Replies to Reviewer 1

OVERVIEW

The study investigates the use of modelled soil moisture data obtained from land surface modelling for the prediction of landslide occurrence. Specifically, three different versions of WRF model (three configurations for the land surface model scheme) are used for developing a soil moisture – based landslide threshold model in Emilia Ro- magna (Italy) in the period 2006-2015.

GENERAL COMMENTS

The paper is fairly well written and clear. The topic is surely of interest for the readership of "Hydrology and Earth System Sciences" journal. In recent years, the use of modelled and satellite soil moisture data are increasingly used for the prediction of landslides occurrence in space and time, and the study might represent an important contribution in this respect. However, in my opinion, some parts and aspects should be clarified before the publication.

I listed here the general comments also including their relevance:

1) MAJOR: The same authors have just published a similar paper on JSTARS over the same study area and using the same landslide catalogue for testing a soil moisture (and rainfall) threshold model. In the JSTARS paper, the authors have used satellite soil moisture data instead of modelled data. Firstly, the differences between the two studies should be clearly highlighted. Secondly, the comparison of the results obtained in the two studies should be carried out (the same 45 rainfall events are used for the ROC curve in the two studies). Is it better to use modelled or satellite soil moisture data for landslide prediction?

Reply: Agreed.

Firstly, in the introduction, some parts will be removed to reduce the similarity from the previous paper on explaining the existing research gaps. The shortcomings of the previous study will be added in the updated manuscript, to clearly explain the necessity and novelty of this study (i.e., the need of using high spatial (both horizontally and vertically) and temporal resolution soil moisture products for landslide application).

Secondly, as suggested by the reviewer, the best Euclidean distance d in the previous study of using satellite soil moisture is found as 0.51, while this study shows the best performance can reach 0.37 (by Noah-MP model at 10 cm depth). So based on this comparison, the WRF modelled soil moisture can provide better landslide prediction performance than the satellite (i.e., ESA CCI soil moisture product). We will include this comparison result in the updated manuscript.

2) MODERATE: In the introduction, a brief description of limitations of satellite soil moisture data is given. However, I have found some errors: 1) microwave observations have not the problem of cloud cover, 2) with Sentinel-1 we have 1 km resolution / 3 days soil moisture observations (operationally available under the Copernicus Land Monitoring Service). Therefore, currently there is large potential in using satellite observations for landslide prediction, it should be clearly acknowledged.

Reply: Agreed. We will update the information for satellite soil moisture estimation, and include the discussion of using Sentinel-1 satellite soil moisture retrievals for landslide application (e.g., its availability only over the recent years is a limiting factor to build reliable thresholds).

3) MAJOR: It is not clear which soil moisture value is used. Initial soil moisture, final soil moisture at the end of rainfall event, maximum soil moisture, mean soil moisture? It must be clarified. Moreover, it is well known that soil moisture is strictly related to rainfall, and I was wondering how accurate are the WRF simulated rainfall? A comparison between observed and simulated rainfall should be carried out to have a better understanding of the quality of WRF model in the study area.

Reply: In this study, the daily mean soil moisture is used. The reason for not using the antecedent soil moisture condition plus rainfall data on the day is because the purpose of this study is to explore the relationship between different WRF simulated soil moisture and landslides solely. In general, soil moisture is a predisposing factor for slope instability, while rainfall is the triggering factor. The same rainfall may trigger or not a landslide depending on the soil moisture content at the time of the rainfall event. The mean soil moisture on the day of the landslide implicitly account for both the initial soil moisture and the effective rainfall absorbed by the ground, and can be a robust indicator of the hydrological condition of the slope.

We thank the reviewer for suggesting on evaluating WRF model through its rainfall performance, and we will add this work as suggested in the updated manuscript. Currently, the WRF estimated soil moisture is only evaluated through the single point in-situ measurement, which has posed concerns from the Reviewer 2. The added work will provide a useful indicator of the accuracy of the WRF estimated soil moisture.

4) MODERATE: It would be very relevant to perform a comparison with an approach based on rainfall threshold. Intensity-duration (or accumulated-duration) rainfall thresholds are largely used for landslide prediction. What is the accuracy of such an approach with respect to the one based on soil moisture proposed in the paper? This would add something new with respect to the JSTARS paper.

Reply: The purpose of this study is to preliminary assess the relationship between the WRF modelled soil moisture products and the landslide events. Currently as aforementioned the mean soil moisture is adopted which account for both the initial soil moisture and the effective rainfall absorbed by the ground. Only after the evaluation of the soil moisture product, rainfall information can then be used together with the antecedent soil moisture information for forecasting and monitoring purposes. Due to page limits, those work will be in our future studies, and at that point, a comprehensive comparison of the new method and the traditional rainfall-only method will also be carried out.

5) MODERATE: In the results, it is clearly shown that the soil moisture threshold percentiles are different for different slope angles. Then, it is not clear if the slope dependence of soil moisture percentiles is used in the validation of the approach on the 45 rainfall events showing in Table 4 and Figure 9. It should be clarified.

Reply: The utilisation of different percentiles for different slope angle groups is applied in the validation. For the validation study, each threshold determined for each of the slope class is used for summarising the numbers of T, F, P, and N events. Those numbers are then combined

to determine the overall statistical indicators (i.e., HR, FAR, HK). We will clarify this in the updated manuscript.

I listed in the specific comments a number of corrections and changes that are needed.

SPECIFIC COMMENTS (P: page, L: line or lines)

P3, L60: Use of soil moisture for landslide prediction has been recently used. However, in Italy some studies using modelled soil moisture data have been published and I believe they should be mentioned (e.g., Ponziani et al., 2012, doi: 10.1007/s10346- 011-0287-3; Ciabatta et al., 2016, doi: 10.1016/j.jhydrol.2016.02.007).

Reply: Agreed. They will be added.

P4, L87: Spatial and temporal resolution of modelled data can not be set "discretionarily". It depends of many aspects, among them resolution of input observations and of maps used for the parameterization. Please revise.

Reply: Agreed. This will be revised.

P5, L112-113: Threshold of what? At this stage, it is not clear to what the authors refer. Please clarify.

Reply: Agreed. This will be revised.

P6, L127: 20-percent of mountainous area is covered by landslide. Is it correct? It seems to be overestimated.

Reply: Agreed. 20% is an estimation. The sentence will be revised.

P6, L129-130: Shallow landslides are not triggered by short and intense rainfall events only. Long and moderate rainfall events over saturated conditions may generate landslide events. Please revise.

Reply: Agreed. This will be revised.

P7, L144: Typo "WRF""

Reply: This will be corrected.

P12, L252: Typo "spun-up", also at L253.

Reply: They will be corrected.

P12, L255: The ERA5 dataset is found to be better than ERA-Interim, also with a better spatial resolution. It should be used, at least for future studies.

Reply: Agreed. We will include this information in the discussion.

P15, L328: 500 km radius seems to be too large. Please revise.

Reply: Agreed. This will be revised. And the suggested WRF rainfall evaluation study can provide useful guidance of the models' soil moisture performance.

P16, L358-359: I believe that in situ soil moisture observations at deep layer are wrong, at least for some periods. Therefore, it should not be used for model evaluation.

Reply: We have included a discussion about the possible in-situ sensor failure at the deep layer (i.e., "However, the soil moisture measurements from the in-situ sensor also get our attention as they show strange fluctuations with numerous sudden drops and rise situations observed. The strange phenomenon is not expected at such a deep soil layer (although groundwater capillary forces can increase the soil moisture, its rate is normally very slow). One possible reason we suspect is due to sensor failure in the deep zone."). But we will clarify in the updated manuscript that 'therefore the deep-layer data should not be used for evaluation'.

P17, L365-385: The visualization of 2 soil moisture maps for 2 specific days has little sense to me. Better would be to perform a cross-correlation analysis in space and time to highlight the space-time agreement between the modelled datasets.

Reply: Agreed. The spatial maps will be deleted. The suggested rainfall evaluation study will provide an improved assessment of both the spatial and temporal soil moisture accuracy of the WRF model.

Figure 7: It is crowded, with too many lines. Please try to simplify.

Reply: Agreed. The figure will be improved.

Figure 8: Try to improve the visualization of the results in the figure.

Reply: Agreed. The figure will be improved.

P20, L453-454: It is quite unexpected that deeper soil moisture is less effective for landslide prediction. It should be explained, or at least discussed, this important aspect.

Reply: Agreed.

P22, L482: What is the "weighting factor" that should be considered?

Reply: Weighting factors can include both social and economic components, for instance it can include the cost of a disaster event (e.g., both short-term and long-term impacts), the cost of an evacuation (e.g., relocation cost, business shutdown), as well as the social impacts of both cases. More details will be included in the updated manuscript.

RECOMMENDATION

On this basis, I found the topic of the paper relevant, and I suggest a moderate revision before the paper can be published on Hydrology and Earth System Sciences.