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Interactive comment

Interactive comment on "Rainfall-induced shallow landslides and soil wetness: comparison of physically-based and probabilistic predictions" *by* Elena Leonarduzzi et al.

Anonymous Referee #2

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The paper investigates the use of (very) large scale hydrological modeling to improve the prediction of shallow landslide occurrence throughout Switzerland, compared to the typical approach based on the statistical analysis of triggering precipitation alone. This is a quite actual topic, which can be of interest for the readership of HESS, as most landslide early warning systems still rely only on precipitation information, while there is physical and operational evidence that including hydrologic information may be useful in many cases.

The paper is well organized and the English language pretty good, although the choice of the scatter plots adopted for presenting the results may result a bit awkward (al-

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Discussion paper

though quite synoptic, which is a good point). However, I find some issues in the adopted modeling approaches, which somehow affect also the results and the conclusions, so I believe that major revisions are needed before reevaluating the manuscript for possible publication.

Specifically, all the paper deals with the comparison of a “physically-based” hydrological model, run over a coarse spatial grid and coupled with a simplified slope equilibrium equation based on the infinite slope hypothesis so to end with an assessment of a safety factor value at any point of the grid, with a purely probabilistic evaluation of the coupled effects of slope conditions in landslide and non-landslide days, carried out by estimating soil degree of saturation with a conceptual hydrological model run at a much finer spatial scale. The obtained results indicate that the “physically-based” approach is largely outperformed by the probabilistic, and the discussion ascribes this outcome mostly to issues related to the coarse resolution (i.e. wrong local estimates of soil depth, slope inclination, soil mechanical properties, slope hydraulic response to precipitations, and so on). All these discussion points are clearly valid and acceptable, but I believe that the Authors should more deeply describe, discuss and comment the limitations of the model that they consider as “physically-based”.

I understand that the Authors probably mean that with such a modeling approach they assess landslide occurrence with an equilibrium equation, that is the application of a physical principle. However, although no detail is provided about the characteristics of the infiltration model which provides the water table depth for the application of the equilibrium equation, I have the feeling that it may be not completely physically based. In fact, while dealing with shallow landslides, which occur in initially unsaturated soil covers (as the Authors indeed notice in the Discussion section), it is assumed that the infiltration process results in the building of a water table at some depth in the soil, which is not necessarily the case (it strongly depends on the assumed boundary condition at the base of the soil cover), and which seems more a conceptualization of the effects of infiltration, rather than the result of a physically-based model of rainfall infiltration process. The adopted expression (1) of the factor of safety (and the obtained results, as well as the discussion about them) seem to be deeply affected by this conceptualization.

1. When the soil is considered dry and cohesionless (h=0 in equation (1)), FoS reduces to tan(fi)/tan(beta), which implies that soil depth is ineffective and that everything depends on the quality of your topographic map (beta varies much more than fi, which not surprisingly remains always not far from 30° for the kind of soils you may have in mountain environment).

2. When root-cohesion is introduced (by the way, another conceptualization), considering dry slopes with inclination larger than 30°, you can easily see that it mostly results FoS>tan(fi)/tan(beta)+0.1c, so that even with the smallest hypothesized cohesion (5 kPa), FoS can be smaller than one only for slopes more inclined than 50°.

3. When saturated soil cover (h=d) is considered without root cohesion, it is FoS=(g-gw)/g*tan(fi)/tan(beta), that, with the values of g that you assume for the soil (seemingly between 1.2 and 1.6, with gwïĄĂ1) leads to stability possible only for inclinations smaller than 12°, independent of soil depth.

4. If we introduce root cohesion when h=d, you get again that for slopes with inclination above 30° some 0.1c is summed up to the previous expression of FoS, that is FoSïĄĂ0.1c+(g-gw)/g*tan(fi)/tan(beta), so that only when cohesion is the smallest (c=5 kPa) you may get some slope inclinations for which stability depends on the value of the water table h.

This given, my overall impression is that all the results from the "physically-based" modeling, namely all the considerations about (un)conditional (un)stable situations, and their comparison with the landslide inventory are strongly affected by the weakness of the model, before than by the issues related to the coarse modeling grid. I mention a few points that I believe are worth some discussion: (i) what is the meaning of gamma in equation (1)? This value should change according to soil saturation, and the assumed values between 12 and 16 kN/m3 seem rather to refer to some average field condition (this certainly has an effect on the predicted values of FoS); (ii) to what extent the assumption of the building of a water table is acceptable and consistent with the geomorphological characteristics of the studied alpine slopes (i.e. type of soil and type of bedrock)? (iii) is groundwater table (likely much deeper than the shallow soil covers of interest for the study, as the Authors themselves observe at lines 270-271) an appropriate variable to be chosen for the purpose of this study about shallow landslides? (iv) I guess that TerrSysMP model offers also soil moisture data, so why did you choose groundwater table for your analyses?

Concluding on this point, I still believe that the attempt to exploit the information available from a model like TerrSysMP for the sake of predicting landslides is a valuable task, and that it merits to be investigated. But it seems to me that this could be made with more care than it is in this study.

On the other hand, there is the conceptual hydrologic model and the use of estimated soil moisture with a probabilistic approach to improve landslide assessment carried out with empirical precipitation thresholds. While this part is more straightforward, there is still a major point that should be clarified. Your aim is to investigate the potential of soil moisture prior the onset of triggering rainfall to improve empirical thresholds. Despite this, from figures 6 and 7 it seems that you never consider this information, as only saturation on the day of the landslide, maximum or mean saturation during an event, and general statistics of the saturation in the cells are calculated. The discussion of the moisture conditions prior the event is limited to graphs of fig. 8, considering mean saturation for 5-60 days long periods preceding rainfall events. Some discussion of the graphs would be worth. For instance: the 5 and 10 days averages seem to be the best choice to correct false alarms (red line well below the others); long events (6 days) seem to lose memory of the effects of initial conditions on missed alarms (all yellow lines drop down for 6 days, while they are above all other lines for shorter event durations). Instead, in the paper only the brief sentence at lines 303-304 is dedicated to the possibility of building hydrometeorological thresholds, which are just said to be uncapable of improving the performance of precipitation thresholds without any information. I think that much more discussion and data should be presented to the reader, as the effects of prior soil moisture is all in all the focus of the paper.

In addition to these two major issues, you can find some remarks and comments as annotations in the attached file.

Please also note the supplement to this comment:
https://hess.copernicus.org/preprints/hess-2020-624/hess-2020-624-RC2-supplement.pdf

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