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Comment on nhess-2021-140

Anonymous Referee #1

Referee comment on "Introducing SlideforMAP: a probabilistic finite slope approach for modelling shallow-landslide probability in forested situations" by Feiko Bernard van Zadelhoff et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-140-RC1, 2021

GENERAL COMMENT

Dear Editor, Dear Authors

I reviewed with interest this manuscript for possible publication in NEHSS journal. The work describes a comprehensive modeling tool to assess shallow landslides initiated by rainfall, in a probabilistic framework. The manuscript provides an interesting contribution in this field, although some aspects are strongly simplified, in contrast with others. The scientific quality is good, the reading is agile although the manuscript is overall a bit long and often dispersive. The literature review can be improved with additional appropriate references of strictly related works. The description of the climate forcing that initiates (or not) the landslide events requires significant improvement.

To my opinion, the work can be published after some important clarifications and revisions.

SPECIFIC COMMENTS

Please read in the following my specific observations.

1. Literature review (introduction/discussion).

The discussion on the impacts and costs of the natural hazards, from the point of view of insurance institutes, is interesting. However, in general, I found the introduction a bit dispersive, lacking in some aspects. The work of Dietrich and Montgomery, 1994, (SHALSTAB) represents the pioneering work within this approach, and it has been followed by many other deterministic work that gave different contributions in improving the hydrological modeling at support for the shallow landslide, such as the cited Iverson 2000, and, additionally, Rosso et al., 2006; Claessens et al., 2007, Arnone et al., 2011; Lepore et al., 2013; Simoni et al., 2008, Baum et al., 2002 (TRIGRS), Montrasio et al., (2011) (SLIP) (among the others).

With regard to the effect of vegetation, the aspects related to the hydrological effects should be at least discussed, which can sometime be even more significant than the mechanical ones (Feng et al., 2020).

An interesting review are by Chae et al., 2017, Gasser et al., 2019 and the just published by Masi et al., 2021. (SEE REFERENCES LIST)

2. Definition of the stability problem.

I found the definition of the problem of stability estimation (section 2.2, Figure 2) a bit misleading. It is not clear the definition of the volume of soil to which forces are applied. In the method of the limit equilibrium, under the hypothesis that the width of the landslide is sufficiently large so that the deformations are in the plane parallel to the soil thickness H_{soil} (i.e. perpendicular to the elliptic landslide in figure 2), forces are assessed by considering a 'slice' of soil with unit width (in the direction parallel to the elliptic landslide plane). Figure 2 is confusing and the planes of forces are not well drawn. The limit equilibrium method (and infinite slope model) is based on the hypothesis of large and elongated element with respect to the soil thickness, so that a unit in width element can be considered. Also, P_{water} is not indicated in the Figure 2. According to the definition in the manuscript, R_{lat} and F_{res} apply on different planes.

I suggest to modify in a 3D perspective the Figure 2 and specify the hypothesis/assumptions.

3. Hydrology and precipitation.

Here is my main comment. The proposed modeling framework addresses shallow landslides that are initiated by rainfall, which is the triggering factor. The approach used (based on TOPMODEL) is extremely simplified because based on steady state conditions, which do not take into account the transient of the hydrological processes (Chae et al., 2017). The authors declare the limitation of the approach used in the discussion section, but this should be clearly stated soon in the methodology. As correctly written by the

author, the stationarity is supposed to be reached within the hour of timestep. Clearly, this cannot be largely verified.

That said I arise two more critical issues that are not mentioned by the authors:

- under unsaturated conditions, soil (especially fine and clayey soils) exerts a strong water uptake effect due to suction, which leads to an apparent 'hydrological' cohesion. This represent a further limitation of the Montgomery and Dietrich approach that the authors should mention (see, works mentioned in Chae et al., 2017, e.g. Lepore et al., 2013).
- In the description of the model application (section 3.4.2) it is not clear how rainfall initiating events are selected. If I understood well, only events of 1hour duration are selected, whose intensity is identified from the Depth-Duration-Frequency (DDF) curve at different return periods (i.e. from 10 to 100 years). Therefore, I guess 10 events of 1 hours are simulated. Is that correct? If so, it should be explained and justified the reason of analyzing events of only 1 hours, which cannot be 'critical' for landslide initiation. Authors should deeply clarify this part in the manuscript, explain the methodology used to define the events, and report the parameters of the DDF curves.

4. Data inventory

The proposed methodology used to characterize the hypothetical landslides (extent) is strictly dependent on the data inventory (section 2.3), as also stated somewhere by the authors. However, it is important that the observed landslides used to characterize the model are of the same type, according to the hypothesis of the stability model used and all triggered by rainfall. Is it so? Please specify.

5. Calibration/sensitivity analysis

With regard to the best set of parameters, my question is: are the found parameters consistent and realistic? For example, I argue the choice of including the precipitation intensity as calibration parameter. As discussed in the previous comment, rainfall represents the triggering forcing and it is a dynamic variable. Ideally, we should know the precipitation intensity associated to each observed landslide. Otherwise, if used as parameter, it seems that the model is tuned ad hoc just to reproduce the past events. If so, which could be its utility?

Additionally, it would be interesting to see the AUC curves for the calibrated and the best model combinations. The shape of the curve also tells about the model performance.

Then, to my opinion, sensitivity analysis should go before the model calibration. Normally, calibration is done on parameters that are more sensitive. I understand figure 7 and 8, but not sure this is the most efficient way to verify the sensitivity of the parameters. I am curious to see how, for example, the landslide probability varies with the chance of parameters values. This test could be shown with the least and most sensitive parameters.

6. Results

The result of high m_f and low m_c is quite obvious; as the author clearly say in the discussion, and as found by other past works, in the end only few parameters really affect the process: the geometry of the slope (i.e. the soil thickness), the mechanical properties (i.e. friction angle) and the characteristic of the trigger (i.e. precipitation) whose effects are controlled by the soil transmissivity.

With regard to the vegetation: different vegetation scenarios are analyzed (and this is fine). which is the real configuration? Which is the ultimate target of the simulations?

7. General

I suggest to clearly state which is the ultimate main target of the model. Can we use it as forecast tool in an early warning system? If so, in which way? My impression is that it is too constrained to the calibration parameters, which, in some cases, may lose their physical meaning.

TECHNICAL CORRECTIONS

Please see in the following technical observations:

- Abstract: I strongly recommend to reduce the abstract to make it more concise.
- L3: I do not completely agree with this sentence given that there are of works that take into account the effect of vegetation, although from different perspective such as the hydrological one, together with the mechanical one. Please remove this sentence from the abstract, where you do not have room to discuss.
- L72-L80- I suggest to synthesize.
- Figure 1: it is useful and appropriate. However, consider to improve it to make it clearer. Not clear from where to start. "extract mean value for each landslide": do you mean hypothetical landslide? Emphasize the 'append' box where everything converges. Avoid text outside from the box. Also, I suggest to use the symbol used in the section

(instead of the description). For example: definition of rho_ls; it would improve the correspondence with, for example, section 2.3.

- Line 417: Not clear which single rainfall event you refer to. I understand that the database include landslide triggered by different storms across the years.
- Lines 378-379: it is not really clear the procedure. Please try to write more clearly.
- Lines 385-386: I understand the reference, but please give an explanation also here, based on your results.
- Section 2.7: how did you define the threshold from daily to hourly??

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