Hydrology and
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Interactive comment on "Assessment of Simulated Soil Moisture from WRF Noah, Noah-MP, and CLM Land Surface Schemes for Landslide Hazard Application" *by* Lu Zhuo et al.

Anonymous Referee #1

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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?

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.

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.

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.

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.

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).

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.

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

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

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.

P7, L144: Typo "WRF'"

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

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.

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

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.

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.

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

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

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.

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

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.

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