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.

We are happy about the overall positive judgment and appreciate the constructive comments by the reviewer that we addressed below. Further, we commented the remarks from the supplement at the end of this document.

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.

Roberto Greco

We acknowledge that the model is worse at representing the long-term water balance compared to characterizing infiltration event conditions. We discussed potential reasons for this and attributed the underrepresentation of the seasonal soil moisture cycle mainly to the definition of a common parametrization of the boundary conditions (lines 435 – 455). The definition of common boundary conditions was needed in order to able to apply the model at locations where no site-specific calibration was possible. The motivation behind applying the model at such locations was to test the use of modeled soil moisture data to complement a soil monitoring system.

The reviewer argued that some part of this water balance misrepresentation may originate from an underestimation of evapotranspiration as some sites clearly deviate from the validation function shown in Fig. 10. It has to be noted that the validation function was developed for flat open-land grassland locations in Switzerland. In contrast, some of the meteorological sites are partially shaded due to topography or located on an oriented slope. Hence, the validation data serves only as a rough point of reference for a specific site elevation and we believe that the evapotranspiration is within reasonable ranges. We will explain the limits of the validation data in more detail in the discussion part.

Given all the simplifications (soil hydrological properties, homogeneous upper and lower boundary conditions, no lateral flow considered) we agree that a simpler model might as well produce similar results. In contrast, it would also be interesting to assess the benefit of applying a site- or region-specific parametrization or of using a model that also considers lateral water flow. While such investigations are out of scope of this study, the dataset used here may serve as a basis for further analyses in that direction. In the revised paper, we will explicate more the choice of the model and put it into a broader context in the discussion part.

Finally, the question was raised whether all the aims listed in the introduction were reached. We believe this is the case. With regards to the above discussed points, however, the aims may have been formulated too broadly. We will reformulate them and explicitly narrow them down to the use of a 1D soil hydrological model. Along the same lines, the title may be formulated too generally for the analysis conducted and hence we will change it to “Application of a 1D soil moisture model for regional landslide early warning: Added value and limitations.”

Answers to comments in the supplement:

- P. 6, line 191: We will add information about the climatology in Switzerland in the revised manuscript.
- P. 9, line 267: The simulated saturation time series were based on a Coup-Model set-up with groundwater and using soil properties from SoilGrids. We will specify this in Fig. 3 in the revised manuscript.
- P. 9, line 274: The triggering probability remains low for all triggering events. This is due to the imbalanced dataset (very few triggering events as opposed to many non-triggering events) and commonly reported for logistic regression models for such data sets (also referred to as rare events data; King and Zeng, 2001). Nevertheless, the relative difference between triggering and non-triggering events is large enough to be detected in the ROC analysis. We will elaborate on this in the revised manuscript.
- P. 11, line 329: We have plotted the measured soil moisture time series in as well and we have added a trend line to all plots (Figure S1). No clear trend is visible for the modelled time series, whereas a decreasing trend is apparent for the near-surface layers in the measured time series. While this might be indicative for underrepresented drying out towards the end of the study period, it might also be the result of data quality issues of the measurements resulting in reduced homogeneity of the long-term soil moisture time series which were partially running for up to 10 years (e.g. due to compaction of the soil, enhanced root development around the sensors). In a future study with site-specific calibration, this could be studied in more detail e.g. by comparing these trends with nearby long-term ground water or runoff measurements. Further to that, individual time series have different lengths and thus the depth-integrated signal shown in the plot may be influenced by partial under- or over-representation due to the simplification of the soil hydrological properties. We will elaborate both points in more detail in the revised paper and we will add the new figure.
- We will correct the various grammatical errors highlighted.

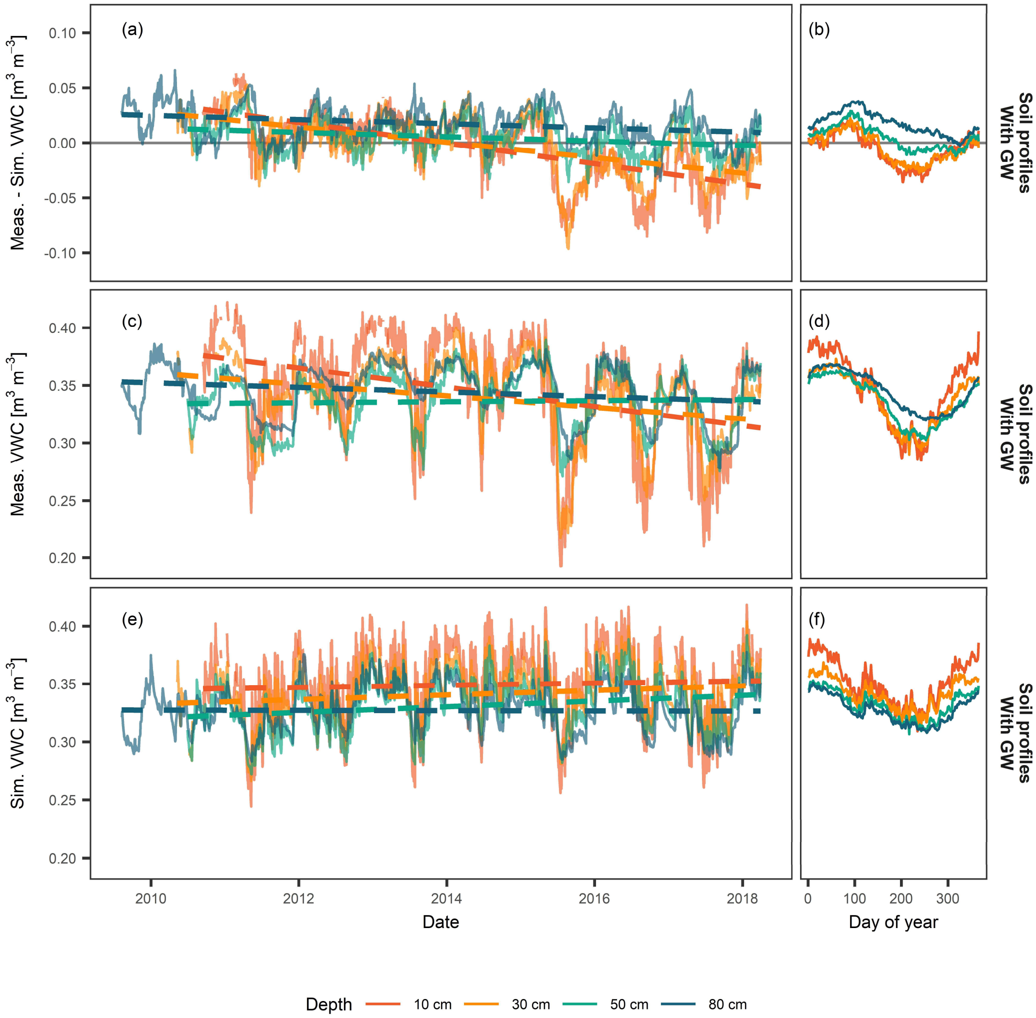

Figure S1 Temporal evolution and seasonal variation of mean daily residual VWC (a, b), i.e. deviation between simulated and observed soil water content, and mean daily measured (c, d) and simulated VWC (e, f) across all 14 reference sites by sensor depths (different colours) for a CoupModel set-up using soil hydrological properties derived from SoilGrids and a lower boundary condition with groundwater. Panels c and e include trend lines by sensor depth.

King, G. and Zeng, L.: Logistic Regression in Rare Events Data, Polit. Anal., 9(2), 137–163, doi:10.1093/oxfordjournals.pan.a004868, 2001.