

Dear authors,

Thanks for your contribution on a hot topic regarding hydrology in mountainous and more generally sloping areas, which can indeed be of interest in many perspectives (risk management, agriculture/forestry, water supply).

Summary of the preprint

The goal of this study is to determine, amongst the parameters (rainfall, groundwater water head, and two soil water content) of an already calibrated 1D physical model, which are the main drivers (in the form of the most important antecedent conditions) of the soil water content. Indeed, this variable has been identified as critical in the occurrence of landslides.

To do so, the field data is enriched with synthetic data produced by the 1D physical model, fed by stochastically-generated rainfall events. This augmented data pool is analysed through a machine learning method, combining Random Forest to assess the variable importance of each antecedent parameter conditions with regard to the soil water content output, and K-means clustering to classify the results.

Provided that the output is normalized (ratio between the soil water content and the rainfall event depth), the analysis shows a quite balanced importance of the antecedents (with a slight predominance of the soil water content condition). The triplet {rainfall, water content of the first meter of soil, groundwater head} is the most predictive of the soil water content evolution.

Finally, the classification reveals seasonal behaviors consistent with previous studies and the field observations.

General comments

- The article is well written, at the exception of some phrasing and vocabulary issues. Especially, I reckon that "soil cover" is misused as it is a synonym of "land use" and refers to vegetation or anything that covers the soil. In the article, it is used as a synonym of "soil" or more precisely "topsoil". Please correct this, as the land cover is not a variable nor a parameter in your model. Also, "slope response" is a bit spurious phrasing as what you're investigating is the soil water content response in a sloping context, and not that of the slope itself. I would suggest to use soil instead of slope and maybe rethink the title accordingly to avoid confusion (e.g. Understanding hydrologic controls of sloping soils response to precipitation...).

- The structure is good and most of the sections (or sub-sections) are clear (the description of the site and the field surveys, the clustering analysis etc.).

- Nevertheless, the aim of the study should be more precisely explained at the beginning. Is it oriented toward a seasonal analysis (what it seems, regarding the Results section and some mentions beforehand) or a crisis-warning model (which is also mentioned both in the Introduction and Conclusion, and would be more consistent with the time resolution of the data)? In my opinion, it can't do both. Depending on

the objective, the design choices for the synthetic data (time resolution, separation criterion) would be quite different.

- A figure (a workflow for instance) could be of help to summarize the method.

- Some information on the hardware used and the computation time would be welcome, especially by comparing it with another approach (sensitivity analysis).

Specific comments

- The method is based on a physical model for simulate the variable relationships on one hand (calibration), and produce the synthetic data on another hand (datapool augmentation). Could you not reach the same goal (or at least compare your results) with a sensitivity analysis of the 1D model with respect to the initial conditions (as you're focusing on the antecedents). In that regard, some supplementary elements supporting the choice of the method would be welcome.

- Concerning the synthetic rainfall generation, I assume that the separation criterion has a huge impact on the outcome of your methodology, especially if you aim at identifying the dominant parameter during extreme events (when the risk is the highest). Is there not a contradiction in choosing a 24h separation criterion (i.e. the time for the topsoil to drain almost completely) when the goal of the study is to assess the importance of the prior state of the soil? Could you elaborate a bit on the choice of this separation criterion (is it only based on the physics of the phenomenon, or is it constrained by the number of events you can bring into the machine-learning and still be computationally-reasonable)?

- Did you try to assess the sensitivity of your method to the value of this separation criterion?

- On a related matter, by producing very diverse events in terms of duration and intensity, are you not risking to blur (to average) the relative importance of each parameter that might differ depending of the type of event? Said otherwise, if the goal is to find the sensitivity of your model to the prior conditions when an extreme event occurs, why not choose only extreme events for the analysis? Or for seasonality, why not splitting the rainfall chronicles beforehand and therefore acquire more specific relative importance of the antecedents?

- I understood that the previous studies were focusing on analysing and predicting the seasonal changes. Now, in this study, is this time-scale still relevant? I thought that the hour-time resolution was aiming at refining a model and a monitoring network more crisis-oriented (informing on the risk of a landslide to occur for example).

- Line 54: please, explicit what you mean by "long timescales".

- Line 98: Would not it be "at the contact between soil and bedrock" as soil cover is the description of what covers the surface of the soil? (Same remark for all "soil cover" occurrences)

- Lines 98-104: That refers to epikarst. You should maybe cite supplementary studies and modelling approaches outside your workgroup (Perrin et al., 2003; Hartmann et al., 2014; Dal Soglio et al., 2020 for instance).

- Line 117: "identified" appears a bit confusing here, and can be understood only once the Method

section has been read. I would suggest "sorted" or "chosen". I also suggest to rephrase the whole sentence, whose syntax seems wrong to me.

- Line 321: Note that a purely 1D approach does not account for flow accumulation and possible secondary infiltration (runoff that infiltrate during its course downstream). That could overestimate the influence of the groundwater level by underestimating the amount of infiltration.

- Lines 373-376 and 491-493: How do you support this direct relationship between the water level in the aquifer and the one in the stream? No dedicated parameter appears in the mathematical description of the model. Moreover, a direct proportionality might not be true, especially during extreme events (droughts and floods).

- Once again, I think that the Conclusion and the contribution of this study would be more valued if it were compared to another approach (local sensitivity analysis for instance).

Technical corrections

- In text reference ordering (reference grouped in the same parenthesis) should follow a consistent pattern (chronologically I would suggest, see HESS editors to make sure).

- Replace "soil cover" by "soil" or "topsoil" all along the article if you're agreeing with my previous statement on the meaning of these terms.

- Replace "slope" by "sloping soil" or simply "soil" as needed.

- Line 136: "is" is missing between "results" and "quite variable".

- Line 439: "developed" is not appropriate here. Use "performed" or "carried out".

- Line 477: "is" is missing between "soil storage" and "less connected". Maybe you should rephrase this sentence.

- Figures 5 and 6: the unit for the h_a (groundwater level) is mm. Shouldn't it be m? What is the base level?