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Comment on "Simulated or measured soil moisture: Which one is adding more value to regional landslide early warning?" by Wicki et al.

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Referee comment on "Simulated or measured soil moisture: which one is adding more value to regional landslide early warning?" by Adrian Wicki et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-133-RC2, 2021

The paper deals with a hot topic for the researchers working on landslide hazard management, i.e. the potential improvement of (shallow) landslide predictive models at regional scale, offered by adding soil moisture information to the commonly used precipitation data. The idea of comparing the predictive performance improvement deriving from modelled or from measured soil moisture is surely of interest for some of the readership of HESS.

The paper is clearly written and organized, the data and methods are adequately described, and much valuable information is made available to scientists dealing with landslide hydrology. Overall, my judgement of the paper is positive, with just few minor points that could help further improving it (you can find them as comments in the attached annotated file).

However, I would like to share some points of discussion with the authors, leaving to them the decision on if, and to what extent, they could find some space in the revised paper.

To judge about the added value of soil moisture information for landslide prediction (and for the comparison between two sources of information, measurements and modelling), there are two tasks: assessing the long-term water balance of the slopes, which is mainly controlled by what happens at the boundaries (overland runoff generation, evapotranspiration, deep leakage at the bottom of the soil cover); simulating what happens during rapid rainfall or snowmelt infiltration events, for which boundaries are expected to be less important compared to soil hydraulic properties. The first task has to do with the antecedent conditions, which may predispose the slopes to failure; the second directly with the triggering of landslides.

The results clearly indicate that modelling the long-term processes affecting the slopes (i.e. water balance) can be quite different than modelling the dynamics of the short-term infiltration of rainfall or snowmelt. In fact, the adopted 1D physically based model satisfactorily reproduces slope response during short periods with several infiltration events (fig. 3, although the soil parameters used in these simulations are not specified), but in the long run there is some systematic mismatch between simulated and measured soil moisture (fig. 5, looking at which I would be careful with the conclusion that there is not "apparent trend" in simulated soil moisture). The authors recognize that there is an issue in the modelling of evapotranspiration, which can be underestimated of even up to 200 mm per year (fig. 10), but surprisingly, they do not elaborate on this in their attempts of simulation. Instead, all the attention is focused on the effects of changing soil hydraulic parameters and on the bottom boundary condition.

All the variables tested to improve landslide predictions (table 2) refer to the water balance of the soil cover (i.e. mean cover saturation, infiltration flux at the upper boundary, bottom boundary condition), and so it is not surprising that the shape parameters of van Genuchten retention model do not affect the predictions that much. Given the tested variables, only Ksat is important, and this is probably the reason why the coarse-grained homogeneous soil profile works well, as this profile is associated with the highest value of Ksat (and thus infiltration), so that the saturation variations are mostly sensitive to rainfall are the highest. I think this is also the reason why it does not seem that including soil moisture changes the predictive performance so much compared to using rainfall information alone: the possible effects of antecedent conditions on infiltration dynamics are lost, as the model fails reproducing the water balance, and so soil saturation trend simply follows precipitation trend.

About the bottom boundary condition, the most valuable for landslide predictions are the ones which maximize the drainage. I wonder if this is a way to compensate the underestimation of evapotranspiration: in Alpine environment (high altitude, rocky bedrock, steep slopes etc…), is it plausible that there is a groundwater table, few meters below the soil cover, affecting it? It seems to me that this bottom boundary conditions becomes more conceptual than physically based.

All in all, it is not surprising that this kind of soil moisture modelling does not add useful information for landslide prediction about antecedent conditions, being outperformed even by sparse field measurements, and that so little improvement is provided by the modelling of infiltration event dynamics (compared to precipitation alone).

Given all these considerations, some questions arise: is it worth using such a sophisticated unsaturated soil model, with so many equations and so many parameters difficult to set (table A1), when only the water balance of the soil cover is needed? Would the result of the comparison between modelled and measured soil moisture give a different result if a simpler modelling approach was used? Is it possible to conclude that the aims listed at lines 85-89 have been achieved?

Indeed, after the presented detailed study, with a rich dataset (landslides, soil properties, meteorological input, soil moisture measurements) and with a complex modelling exercise (exploring also the effects of different parametrizations), little conclusions are drawn: soil moisture measurements seem to allow a better assessment of antecedent conditions, but their use is limited by spatial resolution; soil moisture modelling requires different parametrization to provide better results. In view of this, maybe the aims of the study, and the title as well, could be reformulated in a less ambitious way.

I hope that my considerations can be of some help for the authors, for this paper or for future further elaborations of their data.

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Please also note the supplement to this comment:
https://hess.copernicus.org/preprints/hess-2021-133/hess-2021-133-RC2-supplement.pdf