We thank the reviewer for the review and the constructive comments. We address here all the points raised, and we indicate how we will take care of them in the revision.

General comments: The topic of this study presented in this paper is interesting. The manuscript attempts to present the comparison of two different methods applied for rainfall-induced shallow landslide prediction. However, the data used for each method are mainly obtained from the database and estimations. So, it is hard to see the novelty of the study presented in this paper.

While the methodologies used in the manuscript are not novel, but rather taken from ours and other previous studies, the messages derived from the analyses and the comparisons carried out are in our opinion relevant, important, and new. Above all, the superiority of the soil wetness estimates from the conceptual higher resolution model highlights that the resolution of the hydrological model is critical, even more than the physical description behind the hydrological model (i.e., a conceptual model at 500m resolution is found to be superior to a physically-based one at 12.5 km resolution). This conclusion comes even before getting into the methodology for landslide prediction (i.e., physically based modeling of soil moisture and stability vs a probabilistic approach based on hydrometeorological thresholds).

The presentation of the results is quite difficult to understand since the authors presented the results of the probabilistic approach in graphical forms.

We are not sure we follow what the referee means by “graphical form” only. In the probabilistic approach first results just concerning the hydrological component are shown (Figure 6 and 7) and commented in lines 281-293, and in combination with rainfall thresholds in lines 293-301. Secondly, we utilize the soil wetness estimates in a probabilistic approach, by defining two rainfall thresholds depending on antecedent wetness. These thresholds are reported and commented in lines 302-307. The probabilistic approach only deals with the temporal dimension of landslide triggering, and only focuses on triggering conditions (rainfall and antecedent wetness), but not whether a landslide is actually possible at a given location (which is what susceptibility mapping does). Therefore, when considering a probabilistic approach one should look at the temporal dimension (i.e., whether wetness and rainfall conditions are exceptional when a landslide happened), which is better explained by figures such as Fig 7 and 8 than purely with skill metrics in tables.

The discussion covered something that has already been understood from the results of using data from estimations.

The discussion builds on the results presented in the previous sections and focuses on understanding and explaining the results obtained, mainly focusing on the “failure” of the physically based approach, which we did not anticipate at the start of the study. All the limitations of the proposed framework (i.e., methods and data) are analyzed, to identify the main sources of error.

The manuscript is more like a technical paper than an academic paper in the present format. This paper also needs to be grammatically corrected.

We believe that this manuscript is an academic paper as it doesn’t just follow established methodologies, but combines and explores aspects from several different studies (i.e., probabilistic approach, infinite slope approach, TWI downscaling, soil depth estimation, physically based susceptibility mapping, etc.), providing a fair comparison of two approaches which not only stresses the known importance of antecedent conditions, but also highlights what the limitations are of such approaches at the regional scale. Grammar corrections will be made where errors are found.

Specific comments: 1. Introduction Provide some reasons/ justifications for selecting the physical-based model and probabilistic in your study. Please state the urgency of comparing these two methods. What was the hypothetical background that prompted you to compare these two methods?

The probabilistic approach is the most commonly applied method at the regional scale in landslide predictions. Physically based approaches are often only applied at the local scale (e.g. in small watersheds where landslides occurred), due to computational and data limitations, but could in principle provide better predictions at any scale. The idea behind this study was to objectively compare these two methods at a large scale by choosing readily available hydrological estimates and data, and robust and widely used methodologies for this purpose (rainfall thresholds and infinite slope approach). We will explain the context and purpose of the comparison better in the revised manuscript.

2. Methods: (a) The physical-based modeling is dependent upon the accuracy of soil properties data. Your current modeling study used estimated soil properties. Please state how accurate your estimation of soil unit weight, cohesion, and friction angles using method/ approach. Please provide some justifications of these approaches/ methods, perhaps by presenting some previous studies results in the Introduction section.

The reviewer is correct in raising this point concerning parameter uncertainty. The uncertainty of the soil parameters is either directly coming from the uncertainty in the SoilGrids dataset in OpenLandMap, or a combination of that and other choices, such as in the case of friction angle, where the values are chosen from literature, given the soil classification. It is complex to quantify the combined uncertainty rigorously. Because we found that the limitation of the suggested physically based approach was the lack of temporal dynamics, which is absolutely independent from the infinite slope approach and its parameters, we decided not to specifically quantify the effects of soil parameter uncertainties, for example by Monte Carlo analysis. However, we did quantify the effect of the single most important uncertainty that is the soil depth, by scenario analysis with four different soil depth estimation methods typically used in scientific investigations and in practice. We will expand the discussion of these uncertainties and their implications in the Discussion section of the revised manuscript.

(b) All datasets OpenLandMap are at a resolution of ca 250x 250 m, but the modelling considered a resolution of DEM at grid cell size of 25x25 m. Please state what method you chose to resolve this difference in map resolution.

For each 25m x 25m cell, the value of the 250m x 250 m in which the cell is completely or mostly contained is chosen. We will add this explanation in the revised manuscript. It should be recognized that the high resolution of the topography is primarily needed for a slope gradient estimation, which is closest to the appropriate scale of the infinite slope stability model. The coarser scale of the soil and landcover data are a secondary and less important problem.

(c) Please justify choosing the values of cohesion due to tree roots of 5 - 22 kPa. The values may indicate that the soil layers have low strength.

As already explained in the text, the range of cohesion values and the individual values assigned to each land cover class were decided based on previous studies that either provide measured ranges and suggest that denser tree covers and mixed forests are associated with larger values of cohesion (Schwarz 2012, Dorren and Schwarz, 2016). We have attempted to cover a range of values of root added cohesion reported in studies in experiments (e.g. Cazzuffi et al., 2014). The referee is reminded that the added cohesion by roots is strongly depth dependent and for our estimation with the Mohr-Coulomb approach we need a depth integrated value for the soil layer, for which the range up to ca. 20 kPa is realistic for a range of herbaceous plants and tree species.

3. Results The results of the physical modeling and probabilistic approaches also need to be presented in a spatial format and then validated with historical landslides.

There must be a misunderstanding here. All the analyses are done considering the spatial dimension and the historical landslides from the database mentioned in the paper. All figures 4-8, which present the probabilistic and physically based approach are done comparing to the historical landslides. For each landslide location, the rainfall events and antecedent wetness conditions or factor of safety values are separated into triggering (if a landslide happened in that location during the rainfall event) or non-triggering otherwise. As mentioned in the second response above, the probabilistic approach doesn't deal at all with the spatial susceptibility (i.e., whether at a location landslides are to be expected or not), which is the explanation for the absence of spatial maps for this approach. In the physically based approach, the spatial dimension is considered through the infinite slope approach. This is shown in figure 3 with the wet and dry boundary scenarios and in Table 1, where the percentage of landslides is reported for each region (unconditionally stable/unstable and conditionally unstable). As mentioned in lines 257-259, adding dynamic hydrology, with the downscaled TerrSysMP estimates, reduces even more the conditionally (un)stable portion of the domain, as most cells are simulated constantly at saturation. We do not see what else we could add about the spatial analyses that would add more clarity.

4. Conclusions: please check again whether the conclusions drawn have answered all the research questions. You seemed to miss answering questions 1 and 3 stated in the Introduction section.

In the revised manuscript we will revise this so that conclusions and research question in the introduction will match closely.

5. Please check the writing again for grammatical errors (see the attached file). You can use Grammarly to find the errors and to get suggestions for the corrections.

Indeed, there were some small grammar errors in the text, but what the referee is pointing to are not grammar errors rather styles of writing. Two of the co-authors use English as a mother tongue, we will make sure that grammar errors are removed.

Cazzuffi, D., Cardile, G. and Gioffrè, D.: Geosynthetic Engineering and Vegetation Growth in Soil Reinforcement Applications. Transp. Infrastruct. Geotech. 1, 262–300 (2014). https://doi.org/10.1007/s40515-014-0016-1

Dorren, L. and Schwarz, M.: Quantifying the stabilizing effect of forests on shallow landslide-prone slopes, in: Ecosystem-Based Disaster Risk Reduction and Adaptation in Practice, pp. 255–270, Springer, 2016.

Schwarz, M., Cohen, D., and Or, D.: Spatial characterization of root reinforcement at stand scale: theory and case study, Geomorphology, 171, 190–200, 2012.