Referee #3

EC1: ‘Comment on egusphere-2026-1599’, David J. Peres, 24 Jul 2026

We sincerely thank the Editor/Reviewer for the thorough and constructive evaluation of our manuscript. In response to the comments, we clarified that the proposed CWI-CSM framework provides static, physically based hydrological thresholds and that the application does not demonstrate predictive capability. We moderated the statements concerning operational LEWS applications and explicitly identified their implementation and validation against time-stamped, hydrologically characterized landslide events as future developments. We also clarified the interpretation of the calibrated cohesion in relation to failure-depth uncertainty and expanded the discussion of spatially homogeneous versus distributed soil properties. Finally, we reorganized the Discussion and Conclusions and updated the Introduction and literature review to better distinguish the methodological contribution demonstrated here from its broader potential applications.

Below, we detail a point-by-point response to the referee’s comments and outline the corresponding revisions made to the manuscript.

Our reply is in italic. Indications of the changes proposed for the manuscript are highlighted in red.

EC/Referee #3: The manuscript presents an interesting extension of the classical physically based slope stability framework by introducing the concept of Critical Soil Moisture (CSM), aimed at complementing the traditional Critical Wetness Index (CWI) under unsaturated conditions. The study is well written, mathematically sound, and addresses a topic of clear relevance for physically based landslide hazard assessment. However, I believe that some aspects deserve further clarification before publication. In particular, the manuscript would benefit from a clearer definition of the scope of the proposed framework and of what is effectively demonstrated by the presented application.

GENERAL COMMENTS

Throughout the manuscript, the proposed CSM is presented as a physically based hydrological threshold with potential applications for rainfall-induced landslide initiation and early warning systems. However, the adopted validation necessarily relies on the spatial agreement between predicted unstable areas and a historical landslide inventory. As acknowledged by the authors, the available inventory does not contain information on the timing of the landslide events nor on the hydrological conditions at failure. Consequently, the presented analysis primarily demonstrates the spatial consistency of the proposed framework with the observed distribution of landslides, rather than directly demonstrating the predictive capability of CSM as an operational triggering threshold. Although the authors appropriately acknowledge that validation against instrumented slopes remains future work, I believe that this distinction should be reflected more clearly in the Discussion and Conclusions when addressing the operational implications of the proposed methodology.

In this respect, some statements in the Discussion appear stronger than what is directly supported by the presented results. For example, the manuscript states that the proposed methodology would reduce false alarms, although no comparison with existing early warning strategies or operational performance metrics is presented. I therefore

encourage the authors to moderate these statements or explicitly frame them as prospective applications rather than demonstrated outcomes.

Reply: *We thank the Referee for these suggestions. First of all, we deleted the sentence the referee refers to. Additionally, we clearly stated that our approach does not aim to make predictions but rather to provide a set of physically based criteria to identify what areas are most susceptible to failure. Specifically, we explicitly stated that the inventory is not time-stamped, that the hydrological state at failure is unknown, and that what the present application therefore demonstrates is the spatial consistency of the derived thresholds with the observed distribution of failures, not their predictive capability as operational triggering thresholds. The same distinction is now carried into the Abstract and into the Conclusions so that a reader encountering only those sections is not left with a stronger impression than the evidence supports. Accordingly, we revised the entire Discussion section to moderate the statement.*

Please, see the following changes (see also reply to comment 1, referee #1):

Abstract (L15-L17): "*The aim is deriving spatially consistent maps of thresholds that identify areas susceptible to rainfall-triggering landslides; the thresholds are therefore 'static' and not predictive"*

3.3 [Case study] Landslide inventory (L264-270): *"…. the records are not time-stamped. …. As previously indicated, there is no information on the time the landslides occurred. Additionally, the ISPRA-IFFI does not include standardized measurements on depth to failure plane or slip surface or of the soil moisture profile and groundwater at the time of failure.*

5.1 [Discussion] Interpretation of the proposed framework (L541-542): "*Finally, we restate that the aim of the framework is to produce spatial distribution thresholds that are spatially consistent with expected or observed distribution of failures and not to provide a predictive tool."*

6 Conclusions (L604-614): *"The primary outcome is a methodology that converts topographic and soil information into static maps of critical hydrological thresholds and delimits the area of the basin susceptible to shallow rainfall-triggered landslides, rather than predicting the failures". … These figures are conditional on the assumed failure-depth distribution and on a uniform loam parameterisation and should be read as such. What transfers to other settings is the procedure. Any catchment for which topography, a soil-water retention curve and plausible mechanical parameters are available can be converted into a field of critical hydrological states without running a hydrological model and without a landslide-triggering database."*

Referee #3: A second aspect concerns the interpretation of the calibrated cohesion. The probabilistic calibration strategy adopted to account for the lack of information on failure depth represents a reasonable solution given the available dataset. Nevertheless, since both failure depth and soil cohesion are effectively uncertain, the physical significance and uniqueness of the calibrated cohesion values remain difficult to assess. As also acknowledged by the authors, the variability of the optimal cohesion among the different realizations is primarily driven by the uncertainty associated with the assumed failure depths. From my understanding, this suggests that different combinations of cohesion and failure depth may produce comparable predictive performances, making the calibrated cohesion difficult to interpret as a uniquely identifiable mechanical property. A more balanced discussion of this aspect would strengthen the manuscript by clarifying to what extent the calibrated cohesion reflects the intrinsic mechanical characteristics of the investigated soils and to what extent it compensates for uncertainties associated with failure depth and other modelling assumptions. Such a discussion would also help readers better assess the transferability of the proposed framework to other study areas.

Reply: *We thank the Editor for this observation, which we share, and which was also raised by Referee #1 and Referee #2. We agree that the calibrated cohesion should not be read as a uniquely identifiable mechanical property of the soils of the Pontaiba catchment, and we have revised the manuscript to say so.*

As Beven (2006) argues, equifinality is not a defect of a calibration but an unavoidable consequence of estimating parameters from a limited amount of data. In the present framework, topography and all soil hydraulic parameters are constrained independently of the landslide inventory, and only two quantities are estimated: the effective cohesion c and the depth to the failure plane z. These two enter the limit-equilibrium condition through terms of comparable magnitude - c and the product of the soil unit weight and z - so they are not separately identifiable from failure locations alone: a deeper failure plane can be compensated by a higher cohesion, and conversely.

The procedure adopted here is therefore best described as a constrained maximum-likelihood estimation solved numerically by Monte Carlo, in which the admissible failure depths are constrained by an empirical distribution from an analogue site and by field observations in the study area (Supplement S3), the admissible cohesion range is constrained by the nature of the soil, and the two-parameter space is searched for the combination that maximizes agreement with the mapped landslide locations.

We propose the following changes:

3.5 Model calibration procedure (L390-391): "*The empirical frequency distribution was then constructed from reported failure depths of shallow rainfall-induced landslides in the geomorphologically comparable terrain."*

Caption of Fig. 6 (Fig. 7 in the old version): *"Empirical frequency distribution of assigned depths to failure plane used in the probabilistic depth assignment procedure, based on reported failure depths from shallow rainfall-induced landslides in a geomorphologically comparable environment (Ramos-Scharrón et al., 2022), highlighting the emphasis on mid-range depths (with a peak at 1.25 m)".*

5.2 [Discussion] Implications for Landslide Early Warning Systems (L565-573): "*Specifically, in the procedure, we assumed that topography and all soil parameters are estimated from knowledge of the site except soil cohesion. If data on landslide depths were available, together with landslide locations and soil moisture at failure, then cohesion could be obtained by simple inversion. In most places, though, we do not have depth of failure. The approach taken here is analogous to a constrained maximum likelihood exercise, numerically solved using a Monte Carlo method. We constrain the possible failure depths with a distribution from an analog site. The range of possible cohesion values is given by the nature of the soil. The goal is then to search the two-parameter space to maximize the probability of producing the observed landslide's locations in the data set. This is standard parameter estimation which implies uncertainty (Beven, 2006), much like any regression of parameter estimate procedure".*

Referee #3: Finally, although the proposed framework is presented as a spatially distributed approach, in the case study almost all hydraulic and mechanical soil properties are assumed spatially uniform across the basin, whereas the spatial variability of the resulting CSM and CWI patterns is largely controlled by topography. This assumption is reasonable for the selected application and is appropriately justified by the available datasets. Nevertheless, the manuscript would benefit from a more nuanced discussion of how the proposed framework would perform in areas characterized by stronger spatial heterogeneity of soil properties. In particular, it would be useful to clarify to what extent the distributed nature of the resulting thresholds reflects the underlying terrain morphology rather than the spatial variability of hydraulic and geotechnical properties.

Reply: *We thank the reviewer for this comment, which is closely related to comment #5 of Referee #2. We agree that, in the present application, the spatial variability of CWI and CSM is dominated by topography, because a single loam parameterization was adopted across the catchment.*

Two points should be distinguished. First, the uniform parameterization is a property of this application, not of the framework. The soil information available for the Pontaiba catchment (SoilGrids at approximately 250 m resolution and HWSD v2) resolves at most two texture classes, whose mean compositions both fall within or adjacent to the loam field of the USDA triangle, while mechanical parameters are available only from measurements on a geologically and pedologically analogous basin. Assigning spatially varying hydraulic and mechanical parameters on this basis would have introduced spatial structure that the underlying data cannot support, and would have given the resulting maps an appearance of resolution they do not possess.

Additionally, the proposed formulation is not intrinsically restricted to spatially uniform soil properties. Where spatially resolved information on soil texture, hydraulic properties, bulk density, friction angle, cohesion, or other relevant parameters is available, these quantities can be assigned spatially and the analytical CWI and CSM thresholds can be calculated accordingly for each location. In such applications, the resulting threshold patterns would reflect the combined effects of terrain morphology and spatial variability in hydro-mechanical properties.

We have added a paragraph to the new Discussion section 5.3 stating (i) that the distributed character of the present maps derives largely from terrain morphology, (ii) that this reflects the resolution of the available soil data rather than a structural limitation of the method, and (iii) that in catchments with strong lithological or pedological contrasts the framework would be expected to produce threshold patterns not aligned with topography, and that its behavior in such settings remains to be tested. See the following proposed changes.

5.3 [Discussion] Limitations and future perspectives (L585-594): "*Model performance and sensitivity analyses further highlight the influence of uncertainties in geotechnical parameters and failure depth. This reflects the choice of assuming homogeneous stratigraphic conditions and homogeneous mechanical parameters across the basin. However, the real complex conditions, for example of heterogeneous stratigraphic soils, may be easily taken into account when data are available. Morphometric approaches or remote sensing-based methodologies for estimating spatially distributed soil thickness (e.g., Zizioli et al., 2013, Yurdakul et al., 2025) provide a useful alternative, where direct observations of soil depth are unavailable, that might be considered in further development. Additionally, the application showed that the distributed character of the obtained maps derives largely from terrain morphology; this reflects the resolution of the available soil data rather than a structural limitation of the method. In catchments with strong lithological or pedological contrasts the framework would be expected to produce threshold patterns not aligned with topography. Its behavior in such settings remains to be tested.*

Referee #3: Overall, I recommend that the Discussion and Conclusions more clearly distinguish between the methodological advances demonstrated by the present application and the broader operational perspectives of the proposed framework. This would help readers better appreciate both the strengths of the proposed methodology and the aspects that still require validation in future studies.

Reply: *As suggested by referee #2 and reported in relative reply, we have split the Discussion and Conclusion sections and we have structured the Discussion into subsections as follows:*

5 Discussion; 5.1 Interpretation of the proposed framework; 5.2 Implications for Landslide Early Warning Systems; 5.3 Limitations and future perspectives. 6 Conclusions.

In the following, the paragraphs added to the Conclusions section:

6 Conclusions (L601-613): *6 In this work we proposed a physically-based framework to analytically derive water content thresholds for rainfall-triggered shallow landslides by inverting a slope stability equation. In such a procedure, we accounted for the full continuum of hydrological states, from dry conditions through unsaturated regimes to full saturation, identifying regions of stability in terms of two hydrological variables, the Critical Soil Moisture (CSM) and the Critical Wetness Index (CWI). The primary outcome is a methodology that converts topographic and soil information into static maps of critical hydrological thresholds and delimits the area of the basin susceptible to shallow rainfall-triggered landslides, rather than predicting the failures.*

For the Pontaiba catchment, the calibration returned an effective cohesion of 9.4 kPa (95% interval 5.8-9.6 kPa) and, at the modal failure depth of 1.25 m, classified 72.4% of the basin as unconditionally stable, 26.7% as groundwater-controlled and 1.0% as moisture-controlled, with observed landslide frequency rising sharply beyond 32° and extending into the CSM-controlled domain to about 65°. These figures are conditional on the assumed failure-depth distribution and on a uniform loam parameterisation and should be read as such. What transfers to other settings is the procedure. Any catchment for which topography, a soil-water retention curve and plausible mechanical parameters are available can be converted into a field of critical hydrological states without running a hydrological model and without a landslide-triggering database.

SPECIFIC COMMENTS

Referee #3: The Introduction gives considerable attention to the limitations of current operational landslide early warning systems, particularly their reliance on empirical rainfall thresholds and the need for approaches that better represent hydromechanical processes. This discussion suggests that the manuscript will focus on an operational early warning framework. However, the present study mainly proposes a theoretical hydromechanical approach, while its application to operational early warning systems is only discussed as a possible future development. I therefore suggest slightly revising the Introduction to better match the actual scope of the manuscript and to make this point clearer to the reader. In addition, the statement that most operational systems still rely on empirical rainfall thresholds appears somewhat strong. In recent years, an increasing number of studies have proposed hydrometeorological approaches that integrate soil moisture, hydrological indices, groundwater conditions, or hydrological modelling to improve landslide prediction and early warning. I therefore suggest slightly moderating this statement and updating the supporting literature to provide a more balanced overview of recent developments in the field, as the following ones:

https://doi.org/10.1016/j.enggeo.2026.108542

https://doi.org/10.3390/w10091274

https://doi.org/10.5194/nhess-25-169-2025

https://doi.org/10.5194/nhess-25-4907-2025

https://doi.org/10.1007/s10346-023-02132-5

Reply: *As suggested also by Referee #1 we have revised the introduction to provide a more recent overview of the LEWS, although these are not the main scope of our work. Therefore, we tried to better address the actual scope of the manuscript and we have included the suggested new references. Please, see following changes.*

1 Introduction (L36-L47): "*From an operational standpoint, hazard assessment and early warning systems increasingly rely on threshold-based approaches, which integrate rainfall information with hydrological indices, soil moisture data and real-time monitoring to anticipate failure conditions and issue alerts (Distefano et al., 2023; Mirus et al., 2018; Mirus et al., 2025; Palazzolo et al., 2025; Piciullo et al., 2018; Segoni et al., 2018a, b; Zhang et al., 2026) However, most of the operational rainfall and soil moisture thresholds are essentially derived through empirical and data driven methods, which require high quality and reliable databases consisting of landslide events with the corresponding triggering hydrometeorological variables (i.e. rainfall, soil moisture) (Bogaard and Greco, 2018; Guzzetti et al., 2020). Additionally, empirical and data-driven rainfall thresholds do not explicitly represent hydromechanical processes, limiting their ability to anticipate failures under evolving climatic and hydrological conditions (Guzzetti et al., 2020). Significant advances have been made in this direction, with methodologies that have improved, for example, the way how the empirical thresholds are derived (e.g. using Artificial Neural Network, ANN, tools), the inclusion of antecedent conditions, and the use of reanalysis data (e.g. Mirus et al., 2018; Mirus et al., 2025; Greco et al., 2023; Palazzolo et al., 2025, Distefano et al., 2023, Zhang et al., 2026)".*

1 Introduction (L82-L87): "*To address this need, the present study proposes a physically-based framework to analytically derive thresholds at basin scale that include the full continuum of hydrological states. We introduce a complementary metric called Critical Soil Moisture (CSM), which extends the Montgomery-Dietrich approach, by incorporating unsaturated soil mechanics via the extended Mohr–Coulomb criterion (Bishop, 1955) and the van Genuchten (1980) soil-water retention model in the procedure of slope stability model inversion. The approach is particularly useful when reliable data is limited or not available to carry out empirical studies. No hydrological model is involved in the procedure."*

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