Authors' response to Referee 2 (RC2)

On behalf of all the authors I would like to thank this Referee for taking her/his valuable time in deeply reviewing our manuscript and making valuable suggestions that will surely enrich our work. Below, you will find a point-to-point response to your comments. Your comments are presented in italics, with our responses in regular font.

I read with interest the paper by Daniel Camilo Roman Quintero and co-authors. Their research in physically-based modelling of shallow landslides is welcome in the literature and it fits well within the scope of NHESS. They propose an approach to deal with synthetic long record of landslide occurrences and hydrometeorological conditions based on a region of Italy. Their research allow to define empirical rainfall thresholds associated with landslide occurrence in the context of landslide early warning systems. Their approach is aimed are being used on large area contexts. I also like the fact that they compare their modelling outputs to actual landslide occurrences.

I would like to thank the Referee for the positive judgement of our work. The Referee perfectly understood the goal of our study and the methodology proposed to that aim.

While I have limited comments over the modelling approach - the author demonstrate a strong understanding of hillslope response to rainfall and soil moisture - my main concern is, overall, that a discussion is truly missed. The authors proposed a combined results-discussion section, but apart from a link to observed landslide occurrences in the region, there is, as also pointed out by the other reviewer, not real reflection of their research with respect to the method approach, the applicability, the early warning context (to name but a few points that could be discussed) and the associated state of the art literature. To me, it makes little sense to have a manuscript without a proper discussion.

We acknowledge that the discussion is somewhat lacking. This is likely due to our initial choice of combining the "results and discussion" in a single section, which contains a substantial amount of data and results and, therefore, obstructs a clear and extensive discussion. Therefore, in the revised paper we will separate Results from Discussion section, so to extend the discussion to better highlight potential and limitations of the proposed approach, in the context of landslide hazard assessment and early warning, and its alignment with the state of the art in the scientific literature.

Added to the comments of the first reviewer, I have listed below other comments which I hope would be helpful to improved the manuscript:

The definition of "large area" is somehow unclear. 80km² for the case-study test zone is not a very large zone when compared to many landslide data-driven susceptibility assessments. There is maybe some way to better define/constrained this scale of analysis context.

Thank you for raising this interesting issue. The distinction between large and small areas is indeed subjective and is often tied to the techniques used to understand the process under study. Data-driven susceptibility models are indeed often applied to areas larger than our study area. However, they usually rely on techniques designed to handle large datasets and extract relationships among the data without considering the physics of the involved slope processes. In our case, instead, we make use of a physically based model of the slope response to precipitation, and this can be done only if it is applied to a homogeneous geomorphological context. This is usually done with detailed flow and equilibrium models at slope scale, but here we encompass hundreds of slopes in an area of nearly 100 km^2. On the other side, the statistical-empirical approaches to define landslide thresholds are usually designed for larger areas, at regional or even national scale (Guzzetti et al., 2007; Peruccacci et al., 2017). However, these approaches merely rely on the correlation between rainfall and landslide occurrence, without introducing information about the physical behavior of the studied slopes.

Differently, our approach is based on a physically grounded rationale, aiming to identify causal (physical) relationships rather than mere correlations (although complex and non-linear as those that can be identified with data-driven approaches). As the Referee correctly points out, we used infiltration and slope stability models to replicate the hydrological conditions that lead to landslides in a specific region. Usually, this approach is suited to representing a specific slope at a small local scale. In our study, we started from a reference slope inclined at 40°, with a 2 m thick soil cover overlying the limestone bedrock. This geometry resembles the features of the slope involved in one of the most catastrophic landslide events in the study area: the Cervinara landslide in 1999. This event is shown with nr. 1 in Figure R2-1, where all the landslides reported in the existing catalogues are also represented. Then, we extended the analysis to a larger area, approximately 80 km^2, as indicated by the green-hatched region in Figure R2-1. To do this, we accounted for the effects of the variability of slope and soil properties, as well as of the effects of the variability of weather forcing.

This is the meaning of “large area” in our case: extending the results of a single slope to an area including hundreds of slopes with variable characteristics, although within the same geomorphological context. We will better clarify this aspect in the revised manuscript.

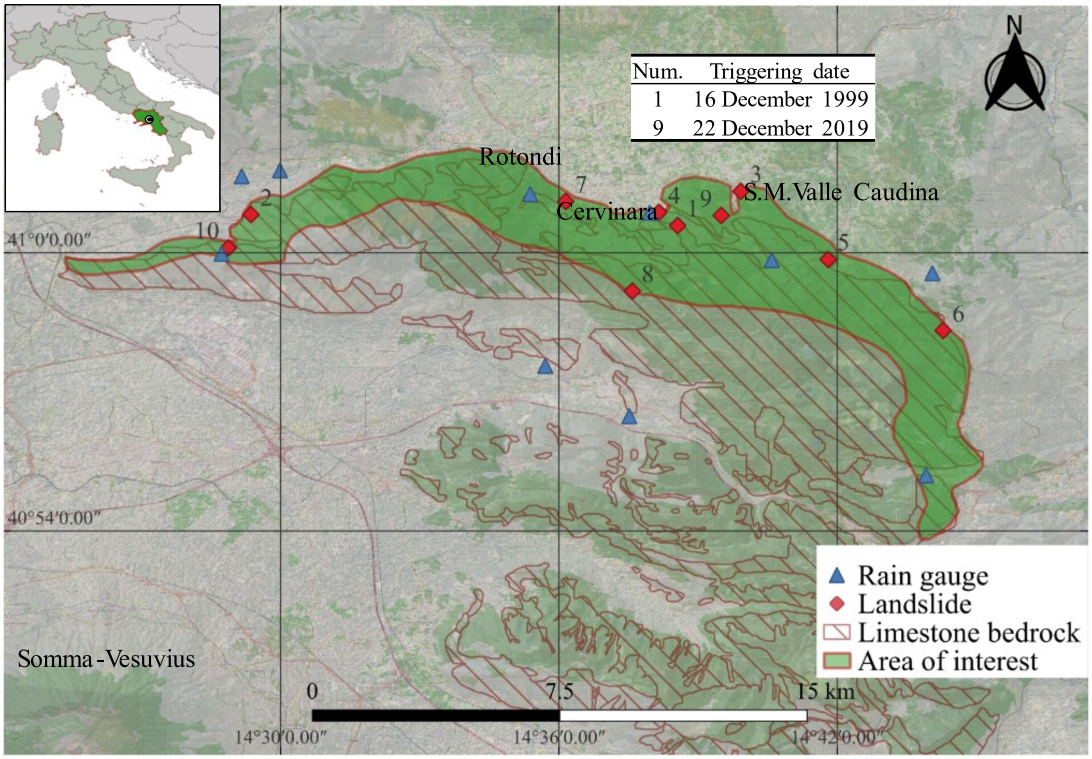

Figure R2-1. Map of the large area of this study, i.e., the north-facing side of the Partenio Massif (green-filled area), with indication of the rain gauges and of the major landslides reported in available landslide catalogues from 1999 to 2020 (sourced from: (Calvello & Pecoraro, 2018; Peruccacci et al., 2023)).

The end of the abstract is very case-study specific. One would welcome an ending with a more general/broader statement.

Thank you for noticing it. We will modify the statement at the end of the abstract, so to make it more general.

Lines 33-34. To be accurate, the issue of human influence on landslides is not only in urban areas. Note also that there are some recent work (and maybe more relevant) that allow to support such a broad statement. For example, Ozturk et al. (2022) https://www.nature.com/articles/d41586-022-02141-9

We see the Reviewer's point, but our statement, probably misleading, refers to the effects of landslides on humans, and not vice versa, as the Reviewer suggests. The following sentences are also along the same line (i.e., damages and economic losses). However, we will enlarge the focus of the phrase in line 33-34 by including some references highlighting also the human influence on landslides in the revised manuscript.

Line 35. Predicting the occurrence of landslide is also relevant when outside cities and/or when the landscape is not disturbed by human activities. Landslides are above all natural processes, and this is what the authors want to model here. The focus on urban areas from the start of the introduction is somehow misleading.

This comment is in line with the previous one, and we agree. We will rephrase the text in the revised manuscript.

Line 44: ..” depends not only on”.. is a strange formulation. After the not only, we would expect “but also” somehow. This sentence must be rephrased.

Thank you for catching it. We will rephrase in the revised manuscript: “landslide initiation depends not only on the triggering rainfall event characteristics, but, as in the case of shallow landslides, the achievement of instability is also favored by antecedent wet soil conditions (Mirus et al., 2018a; Wicki et al., 2020)

Lines 75-82: it is strange to have such an emphasis on the study area in the middle of the state of the art.

We will rephrase this part, to make clearer that, from line 75 onward, we have moved the focus from the general state-of-the-art to the literature about the specific area that we study in this paper.

Lines 95-125: this is a very long part of the introduction to explain the goal of the research and provide a supposedly short overview of what has been done. In my opinion too many details are provided here; it sounds more like an extensive abstract.

Thank you for suggesting it. We will reduce this final part of the introduction in the revised manuscript.

Several times, the emphasis is put in LEWS. Although rainfall threshold determination is a key aspect of LEWS, their study can also be relevant to other hazard assessment needs. This is something that could be nuanced.

We agree and will follow this suggestion in the revised manuscript.

The title refers to large areas. In the introduction “wide areas” is several time used. Beyond this lack of consistency, one would appreciate a definition of what “large” actually means (size, spatial resolution, etc.)

As described in the reply to one of the general comments from this reviewer, we will better specify the meaning of “large area” in this paper. We will also stick to the term “large”

Figure 1. Some local names in the maps are not readable. What is the background information of the map? In such a figure one would expect a visualization of the topography to better understand, for example, the slope context.

We don’t understand what are the not readable names the Referee refers to. The smallest font size in the figure equals more or less the text font size in the caption. However, we will revise the figure to enhance its readability. Moreover, the background presents a street map dividing hillslopes (green areas) from flat areas (grey areas). We will improve the figure replacing this background map with a topographic map with isolines to clarify the aspects outlined by the Referee.

Line 135. Local names are being used for specific location. However, without a map to local them, such information is not relevant to a broad audience.

If the referee is referring to the local names of the cities Rotondi, Cervinara and San Martino Valle Caudina, those locations are not only representing the municipalities hit by landslides, but also the location of the rain gauges we used to analyze rainfall spatial variability in the area. However, the locations are indicated in the map of Fig. 1. We will try to make it clearer in the revised manuscript.

Line 138: "a huge debris avalanche". What do you mean by "huge". I would suggest not to use such a subjective wording. Here is a reference that could help:

McColl, S. T., & Cook, S. J. (2024). A universal size classification system for landslides. Landslides, 21(1), 111-120.

Thank you for your suggestion. We will adopt the suggested classification in the revised manuscript, where we will also give the estimated volumes for the two mentioned landslides.

Line 221. Replace "associated to" by "associated with"

Thank you for catching it. We will modify the revised manuscript accordingly.

Line 245: early warning system. Remover capital letter

Thank you for catching it. We will modify the revised manuscript accordingly.

Line 265. "normal…" remove capital letter

Thank you for catching it. We will correct it in the revised manuscript.

Lines 257-266: There seem to be some repetition here to what is being said earlier.

Thank you for suggesting this. We recognize that there is some repetition of what is written at lines 105-111 (in the Introduction). However, this sentence here is required to properly introduce the discussion about the assessment of variability. Therefore, we will rephrase both lines 105-111 and 257-266 to minimize useless repetitions.

Line 269: remove the two ",'

Thank you for catching it. We will amend it in the revised manuscript.

Figure 4 (and in other places). Is the use of decimal values in the percentage valid? Does it make sense?

The envelopes depicted with red solid and dashed lines in Figure 4 represent the bounds containing 68.3% and 86.6% of the data, respectively. These are the percentages of the data that, in a Normal distribution, belong to intervals centered around the mean with width of two and three standard deviations, respectively. As can be seen in many figures and tables found in literature (see an example below), it is perfectly legitimate to express percentages with decimal digits (if we misunderstood the comment, please accept our apologies).

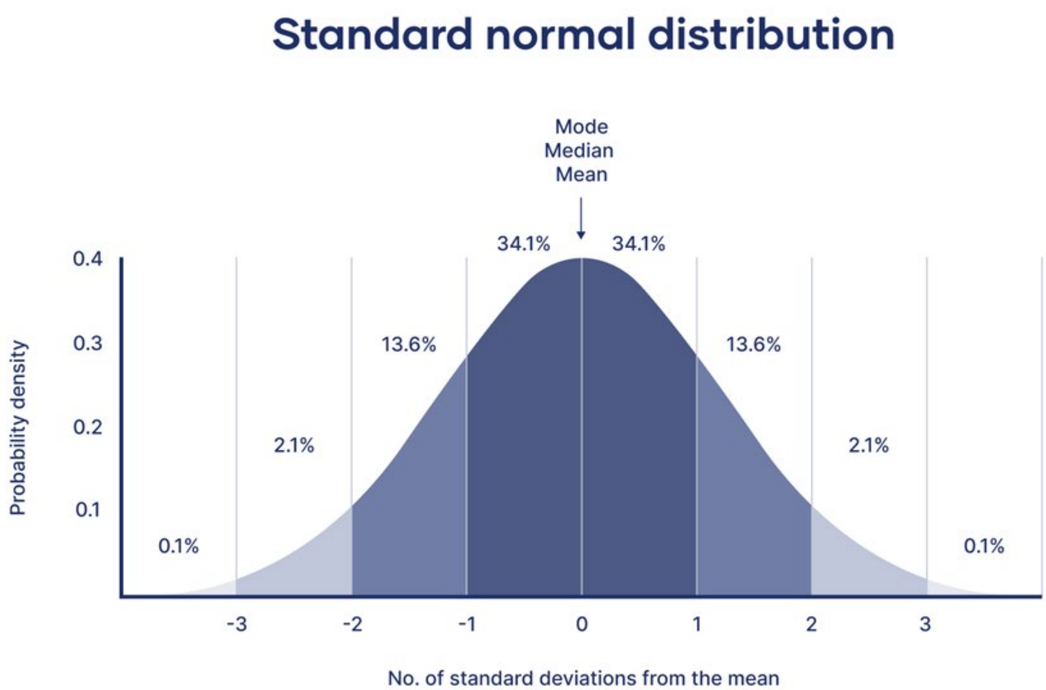

Table 2. What is "s.l.m."?

Thank you for catching this typo. We meant m.a.s.l. (i.e., meters above sea level) and we will correct it in the revised manuscript.

Line 229. The work of Brocca et al 2021) is mentioned as a research carried out on a relatively small area (less than 100 km²). How can we say it is a small area while, here, one speak about lar scale for an area of 80 km². As said earlier, one need a more robust definition of the scale aspect of this research.

We think you mean line 329. Thank you again for your suggestions on this issue related to the definition of the scales (a similar issue has been also raised by the other Referee). In this study, "small scale" refers to the modeling results obtained for a single slope (the reference slope), where the variability of slope features can be neglected (or, in other words, where slope features are very well known). When we extend to an area of 80 km^2, it encompasses hundreds of slopes with variable geometry and soil characteristics, and this variability affects the assessment of landslide triggering. This is what we mean here with "large scale". In the paper of Brocca et al. (2012), the focus was on river catchments, and the extension of 100 km^2 could be considered relatively small (by the way, we realized that, by accident, the mentioned paper by Brocca et al. (2012) was not in the reference list at the end of the manuscript, and we will fix this error in the revised manuscript). We will avoid using misleading words about this kind of scale-related concepts in the revised manuscript.

Line 368. Remove capital letter in "Normally…"

We think you refer to line 358. Thank you for catching this mistake. We will modify to Normal-distributed in the revised manuscript (the capital letter refers to the Normal probability distribution).

Table 4. "large scale" instead of "Large scale"

Thank you for catching it. We will adjust it in the revised manuscript.

Lines 364-370. Some of this information is connected to the description of the study area. It comes a bit as a surprise that this is provided here.

We agree that this information is connected to the description of the study area, that is mostly done in section 2.1. However, we need this information here, as it informs the uncertainty analysis, that is specifically tailored to the spatial variations observed in the study area, to assess how it affects landslide prediction. To avoid useless repetitions, we decided to report here the information written in lines 364-370. However, we will rewrite these lines, remove all the information not essential for introducing the uncertainty.

Figure 6. Make sure that all symbols used in the figure are explained in the caption. Same comment for figure 7 and others

Thank you for noticing it. We will adopt this suggestion in the revised manuscript.

Line 385. Dataset instead of data set.

Thank you for catching it. We will adjust it in the revised manuscript.

Line 410. Such isolated sentence must be removed (or attached to a main paragraph).

Thank you for catching it. This is just a formatting issue, that we will fix in the revised manuscript.

Section 3.1. this is purely results, no discussion here. In sections 3.2 and 3.3 reference is only made to work that identified landslides in the region.

Thank you for highlighting it. As mentioned before, we will separate results from discussion into two different sections in the revised manuscript, and this kind of inconsistencies will be solved.

Line 430. 175 failures in 500 years. However, in line 376, 20 events. This is nuclear.

Thank you for pointing out this, as it reveals that we did not make this aspect clear enough. At line 376, the analysis deals with the reference slope (local scale), without considering the variability of properties and weather forcing that one would expect moving to a larger scale. If we look at what happened to the slopes around Cervinara two landslides have been reported in 24 years (between 1999 and 2023) occurring on the same slope (small scale: smaller than one km^2), which is consistent with the 20 events in 500 years obtained with the synthetic dataset for the reference slope. At line 430, the spatial variability has been introduced by perturbing the dataset, thus we have moved from the reference slope to the “large area” (including all the slopes of the study area with their variable characteristics as well as the variability of the meteorological forcing). In the same 24 years, 10 landslides have been reported in the north facing side of Partenio Mountains. Interestingly, we identified 175 landslides in 500 years in the perturbed synthetic dataset, obtaining also in this case consistent rates (one landslide every 2-3 years).

We believe that these results confirm the reliability of the physically based model as well as the suitability of the synthetic data generation approach and of the way the spatial variability was accounted for. We will modify the text to better clarify all these aspects in the revised manuscript.

References

Brocca, L., Tullo, T., Melone, F., Moramarco, M., & Morbidelli, R. (2012). Catchment scale soil moisture spatial–temporal variability. *Journal of Hydrology*, 422-423, 63-75. https://doi.org/10.1016/j.jhydrol.2011.12.039.

Calvello, M., & Pecoraro, G. (2018). FraneItalia: a catalog of recent Italian landslides. *Geoenvironmental Disasters*, *5*(1), 1–16. https://doi.org/10.1186/S40677-018-0105-5/FIGURES/11

Guzzetti, F., Peruccacci, S., Rossi, M., & Stark, C. P. (2007). Rainfall thresholds for the initiation of landslides in central and southern Europe. *Meteorology and Atmospheric Physics*, *98*(3–4), 239–267. https://doi.org/10.1007/S00703-007-0262-7/METRICS

Peruccacci, S., Brunetti, M. T., Gariano, S. L., Melillo, M., Rossi, M., & Guzzetti, F. (2017). Rainfall thresholds for possible landslide occurrence in Italy. *Geomorphology*, *290*, 39–57. https://doi.org/10.1016/J.GEOMORPH.2017.03.031

Peruccacci, S., Gariano, S. L., Melillo, M., Solimano, M., Guzzetti, F., & Brunetti, M. T. (2023). The ITAlian rainfall-induced LandslIdes CAtalogue, an extensive and accurate spatio-temporal catalogue of rainfall-induced landslides in Italy. *Earth System Science Data*, *15*(7), 2863–2877. https://doi.org/10.5194/ESSD-15-2863-2023