay of the response in time?
Lines 406-408: How does this finding compare with literature? Do we see landslides occurring in these locations?
Line 431: “dominants” -> dominant
Line 443, 449: “repartition” Partitioning? Check word choice.
Line 450: “typhon induced” -> “typhoon-induced”
Line 478: weeks or months after the rain event – has this been observed? I think a reference would strengthen this argument.
Line 489: “amount of cumulated rainfall” -> “depth of rainfall” or “accumulation of rainfall”
Line 495: large variations in pore water pressure?

Works Cited:

Condon, L. E., Kollet, S., Bierkens, M. F. P., Fogg, G. E., Maxwell, R. M., Hill, M. C., et al. (2021). Global Groundwater Modeling and Monitoring: Opportunities and Challenges. *Water Resources Research*, *57*(12), e2020WR029500. https://doi.org/10.1029/2020WR029500

Iverson, R. M. (2000). Landslide triggering by rain infiltration. *Water Resources Research*, *36*(7), 1897–1910. https://doi.org/10.1029/2000WR900090

Montgomery, D. R., Dietrich, W. E., Torres, R., Anderson, S. P., Heffner, J. T., & Loague, K. (1997). Hydrologic response of a steep, unchanneled valley to natural and applied rainfall. *Water Resources Research*, *33*(1), 91–109. https://doi.org/10.1029/96WR02985

Troch, P. A., Berne, A., Bogaart, P., Harman, C., Hilberts, A. G. J., Lyon, S. W., et al. (2013). The importance of hydraulic groundwater theory in catchment hydrology: The legacy of Wilfried Brutsaert and Jean-Yves Parlange. Water Resources Research, 49(9), 5099–5116. https://doi.org/10.1002/wrcr.20407