Natural Hazards and Earth System Sciences
Discussions

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Comment on nhess-2021-340

Anonymous Referee #2

Referee comment on "Finite-hillslope analysis of landslides triggered by excess pore water pressure: the roles of atmospheric pressure and rainfall infiltration during typhoons" by Lucas Pelascini et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-340-RC2, 2022

Re: Review of *Modelling the control of groundwater on landslides triggering: the respective role of atmosphere and rainfall during typhoons* by Pelascini et al., submitted to Natural Hazards and Earth System Sciences as NHESS-2021-340

The subject manuscript describes a modeling study focused on identifying relative (and absolute) contributions of rainfall and atmospheric pressure change during typhoons in causing landslides. Consideration of atmospheric pressure effects on slope stability is quite novel; I'm aware of only one other study related to this subject (cited in paper). During typhoons, atmospheric pressure can drop several kPa, causing reduced slope stability by reducing effective normal stress, whereas rainfall amounts may contribute several tens (or more) of kPa to reduce this normal stress. However, by using simple 2D and 1D hydrogeologic modeling, the authors show that these effects on effective normal stress vary through time and slope position, such that differing initial water table conditions and hydraulic diffusivity of hillslope materials may result in relatively greater or lesser effects on slope instability from rainfall and atmospheric pressure change. The manuscript therefore presents important new insights into landslide triggering factors. Unfortunately, the model is poorly tested by empirical evidence, primarily because of the lack of data available to identify landslide timing and potential triggering during typhoons when rainfall and atmospheric pressure drop both are significant.

The conceptual and mathematical models developed in the manuscript involves deep groundwater within a homogeneous hill (peak to valley), discounting oftentimes perched groundwater within regolith, where many landslides generate during/following intense rainfall. How representative of the test locations in Taiwan is this conceptual model? What implications/omissions exist with respect to the lack of consideration of regolith?

The 2D and 1D models differently treat application of transient atmospheric pressure and rainfall, with diffusion occurring from different locations (ground surface or water table) depending on the forcing. Section 5.2 summarizes some of these differences and indicates that model uncertainty might explain some field observations. The dramatic differences between the models and, sometimes, their output makes me wonder what results may be believed, as well as wonder why the modeling approaches were not more consistent. A more critical evaluation of the implications of their differences is warranted with respect to both magnitudes and timing of effective normal stress change. For example, lines 349-350 indicate that the lack of an infiltration model and application of rainfall immediately and entirely at the water table “might underestimate the response time.” Actually, except in special circumstances, these factors definitely underestimate the response time and by variable amounts ranging likely over several orders of magnitude for realistic conditions. What are the overall impacts of the simplifications involved with the modeling?

One or two lines mention that seepage forces may be important contributors to slope instability, and they certainly are, especially near discharge zones near slope toes (e.g., Iverson 2000, cited in paper). The importance of non-hydrostatic gradients in slope instability should be emphasized, especially with respect to landslide triggering from regions near slope toes. Additionally, please see next comment regarding landslides near slope toes.

2D modeling of the homogeneous hill suggests that groundwater is shallower near the slope toe and deeper near the slope crest, as is well known. This initial condition strongly affects atmospheric- and rainfall-induced pore pressure change timing and magnitude along the height of the hillslope, as the manuscript demonstrates. The authors note that, for one typhoon, landslides in Taiwan concentrated near the lower parts of the slope, and they propose that this at least partly resulted from the shallower groundwater depth there. However, much of the preceding text noted that slope toes are more likely than upper parts of slopes to be saturated from long-term conditions, and if saturated, rainfall has no effect on stability and atmospheric pressure change will be of primary importance. Why would atmospheric pressure change in saturated regions not have been responsible for the landslide distribution? Additionally, such hillslope groundwater distribution should be ubiquitous, so does not the paper imply that rainfall/atmospheric-pressure-induced landslides everywhere should concentrate on lower parts of slopes? Can the authors provide evidence for this? Finally, 3 typhoons in Taiwan are mentioned. It would be beneficial if the authors described how pre-storm rainfall for the 3 events may have resulted in different landslide distributions, in accordance with their model.

Please see the accompanying mark-up for additional comments and suggestions to improve the manuscript.

Please also note the supplement to this comment:
https://nhess.copernicus.org/preprints/nhess-2021-340/nhess-2021-340-RC2-supplement.pdf