Review of the manuscript Modelling of post-monsoon drying in Nepal: implications for landslide hazard by Maximillian Van Wyk de Vries et al.

Summary

The Authors present and discuss a study about soil moisture monitoring and modelling. The analysis is performed based on a dataset of point-scale soil moisture observations collected at three locations and at three depths in Nepal. The land-surface model JULES has been calibrated based on two different criteria. The Authors further discussed the results in the light of landslide predictions.

General comment

I acknowledge that the Authors made a good effort to shape a scientific manuscript out of the collected data-set and modelling exercises. Despite this effort, however, I regret to say that I personally believe that the scientific value is weak in many aspects as detailed below. While trying to be constructive with my critiques, I feel that a completely different manuscript should be prepared, and the current manuscript should not be foreseen.

Specific comments

[1] Landslide topic not addressed.

The Authors promote the landslide prediction as motivation of the study. While I agree that soil moisture is an important trigger in many conditions, by reading this manuscript I believe that this is not the most relevant factor for landslide prediction in this specific environment. Moreover, at the end, the landslide topic is not addressed at all. Specifically, we are in a monsoon area with wet and dry period. So, discussion about when landslides mainly occurred should be reported. The Authors show how precipitation based on different products strongly varies and affect soil moisture prediction (L311). So, I'm expecting that getting precipitation right is more relevant and calibrating the model to the observed soil moisture with wrong forces could be misleading, i.e., compensating the error by calibration (L326). Finally, how much the use of observations and of the calibrated model improves landslides prediction is not at all implemented. Strictly speaking, why not use a landslide/erosion model instead of JULES if the objective is to cover the landslide topic?

[2] Soil moisture network weak

Despite I believe that installation and maintenance of soil moisture sensors at these sites is challenging, it should be acknowledged that the data-set is quite limited. Without pretending the installation of other sensors, the Authors seems to have data from sep-22 till now. Surprisingly they use and show only data of 2022. Why? The analysis should be at least extended to the entire 2023 having then two drying seasons. Moreover, point-scale sensors are used, and no discussion is reported about their representativeness. It could be likely the case that installing the sensors one meter apart could show a completely different behavior. Pretending one profile of point-scale sensor to be a ground truth when driving forces are at 5-10 km resolution is questionable. Many studies working on spatial mismatches have been conducted and should be considered for better shaping the study and extending the discussion.

[3] modelling exercise

The model and the modelling framework is not new, to some extent confuse and it does not provide any new insights on the use and capability of these models, especially for supporting landslides predictions. More specifically, a spatial mismatch between point-scale sensors and modelling is critical and is not addressed. The use of JULES for addressing landslide prediction is misleading. Why using this model? It is also not clear to me why the Authors need the exponential decay function. Could you not directly calibrate the model parameters by looking at the dry down period? I do not expect to see different results than first fitting the decay function. Why even testing that if at the end the Authors argue that it is not a good approach to follow (L301)? The use of different precipitation sources seems to disappear at a certain point, i.e., 3.4 evaluation of parameter distribution is discussed but is not clear which precipitation product is used. Comparison between the distributions obtained based on different precipitation products could shed light on the importance of the driving factors more than soil moisture. The use of measured precipitation should also be foreseen. Evaluation across stations is a good exercise. I would strongly suggest also testing during another period, i.e., 2023? Discussion across precipitation products is missing. RMSE suddenly appears at L285. The exercise shown at figure 9 is not clear. Which best parameter sets is used? From site 1, 2 or 3? All? Why selecting two precipitation products? Overall, the modelling approach fails to quantify if the uncertainty in driving data is more relevant than uncertainty in model parameters and if a model can better predict landslides if soil moisture observations are used.

[4] Clarity weak

The manuscript is in general well written, but I found some passages difficult to understand and many parts where description should be improved. E.g.,

The text at L14-24 is difficult to grasp, it focuses on Nepal and some few general statements anticipating the objectives of the manuscript. The more general introduction seems to start at L25 where the general topic is introduced. The above text might be better integrated later.

L73. I do not think there is a clear definition of how many sensors make a network, but I was expecting more than 3 locations with 3 depths for a network.

Figure 2. Description in the legend caption should point to (a) (b) etc. I do not understand why cumulative precipitations are plotted but the values decrease. If it is a cumulative precipitation I should see monotonic increase. These would also better capture the difference between the precipitation products. What is the meaning here to plot field if they start from September? Why not also using 2023?

L125. How is the soil discretized in the model? Do we have three soil layers according to the soil moisture sensor depth?

L178. If no precipitation occurs during post-monsoon, is there any landslide risk over this period? It is not clear from the manuscript why modelling this behavior is important underpinning the scientific value of this study.

L195. Assessing the value of this study by looking at how much the prediction of soil moisture dynamics increases from the uncalibrated model is misleading. If the objective is to understand landslide predictions, the Authors should show how landslide prediction improves by improving soil moisture modelling.

Figure 4 shows, as far as I understand, only modelled data and high spread of the modelled soil moisture due the different precipitations products. This could support the conclusions that uncertainty in precipitation might be more relevant than soil moisture for landslide predictions.

Figure 5 has too many lines, and it is difficult to read, in my opinion.

Figure 6 is not discussed in the main text. Only cited at L257. Is it then useful? What is default value here?

L275. As far as I have understood the Authors only show the use of the best set of parameters from Site 3 and apply to site 1 (Figure 8). Results over the other combinations are not shown but are relevant. In figure 8 I would have also expected to see a comparison to the KGE distribution obtained based on the best ensemble member from site 1. Legend caption of Figure 8 says that ensemble members were driven using CHIRPS but the plot d says also MSWX driving data.