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Reply on RC2

Lucas Pelascini et al.

Author 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.,
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Review of "Modelling the control of groundwater on landslides triggering: the respective role of atmosphere and rainfall during typhoons"

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.

- **We thank the reviewer for his comments. This specific comment summarizes well the novelty and limits of our manuscript. The model we use has indeed only be tested against data from a slow-moving landslide (Schulz et al., 2009) for small atmospheric pressure changes. Our manuscript uses this tested model in the case of larger atmospheric pressure changes (i.e., typhoons) and consider mostly the case of catastrophic landslides. For these landslides, there is to our knowledge (as also stated by the reviewer) no clear evidence of landslides triggered by atmospheric pressure changes, potentially due to the current inability of observations to resolve the timing of landslides during large storms.**

The goal of the modelling approach was (1) define a simple model, as simple as

the original Iverson model, which could better represent groundwater dynamics under hydrological and atmospheric boundary conditions (2) define the main physical controls. This allows to underline observation requirements which could better inform on actual processes in landslide generation.

- **We now clearly state that the model has not been tested against natural catastrophic landslides (Lines 182-184)**

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?

- **This is a very good point. Our model considers a homogeneous hillslope with no lithology change above a single unconfined aquifer. This hypothesis comes from the hydrological model, which considers an aquifer relatively thick in comparison to the 500 m length of the hillslope so that the groundwater flow can be described as horizontal.**

This simplification indeed does not fully account for the hydrogeological complexity under Taiwan's hillslopes but is necessary to develop a model rooted on basic analytical solutions.

However, the model can be applied at any scale and therefore represent smaller aquifer – including perched ones – at smaller scales, as long as the boundary conditions and the hypothesis of the hydrological model are respected. Considering smaller aquifers, for instance developed in the regolith, and depending on their connexion with the hillslope aquifer, this would lead to similar water table profile yet with shorter characteristic times, following this scaling , where L is the length of the considered perched aquifer.

Following this work, we are now setting up a modflow-based model on a Taiwanese catchment, driven by a land-surface model, to better represent actual groundwater flow in heterogeneous systems.

- **We now acknowledge this simplification and limitation and discuss the scale effect in the discussion (lines 347-350 & 525-529) and illustrated it with a new figure (Fig. 8)**

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?

- **We acknowledge this comment. The 2D hillslope model mainly differs from the infinite slope model by having a dynamic and fluctuating water table, and especially to represent the impact of uphill groundwater flow contributing to local pressure change, in addition to local recharge. This is very important, and translates as critical when considering the specific behavior at the toe and crest of the hillslope. This leads to a key difference as the 2D model uses the water table variations to feed into the pore pressure diffusion. The underlying hypothesis here is that pore pressure variations induced by rainfall are caused by changes in the height of the water table. As a consequence, since the pore pressure is applied from the water table, the infiltration time needed for rainfall to reach the water table is disregarded. Indeed, this underestimates the response time for the areas where the water table is deep – namely the upper part of the hillslope – and does not allow to describe shallow landslides.**
- **We now clearly discuss this limitation of the 2D model in the discussion comparing 1D and 2D models (lines 370-375)**

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.

- **Indeed, the seepage effect leads to slope instabilities and is discussed in many papers. Loss of stability by seepage is mainly related to vertical flow, creating a force against the weight of the slope; yet the hydrological model only considers horizontal flows. Approximation of seepage forces based on such a model would greatly underestimate the effects on slope stability. A proper estimation would require the simulation of flowpaths, by a more complex hydrological model, better representing the impact of heterogeneity, and is beyond the scope of this study. The manuscript here focuses only on rainfall infiltration creating pressure front and atmospheric effects on the pore pressure – which are non-hydrostatic processes. This hypothesis is stated during the model description and the seepage is mentioned in the model limitations.**
- **A brief description of the seepage destabilising effect and why it is not computed in our model is provided in the section describing the water table model (lines 138-141).**

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.

- **The idea behind these observations is indeed that the landslides distribution could be induced by atmospheric pressure changes; since infiltration – and therefore rainfall-induced pore pressure – is prevented in fully saturated areas. However, as already stated the model focuses on the dynamic pore pressure effects, leaving aside seepage or more complex effect, and disregarding the hydrostatic loading, which also may decrease stability in these areas.**
- **We now more clearly state that the landslides distribution towards the toe of the hillslopes indicates an atmospheric-driven failure (lines 489-491).**

Taiwan has been chosen for the large number of typhoons, provoking many large atmospheric disturbances. The repartition of landslides towards the toe of the slope is indeed not limited to this area, but a general trend for landslides triggered by weather events. In case of earthquake-triggered landslides, slope failures tend to focus on the crest rather than the toe of the slope. These observations can be found in (Meunier et al., 2008) already cited in the manuscript.

Among the 3 typhoons showed, only Morakot led to massive amounts of landslides. Masta and Krosa generated mudslides and debris flow, but in less quantity. No study has been conducted on the landslides and slope failure distribution along the hillslope after all these events.

However, the model suggests such events over partially saturated slopes would have led to landslides near the toe of hillslopes during the typhoons, due to the atmospheric effect, then deeper landslides towards the upper unsaturated part of the hillslope after the typhoon due to the diffusion of rainfall-induced pore pressure. This has to be taken with caution, as the model does not represent all destabilizing processes – such as seepage – and should be verified with observed data.

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

improve the manuscript.

The suggestions and comments have been taken into consideration in the manuscript.