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Reply on RC1

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., https://doi.org/10.5194/nhess-2021-340-AC1, 2022

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.

We are grateful to the referee for this detailed and precise comments on the preprint.

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.

- 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.

- 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.

- 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.

Our goal was to better understand the specific (physical) controls of rainfall and atmospheric pressure changes on landslide triggering.

In geomorphology, the 1D diffusion model (Iverson model) remains as a reference for rainfall-driven landslides. Though, this model has strong limitations (infinite slope, i.e. local point of view, inability to consider both recharge and GW upflow, define initial conditions …) and therefore difficult to use at hillslope scale.

Our point of view was to develop a simple model, as simple as the original Iverson model, which could better represent groundwater dynamics under hydrological and atmospheric boundary conditions. As a consequence, we are far from the quality of a "site model", as we rather define physical controls. 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, and also interception with the surface.

It appears the goal of the paper is not stated clearly enough, and this causes confusion for the reader. Some clarifications are indeed needed and will be added in the first parts of the manuscript. The goal is here to consider the water table variations through a simple analytical model in order to improve slope stability assessments.

- **The Dupuit-Forchheimer (DF) model considers horizontal flows and defines the water table surface. The 1D diffusion models allows for the computation of pressure propagation through the groundwater. The two models do not interact, the rainfall pressure diffusion feeds on the output of the DF model.**
- **Additional explanations are provided lines 88-91, 150-152, 175-177 and figure 1 has been modified to further illustrate how the models are implemented.**

- **The hydrological model here is indeed basic. It is based on the hypothesis of negligible vertical flow, which might be suitable for thick aquifers and/or low angle slopes. The hypothesis might not be valid in Taiwan. While this doesn't represent the complexity of the geology for the slopes in Taiwan, it has been deemed necessary to keep the model simple.**
- **We clarify this when we introduce the hydrogeological model (lines 121-126)**

- **The two pore pressure diffusion models indeed predict different pore pressures in response to the different forcing, but the solution of the diffusivity equation can be added by linearity. Physically this can lead to a pressure gradient and groundwater flows in two opposite directions.**
- **This limitation of the model is addressed in the discussion (lines 351-354).**

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"

We agree with this comment and have changed the title accordingly.

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.

Thank you for pointing this out. When thinking about pressure change, 10 hPa atmospheric pressure change is equivalent to a 100-mm rainfall event. Therefore, atmospheric effects can generate similar pore pressure amplitudes as rainfall, in some specific cases. However, the rainfall effects generally reach values of pore pressure orders of magnitude above the atmospheric ones because the latter is function of the derivative of the atmospheric pressure, and atmospheric pressure rarely drops in an instant. The sentence line 12 has been modified to prevent this potential confusion.

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.

The phrasing can indeed be confusing. Both rainfall and atmospheric effects are described with diffusion equations. However, the atmospheric effect is instantaneous due to the transfer of the pressure load from the atmosphere to the pores through the skeleton or solid phase of the medium. The decay of this change in pore pressure is described with diffusion equations. This is now clarified lines 18-20.

Introduction:

Line 31: "cumulated rainfall" -> "groundwater recharge"

Done

Line 38: "Little attention has been by received by this potential slope destabilisation factor" -> "This

slope destabilisation factor has received little attention."

Done

Line 40: "…modifying slope stability." Citation?

The work that was been referred to here was Schultz et al. (2009), and the citation has been added.

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.

The sentence has been clarified (lines 57-58).

Line 59: "allows us to define"

Done

Line 62: remove "about"

Done

Methods:

Line 65: Not sure what "homogenous half space" means and it is not mentioned anywhere else in the

text.

"Infinite homogeneous slope" might be more self-explanatory indeed.

Line 90: "Under rainfall constrain" ?

"under rainfall forcing"

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?

We agree, "hydrological model" is more appropriate since no assumption on the structure and stratigraphy are made.

Line 101: I would use "slope" or "topographic slope" over "dip," because dip has a different geologic

meaning.

Done

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.

We used the term diffusivity equation by default since it describes this physical phenomenon, but indeed, it is generally named after Boussinesq in hydrology. It will therefore be referred to as Boussinesq's equation in order to fit the hydrology community convention.

Line 110: "storage" -> "storage coefficient"

Done

Line 112: "in term of" -> "in terms of"

Done

Line 119, 130: "in function of" -> "as a function of"

Done

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?

This comment corresponds to the same issues raised in the general comment #1. The models needed to be presented with more clarity. The static position h_s of the water table is taken into account to define the water table initial position. The water table level is then updated using the transient solution h_t. The static pressure head (due to h_s) does not create dynamic pore pressure, and is not investigated in this study. Indeed, the focus of this work is the dynamic transient effects – this is why the slope is considered at yield – an no absolute values of safety factor are computed. We deliberately only modelled the transient/dynamic effects due to changes in water table level h_t inducing new loadings. These loadings are then fed into the diffusion model to propagate in depth.

- **Precisions have been added lines 150-152, and the use of transient water table into the rainfall-induced pore pressure diffusion model is explained lines 175-177 with the help of a new graph in Fig. 1.**

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?

This is answered in the previous comment.

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.

Indeed, the propagation of pore pressure can be described by pressure diffusion

– which is what Iverson proposed in his model, under the main hypothesis of an infinite slope geometry, and wet initial conditions, so that there are no changes in hydraulic conductivities above and below the water table. Then flow is described using Richard's equation and reduced to a one-dimensional diffusion equation.

- **These hypothesis are now mentioned line 157.**

Line 138: "characterise" -> "characterised"

Done

Line 139: "model considered a 2D mode" Model? Consider rephrasing to avoid repetition.

Done

Line 141: "one-dimension" -> "one-dimensional"

Done

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?

The upper boundary condition at the surface is a Neumann condition (known flow). There is no lower boundary condition, the solution used here is for the diffusion through a semi-infinite solid (therefore no lower boundary), as the aquifer is taken with a sufficient thickness for the horizontal DF model and its lower boundary can be considered at infinity with regards to the depths investigated here.

- **We now clearly state the boundary conditions of the model lines 165-166**

Line 152: tc = z2/D should this be \hat{D}?

The characteristic time, as presented in several studies (Iverson, 2000; Handwerger et al., 2013), is indeed tc = z2/D . Its definition has been rewritten in a clearer manner lines 161-163.

Line 154: "convoluted" -> "convolved"

Done

Line 167: Again more clearly state lower boundary condition.

As for the commentary relative to lines 148-149, there is no lower boundary condition, it is considered to infinity. The upper boundary this time is a Dirichlet condition. As for the previous commentary, additional explanation as well as more precise citation are provided lines 192-193.

Line 168: change citation type to "Carslaw and Jaeger (1959)"

Done

Figure 1:

Can you label hydraulic head h?

Done

Results – Synthetic:

Line 176: "toe of and at the very top of" I would call the top either crest or ridge.

Required changes have been done, the top of the slope is now been referred as crest in the manuscript.

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.
Yes, this is a critical part in this study. The slope is considered at yield in order to investigate only dynamic variations and not static effects (as already discussed in response to the commentary on line 131). This is done so that the results of the models are not dependent on the intrinsic mechanic and topographic properties of the slope.

- **We have clarify that in the manuscript. A discussion about the reason why we consider a slope at yield has also been added in the description of the failure mechanism lines 92-94.**

Line 183: Don't need the figure description in parenthesis.

Indeed, this was a mistake and should not have appear here.

Line 185: "an 86.4 mm cumulated rainfall" -> "86.4 mm of accumulated rainfall"

Done

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.

We fully agree with this remark. The characteristic time tc is not really appropriate to these comparisons. It represents the minimum time at which a strong pore pressure occurs at the depth z, which is why it is compared to the maximum response time. However, Handwerger et al. (2013) showed it corresponds to "the time 48% of the surface forcing is felt at a given depth". It still gives an idea of the timing of the diffusion. The definition and a discussion about tc is provided lines 221-224.

Line 200: "slop" -> "slope"

Done.

Line 214: Still unclear exactly how the water table rise during event is incorporated into your model.

This is the same issue as addressed in the general comment #1 and the comments concerning lines 131 and 135. The description of the models is clarified.

Results – Application:

Line 247: "east of Taiwan" -> "eastern Taiwan" or "the east of Taiwan"

Done

Line 251: "inferior to" -> less than

Done

Line 255: "contrasted" -> contrasting

Done

Line 257: remove "has"

Done

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?

The hillslope length L, in the equations presented by Townley (1995) does not have an influence on the form of the static water table. Only the position along the hillslope – namely the parameter x/L – impacts the water table response. The transient water table variation would keep the same form but see its response time changing accordingly to the diffusion characteristic time over the full hillslope length L^2/D. The hillslope length has been set as 500 m as it seems to represent hillslopes in Taiwan quite well (Fig. 8 (a)). We now discuss the scale of the hillslope lines 525-529.

- Is there evidence in the literature or in published well/piezometer data that hillslopes fully saturate

during typhoons?

To our knowledge, no study or data monitoring water table of hillslopes undergoing typhoons with high spatial and temporal resolution exist. But this is

a very important question and we are exploring whether sentinel satellites can define such behaviour.

- Provide the equation for the infinite slope model used for comparison

The equation of diffusion of pore pressure for the infinite slope model is the same as the one used in the finite hillslope model (equations 7 and 8), as specified in the manuscript. It does not seem necessary to provide the equation a second time.

- Can you say anything from your model about which storms cause landslides and which ones don't?

The model shows the greatest rainfall-induced pore pressure are achieved when weather events strike hillslopes with a deep water table level, which allow for large water table variations. The atmospheric effect is more pronounced when the drop of atmospheric pressure is rapid. Therefore, according to the model, a sudden and violent storm occurring after a drought period is the most prone to trigger a landslide. On the other hand, a storm striking a fully saturated hillslope with a gradual drop of atmospheric pressure is less likely to cause slope failure. However, this model does not take the static components of the safety factor into account, nor the seepage forces, and these might cause a fully saturated hillslope to fail.

Line 278: you say "amount of rainfall" which to me implies rainfall depths, but rates are given. I would make these agree.

Values given are indeed rates. They correspond to the mean rate during the past 6 months before the typhoons. The sentence has been corrected.

Line 290: "caps off" colloquial language, consider replacing

Done

Line 296: "in function of the event" what does this mean?

We have rephrased as: "depending on the event"

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

Done

Discussion:

Line 314: "models limitations" -> "model limitations"

Done

Line 315: "considered in this study consider" rephrase

Done

Line 355: "has been" -> was

Done

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.

Hydraulic diffusivity in Iverson's model is indeed the maximum hydraulic diffusivity, and is the diffusivity considered along this paper. This is added in the description of the rainfall-induced diffusion model (Sec. 2.3), line 160.

Line 385: Can you provide some more physical insight here on why diffusivity affects these in opposite ways?

The impact on diffusivity on each effect is developed and explained just above, and it doesn't seem to require any modifications in that way.

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)
This is a very good question also raised by the other reviewer. The intensity of seepage has not been modelled in this study, which is focused on the two effects of Psi_rain and Psi_air'. However, the atmospheric effects will never exceed the atmospheric pressure change, which is unlikely to change more than 5 kPa. The seepage is function of the flow of groundwater, and might surpass these values.

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?

Yes, the response to atmospheric effects is instantaneous because of the load transfer through the skeleton, but fades slowly by diffusion. The response to the gate function at a low diffusivity (D=10e-6 m²/s) is almost identical to the gate function, because little diffusion occurred during the forcing.

Lines 406-408: How does this finding compare with literature? Do we see landslides occurring in these locations?

This statement was referring to the model's outcome, not to actual data. No studies were found reporting landslides locations under these conditions.

Line 431: "dominants" -> dominant

Done

Line 443, 449: "repartition" Partitioning? Check word choice.

The word has been changed to "distribution".

Line 450: "typhon induced" -> "typhoon-induced"

Done

Line 478: weeks or months after the rain event – has this been observed? I think a reference would

strengthen this argument.

This has been reported in the context of long-term forcing in slow-moving landslides (Iverson and Major, 1987), but not observed for typhoons.

Line 489: "amount of cumulated rainfall" -> "depth of rainfall" or "accumulation of rainfall"

Done

Line 495: large variations in pore water pressure?

Yes, this was referring to the possibility of the water table in the case of Morakot to accommodate for large variations in water table level, and therefore in pore pressure. The sentence has been corrected (line 547).