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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 #2

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I've read and carefully evaluated the manuscript and my opinion is that it needs major revisions before being published in HESS. Although the scientific significance is high, I think that the scientific quality is affected by several shortcomings. The manuscript is generally well written, the presentation quality is fair and could be improved in some parts. Please find hereafter my main concerns, divided among general and specific issues.

GENERAL ISSUES

1- The authors should clearly differentiate this work from their recently published "Evaluation of Remotely Sensed Soil Moisture for Landslide Hazard (IEEE-JSTARS 2019)"

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Discussion paper

and avoid that some introductory parts read very similar.

2- I am somehow concerned about the dataset used. Emilia Romagna is one of the Italian regions with the best environmental datasets, some of them also publicly available for free. Therefore, I wonder why a dataset period was chosen in which only a soil moisture station is available, and why a manuscript submitted in 2019 relies on datasets until only 2015 (all dataset used extend almost until present days). DEMs of the region are available at finer spatial resolutions (10m and 20m): why using a 90m resolution SRTM DEM? Lastly, the landslide dataset seems largely incomplete. Many works on the same test site (see following comment) used larger landslide dataset for the same time period. This shortcoming may let the readers question about the significance of results obtained.

3- The references of the manuscript are very biased. I think the authors cited almost their whole scientific production (e.g. at line 294: are three references from the same author necessary?), while they almost ignored what has been published on the same subject and in the same case of study. For instance: landslide characteristics could be better described making reference to some previous works (e.g. Bertolini et al., 2005; Rossi et al., 2010). Regional scale rainfall thresholds for the Emilia Romagna have been already published by Berti et al. (2012) using an I-D approach and by Martelloni et al. (2012) using antecedent rainfall. Regional scale landslide warning systems for the Emilia Romagna Region have been addressed in several papers (e.g. Lagomarsino et al., 2013; Segoni et al. 2018a). Lagomarsino et al. (2015) compared an I-D threshold model and an antecedent rainfall threshold model concluding that in Emilia Romagna the latter provides better performances, probably due to the complex hydrologic response of the hillslopes after rainfalls. Segoni et al., 2018b (already in your reference list) tested that the performances of the Emilia Romagna threshold system could be improved by integrating basin-scale soil moisture estimated by means of TOPKAPI model. I think all those antecedent works could be used to properly “set the stage” for your research. Berti, M., Martina, M. L. V., Franceschini, S.,

Pignone, S., Simoni, A., & Pizziolo, M. (2012). Probabilistic rainfall thresholds for landslide occurrence using a Bayesian approach. Journal of Geophysical Research: Earth Surface, 117(F4). Bertolini, G., Guida, M. & Pizziolo, M. Landslides (2005) 2: 302. https://doi.org/10.1007/s10346-005-0020-1 Segoni, S., Rosi, A., Fanti, R., Gallucci, A., Monni, A., & Casagli, N. (2018). A Regional-Scale Landslide Warning System Based on 20 Years of Operational Experience. Water, 10(10), 1297. Lagomarsino, D., Segoni, S., Fanti, R., & Catani, F. (2013). Updating and tuning a regional-scale landslide early warning system. Landslides, 10(1), 91-97. Martelloni, G., Segoni, S., Fanti, R., & Catani, F. (2012). Rainfall thresholds for the forecasting of landslide occurrence at regional scale. Landslides, 9(4), 485-495. Lagomarsino, D., Segoni, S., Rosi, A., Rossi, G., Battistini, A., Catani, F., & Casagli, N. (2015). Quantitative comparison between two different methodologies to define rainfall thresholds for landslide forecasting. Natural Hazards and Earth System Sciences, 15(10), 2413-2423. Rossi, M., Witt, A., Guzzetti, F., Malamud, B. D., & Peruccacci, S. (2010). Analysis of historical landslide time series in the EmiliaâĂŘRomagna region, northern Italy. Earth Surface Processes and Landforms, 35(10), 1123-1137.

4- At this scale of analysis, the attempt to relate modeled soil moisture to a single instrumented site is a too big stretch in my opinion. Please, consider also that the sensor is located in a completely different setting (wide alluvial plain) than the territory typically affected by landslides (hills and mountains). I think the trends of soil moisture could be largely unrelated (as also the authors stated at line 61) and the example at line 328 (500km radius) would not hold in a case study characterized by many differences and peculiarities like Emilia Romagna. The authors could maybe cite other authors that attempted to establish empirical correlations of hydrological variables in Emilia Romagna (Segoni et al., 2018 with soil moisture; Martelloni et al., 2013, with snowpack thickness), however they calibrated the relationships over smaller territorial units, not over the whole region. I think these works could be used to partially defend the approach used in the manuscript, but I don't think they can completely clear the feeling that just one single instrument for the whole region is insufficient.

5- The authors should be very careful in providing unbiased, objective and humble points of view. The feeling is that in some parts of the manuscript they are overreaching when describing the results obtained (e.g. "outstanding" at line 479). Indeed, in my opinion the results are questionable. Beside the issue of using an instrument located in the alluvial plain to model landslide occurrence in very different climatic, hydrologic and geo-morphologic settings, there is a clear problem of result evaluation: most of the results are presented as graphics where it is difficult to ascertain the goodness of the model fitting because a long dataset is compressed in a small figure and also a qualitative evaluation is hard (sections 4.1. and 4.2). Some quantitative validation is mandatory to better evaluate the results. We need to know the differences, how big they are, where/when are located and why they are present. Also, about abstract, results and discussion: I don't think WRF modelled soil moisture has been properly evaluated for landslide monitoring purposes (line 464-465). This work in my opinion can be considered a preliminary attempt towards that direction, but to reach the goal more and better data are needed, together with a more thorough and quantitative evaluation of the results. I suggest that the authors rephrase their statements.

SPECIFIC ISSUES

18. "landslide threshold model" is a very generic term. Please, be more specific.

40-42. Please adjust this sentence and provide more references if necessary. Caine was the first to establish an I-D threshold, but to my knowledge that threshold has never been used for a warning system. In addition, national scale landslide warning systems are not so common and not so many examples of prototypal or operation applications exist in the literature (e.g. Krogly et al., 2018; Rosi et al. 2016; Auflic et al., 2016). Indeed, threshold-based landslide warning systems are usually established for smaller areas (e.g. basins or regions or small alert zones), see e.g. Devoli et al. (2018), Baum and Godt (2010), Mathew et al. (2014) . . .

149-150. "weak earth units" is unclear. Please, rephrase.

237 “an improved”

279-280. Not clear, please rephrase.

303-304. I think the concepts of TP/TN/FP/FN are quite established, no need to make reference to other works.

313. Maybe I’m mistaken, but I don’t think at this point the slope degree groups have been presented yet.

342. Please rephrase: “very well” cannot be used (see also general comments).

356-359. So, you are saying that the dataset has a bad quality? Maybe the dataset needs to be smoothed?

370. Please, revise the English.

378. Actually, in my opinion you don’t have a reliable benchmark, so you can only say that the models provided different results, you cannot say which one provided the best results.

387. This part is very important, but you did not introduce it appropriately. This is strange, because in the introduction you cited many relevant papers (Glade et al., 2000; Brocca et al., 2008; Segoni et al., 2018; Bogaard and Greco 2018). Maybe you should spend a few words saying that previous works demonstrated that in complex geomorphologic settings (as Emilia Romagna) a rainfall threshold approach is too simple and more hydrologically-driven approaches need to be established.

396. I disagree. The relationship between the slope angle and the landslide triggering is not so straightforward and it depends on the landslide typology. E.g. many slow earth flows (which are abundant in Emilia Romagna) can occur also on very low slope gradients.

407. This choice is questionable. The landslide susceptibility does not necessarily have to be equally represented in the territory.

422-426. This is quite obvious, I wouldn't devote so much space to this.

427. Why are you mentioning shallow landslides? Until now, I figured out that you are trying to model landslides in general.

430-431. Please, rephrase this sentence.

453-454. Theoretically, the conditions at the sliding surface should be the ones with the best performances. Therefore, either you have very shallow landslides, or your results are not good at assessing the real soil moisture conditions that are actually triggering/predisposing landslide initiation.

475-476. Please rephrase.

479. Do not use "outstanding".

481. I failed to quickly check the correlation coefficient. Please, clearly write where it can be found by the readers.

499. To my experience, the applicability of a similar system in a warning system would be very limited because it would have a poor spatial resolution: warning would be issued over the whole region, thus having limited actual applicability.

628. Please, check.

JEMEC AUFLIČ M, ŠINIGOJ J, KRIVIC M, PODBOJ M, PETERNEL T, KOMAC M, 2016. Landslide prediction system for rainfall induced landslides in Slovenia (Masprem). GEOLOGIJA 59/2, 259-271 Baum RL, Godt JW (2010). Early warning of rainfall-induced shallow landslides and debris flows in the USA. Landslides, 7(3):259–272. Krøgli, I. K., Devoli, G., Colleuille, H., Boje, S., Sund, M., and Engen, I. K.: The Norwegian forecasting and warning service for rainfall- and snowmelt-induced landslides, Nat. Hazards Earth Syst. Sci., 18, 1427-1450, https://doi.org/10.5194/nhess-18-1427-2018, 2018 Martelloni, G., Segoni, S., Lagomarsino, D., Fanti, R., & Catani, F. (2013). Snow accumulation/melting model (SAMM) for integrated use in regional scale landslide early warning systems. Hydrology and Earth System Sciences, 17(3), 1229-1240. Mathew, J., Babu, D. G., Kundu, S., Kumar, K. V., & Pant, C. C. (2014). Integrating intensity–duration-based rainfall threshold and antecedent rainfall-based probability estimate towards generating early warning for rainfall-induced landslides in parts of the Garhwal Himalaya, India. Landslides, 11(4), 575-588. Rosi, A., Peternel, T., Jemec-Auflič, M., Komac, M., Segoni, S., & Casagli, N. (2016). Rainfall thresholds for rainfall-induced landslides in Slovenia. Landslides, 13(6), 1571-1577.

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