Referee #1

RC1: 'Comment on egusphere-2026-1599', Anonymous Referee #1, 20 Jul 2026

We sincerely thank the reviewer for the thorough and constructive evaluation of our manuscript. In response to the comments, we clarified the novelty of the proposed CWI–CSM framework with respect to existing physically based models and strengthened its physical interpretation. We expanded the discussion of the calibration strategy, explicitly addressing equifinality and uncertainty in failure depth. We also clarified the transition between the CWI and CSM domains and better defined the applicability and limitations of the infinite-slope formulation. Finally, we moderated the claims regarding operational LEWS applications and reorganized the Discussion and Conclusions, as suggested.

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.

Referee #1: The manuscript "Spatially distributed water content thresholds for rainfall-induced landslide initiation" presents an extension of the classical Montgomery and Dietrich (1994) physically based slope stability framework by introducing the concept of Critical Soil Moisture (CSM), aimed at explicitly accounting for the stabilizing contribution of matric suction under unsaturated conditions. The proposed framework combines the traditional Critical Wetness Index (CWI), applicable to groundwater-controlled instability, with a complementary threshold based on unsaturated soil mechanics. The methodology is applied to a small Alpine catchment, where sensitivity analyses and a probabilistic calibration procedure are used to derive spatially distributed instability thresholds.

General comments:

The topic is certainly interesting and relevant for the readership of the Journal, since the attempt to extend the classical Montgomery and Dietrich framework toward unsaturated conditions has potential implications for physically based landslide hazard assessment and early warning systems. Furthermore, the manuscript is generally well written, the mathematical development is rigorous, and the figures effectively illustrate the proposed concepts.

However, I believe that the manuscript requires major revisions before it can be considered for publication. Although the proposed framework is mathematically consistent, several aspects require further clarifications. In particular, the manuscript would benefit from a clearer distinction between the mathematical part of the proposed approach and the physical interpretation of the resulting stability scenarios. Moreover, the calibration strategy and practical applicability of the proposed thresholds require a more critical discussion. Specifically, I encourage the authors to address the following points:

1. **Novelty with respect to previous physically based models:** the manuscript presents the Critical Soil Moisture (CSM) framework as a substantial extension of existing physically based slope stability approaches. However, unsaturated soil mechanics has already been incorporated into several, well-known physically based models (e.g., TRIGRS model). Thus, the authors should clearly explain the novelty of

their work, how it differs from previous approaches, and why the proposed CSM represents a genuine advancement over existing physically based models

Reply: *The presented methodology does not employ any spatially-distributed and physically based simulation/predictive models such as TRIGRS (Baum et al., 2002) or similar (e.g. tRIBS-Landslide by Lepore et al., 2013, GEOtop by Simoni et al., 2008, and so on). The objective is deriving maps of regions susceptible to shallow landslides as a function of topography, soil types, and critical soil moisture. The regions are obtained by inverting infinite slope stability equations. To our knowledge, similar approaches exist only for cohesionless soils and under the hypothesis of dry soils or saturated soils due to the presence of groundwater in steady state conditions (Montgomery and Dietrich, 1994), thus neglecting the transient and the unsaturated soils (Iverson, 2000). Our approach leads to two complementary state-variable thresholds, the Critical Wetness Index (CWI) and the newly proposed Critical Soil Moisture (CSM), which respectively characterize groundwater-controlled and unsaturated-moisture-controlled instability for particular slopes, and soil types.*

Most of the operational tools of Landslides Early Warning System (LEWS) rely on rainfall and soil moisture thresholds derived essentially 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). Significant advances have been made in this direction, with methodologies that have improved, for example, the way how the empirical thresholds are derived using ANN tools, the inclusion of antecedent conditions, the use of reanalysis data (e.g. Mirus et al., 2018; Greco et al., 2023 Palazzolo et al., 2025, DiStefano et al., 2023, Zhang et al., 2026).

All these works are valuable, but their development depends on the availability of large landslide inventory and hydro-meteorological dataset, and on the data quality.

Our approach uses physically-based thresholds (not empirical), particularly useful when reliable data is limited or not available to carry out empirical studies. An additional advantage is that, if need be, the required soil moisture or wetness index, could be estimated from any hydrological model to derive maps of landslide hazards.

To better clarify all these aspects, we include the following changes in the revised manuscript (in red), introducing sub-sections into the Discussion section.

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*"

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 (L58-L63): *"No hydrological model is needed to solve the inversion, therefore this approach differs from the subsequent implementations that couple advanced spatially- distributed modeling of hydrological processes with the stability model (Arnone et al., 2016; Baum et al., 2008, 2010; Brilli et al., 2025; Capparelli and Versace, 2011; DiBiagio et al., 2024; Dietrich et al., 1995; Godt et al., 2008; Iverson, 2000; Lepore et al., 2013; Masi et al., 2023; Medina et al., 2021; Rossi et al., 2013; Sannino et al., 2024; Sannino et al., 2025; Simoni et al., 2008; Wu and Sidle, 1995).*

1 Introduction (L77-L78): *"… however, none of them provide an inversion approach which identifies critical thresholds because their primary objective is to predict the temporal evolution of slope stability under prescribed rainfall or infiltration forcing.*

5.1 [Discussion] Interpretation of the proposed framework (L518-524): "*Numerous physically based slope stability models already incorporate unsaturated soil mechanics to simulate rainfall infiltration and the transient evolution of slope stability (Baum et al., 2008; Baum et al., 2010; Brilli et al., 2025; Capparelli and Versace, 2011; DiBiagio et al., 2024; Dietrich et al., 1995; Godt et al., 2008; Iverson 2000; Lepore et al., 2013; Masi et al., 2023; Medina et al., 2021; Rossi et al., 2013; Sannino et al., 2024; Sannino et al., 2025; Simoni et al., 2008; Wu and Sidle, 1995). The contribution of the present study is therefore not the inclusion of matric suction itself, but the analytical derivation of physically-based hydrological thresholds that explicitly accounts for the continuum of the hydrological states. The thresholds are expressed in terms of Critical Wetness Index (CWI) and Critical Soil Moisture (CSM), by providing a spatial characterization of the critical groundwater and soil-moisture conditions potentially associated with slope failure".*

5.1 [Discussion] Interpretation of the proposed framework (L529-531): "*Our approach directly targets the underlying failure mechanism, by defining analytically the critical groundwater and soil-moisture states required to satisfy the stability criterion across the full hydrological continuum, i.e. from unsaturated to saturated conditions.*

5.2 [Discussion] Implications for Landslide Early Warning Systems (L550-556): "*Operational LEWSs commonly rely on empirical or data-driven rainfall threshold approaches (Guzzetti et al., 2008; Bogaard and Greco, 2018; Segoni et al., 2018; Guzzetti et al., 2020). Although significant advances have been made in terms of methods and integration with hydrological variables and antecedent conditions (e.g. Mirus et al., 2018; Greco et al., 2023; Palazzolo et al., 2025, DiStefano et al., 2023, Zhang et al., 2026), the definition of such type of thresholds strictly depend on the quality of landslide archive and the corresponding hydro-meteorological dataset, not always available. The proposed CWI–CSM thresholds are completely physically-based, thus particularly useful when reliable data is limited or not available to carry out empirical studies".*

2. **Calibration strategy:** In my opinion, the calibration procedure represents the weakest aspect of the manuscript. If I understood well, the authors calibrate soil cohesion by maximizing the Youden index after generating 500 synthetic scenarios of failure depth. However, both cohesion and failure depth are treated as unknown variables (as acknowledged by the authors themselves). Consequently, the optimized cohesion may partly compensate for uncertainties in the assumed failure depths rather than representing a physically meaningful mechanical parameter. In this respect, the authors themselves, at lines 446–447, assert that "The observed spread in optimal cohesion values among realizations primarily reflects the uncertainty associated to the depth to the failure plane...". This statement is important since it implicitly recognizes that the uncertainty in the calibrated cohesion is largely driven by the uncertainty in the assumed failure depths. In other words, a better constraint on the failure depth would likely result in a more robust estimate of soil cohesion.

- This introduces a potential equifinality problem, for which different combinations of cohesion and failure depth may produce similar predictive performances. The manuscript would therefore benefit from a more explicit discussion of this issue, particularly regarding the physical significance and robustness of the calibrated cohesion values.
- Additionally, it would be useful to discuss whether the results are sensitive to the number of realizations (500). For example, would a substantially lower number of realizations produce similar optimal cohesion values?

Reply: *As discussed by Beven (2006), "… many different representations (model inputs, model structures, model parameter sets, model errors) might be consistent with the measurements with which the predictions are compared in calibration (allowing for the errors associated with those measurements). Equifinality is endemic to this type of environmental modelling." In essence, equifinality is always a possibility, more or less serious, depending on the number of parameters that are estimated from a limited amount of data. In this methodology we assumed that topography and all soil parameters are estimated from knowledge of the site except soil cohesion, which is important in the unsaturated zone. 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, and it implies uncertainty, much like any regression of parameter estimate procedure.*

We revised the manuscript as follows:

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. Then 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".*

Regarding the number of realizations, we repeated the calibration using 250, 500, and 1000 failure-depth realizations and found that the optimal cohesion values and overall calibration results were very similar across

the three cases. This indicates that the results are not significantly sensitive to the number of realizations within this range. Accordingly, 500 realizations were considered sufficient, which is a compromise between computational efficiency and adequate sampling of the assumed failure-depth uncertainty.

3. **Physical interpretation of the transition between CWI and CSM domain:** one of the main conceptual assumptions of the proposed framework is the existence of two distinct stability regimes separated by the critical slope angle ω**, below which instability is controlled exclusively by groundwater conditions (CWI) and above which it is controlled by unsaturated soil moisture (CSM).

- While this distinction naturally emerges from the mathematical inversion of the adopted equations, its physical interpretation deserves further clarification. Matric suction contributes to the shear strength of unsaturated soils irrespective of slope angle. Therefore, its stabilizing effect is not expected to disappear below a specific threshold inclination. Conversely, groundwater is not necessarily the only controlling mechanism for moderate slopes. Thus, I encourage the authors to clarify that the proposed CWI and CSM domains should be interpreted as the predominant instability regimes predicted by the model, rather than mutually exclusive physical mechanisms. A more detailed discussion of the physical meaning and limitations of this transition would considerably strengthen the conceptual framework. In this respect, the manuscript concludes that instability on very steep slopes may occur under unsaturated conditions without the development of a groundwater table. This conclusion only relies on mathematical formulation, while its interpretation in terms of real field conditions deserves a further discussion. In fact, in many mountain environments, and particularly in Alpine areas, very steep slopes are commonly affected by failure mechanisms that differ from shallow translational landslides, including debris avalanches, debris flows, rockfalls, or mixed soil-rock failures. Consequently, it would be useful to better define the geological and geomorphological conditions under which the proposed CSM framework remains applicable, especially in relation to the characteristics of the study area.

Reply: *The concept of critical threshold values, in general, refers to the minimal conditions, in terms of slope and water content, that must be met to trigger a failure, according to the equilibrium of forces. If the actual hydrological conditions are worse than the 'minimal', then the failure is likely possible, otherwise not.*

We agree with the situations described by the referee, but there is no contradiction. In slopes less than a certain value, let's say ω, even the worst conditions in terms of water content, complete saturation at the potential failure plane, will not be 'sufficient' to trigger any failure. If slope is greater than ω*, positive pore-water pressure is required to reach the critical condition, i.e., 'minimal' conditions are given by pressure that can be conceptually associated to the presence of a groundwater or to a perched saturation. Hillslopes with slopes greater than ω* without groundwater and rather with soils where negative pressures easily arise, will be likely stable, since suction provides a stabilizing contribution. This is true for hillslopes until a certain slope, which is ω**. For slopes greater than ω**; even the stabilizing effect of the negative pressures may not be sufficient to guarantee the stability; therefore, when positive pore pressure arises at the potential failure depth in a very steep hillslope (ω >ω**) failure is likely possible. Although in Figure 2 (below) there is the appearance of "discontinuous" indices, CWI and CSM, the state variable, i.e. pore pressure or matric potential, and thus the threshold line, changes continuously from negative to positive depending on whether the failure occurs in a plane that is within the unsaturated or saturated regions, respectively.*

We revised the terminology throughout the manuscript accordingly and explicitly state that this mathematical transition does not imply an abrupt disappearance of either suction or groundwater effects. See following changes.

2. Methodology (L177-179): "*The concept of critical thresholds values, in general, refers to the minimal conditions, in terms of slope and water content, that must be met to trigger a failure, according to the equilibrium of forces. If the real conditions are worse than the 'minimal', then the failure is likely possible, otherwise not".*

2. Methodology (L184-196): "*… In other words, in slopes less than ω* even the worst conditions in terms of water content (CWI=1 at the potential failure plane), will not be 'sufficient' to trigger any failure. If slope is greater than ω*, positive pore-water pressure is required to reach the critical condition, i.e., 'minimal' conditions are given by pressure that can be conceptually associated to the presence of a groundwater. Hillslopes at slope greater than ω* without groundwater and rather with soils where negative pressures easily arise, will be likely stable, since suction provides a stabilizing contribution. This is true for hillslopes until ω**. For slopes greater than ω**, even the stabilizing effect of the negative pressures may not be sufficient to guarantee the stability; therefore, when positive pore pressure arises at the potential failure depth in a very steep hillslope (ω >ω**) failure is likely possible. Although two equations were employed to analyze hillslope stability, with respect to wetness index and soil moisture, CWI and CSM provide a continuous demarcation of the topography into stable and unstable from gentle to steep slopes in terms of continuous pore pressure, either positive (CWI) or negative (CSM). Therefore, the mathematical transition does not imply an abrupt disappearance of either suction or groundwater effects. Notably, the CWI and CSM converge at the condition where the groundwater table is at the depth of failure surface and yet the soil remains saturated at that depth".*

5.1 [Discussion] Interpretation of the proposed framework (L535-540): "*The slope-dependent transition between the CWI- and CSM-predominant domains should be interpreted as a transition in the hydrological state required to reach the critical stability condition, rather than as a sharp separation between mutually exclusive failure mechanisms. Although Fig. 2 shows a change in the indices, i.e. CWI and CSM, appearing as a discontinuity, the state variable, i.e. pore pressure or matric potential, is continuous changing from negative to positive depending on whether the failure occurs in a plane that is within the unsaturated or saturated regions, respectively.*

With regard to the stability across very steep slopes, we agree with the referee that analyses require particular caution. We are aware that the infinite-slope formulation is appropriate only for shallow failures within a continuous soil mantle and with an approximately planar, as specified at Line 99, slope-parallel failure surface. In fact, the choice of the study area and the selection of the historical landslides from the archive was done according to this predominant mechanism (Lines 225). Clearly, the analysis at basin scale may easily include areas that do not meet these conditions. We expanded the Discussion to explicitly define the applicability domain in the contest of Alpine regions, as follows:

5.3 [Discussion] Limitations and future perspectives (L580-584): *"The model is appropriate only for shallow translational landslides where the failure surface is approximately parallel to the ground surface and it is not suitable for other failure mechanisms (Fredlund and Rahardjo, 1993) . For this reason, the application of the methodology to our study area is limited to this type of landslides, as demonstrated by the historical events shown in Fig. 3, although in the Alpine area very steep slopes are commonly affected by other failure mechanisms such as debris avalanches, debris flows (Arnone et al., 2023).*

4. **Practical applicability for early warning systems:** the manuscript repeatedly emphasizes the possible application of the proposed approach within operational landslide early warning systems. However, operational implementation would require spatially distributed information on different parameters, such

as: soil moisture, groundwater conditions, soil mechanical parameters, failure depth. Obtaining all these variables with sufficient spatial and temporal resolution remains challenging in most operational contexts. I therefore encourage the authors to moderate some of their statements regarding operational applicability or to discuss more explicitly the practical limitations and potential implementation strategies.

Reply: *We thank the referee for this observation. The intention of this study was not to present a complete operational Landslide Early Warning System (LEWS), but rather to develop a physically-based framework that defines the critical hydrological states to be then used in a LEWS. According to the referee suggestion, we have moderated our statements throughout the manuscript and clarified that the proposed methodology represents one component of a physically based early warning system. See the following:*

5.2 [Discussion] Implications for Landslide Early Warning Systems (L545-556): "*The CSM and CWI metrics have the potential to provide operational benefits for Landslide Early Warning Systems (LEWS) and may represent one component of a physically-based early warning system. Real time evolution of soil moisture and/or groundwater levels can be obtained through hydrological monitoring networks, remote sensing products, or spatially-distributed hydrological models capable of estimating the evolving hydrological conditions; measurable or model-forecasted hydrological variables could be then directly comparable with the critical thresholds derived through our methodology to ultimately derive maps of landslide hazards. Operational LEWSs commonly rely on empirical or data-driven rainfall threshold approaches (Guzzetti et al., 2008; Bogaard and Greco, 2018; Segoni et al., 2018; Guzzetti et al., 2020). Although significant advances have been made in terms of methods and integration with hydrological variables and antecedent conditions (e.g. Mirus et al., 2018; Greco et al., 2023; Palazzolo et al., 2025, DiStefano et al., 2023, Zhang et al., 2026), the definition of such type of thresholds strictly depends on the quality of landslide archive and the corresponding hydro-meteorological dataset, not always available. The proposed CWI–CSM thresholds are completely physically-based, thus particularly useful when reliable data is limited or not available to carry out empirical studies.*

Minor comments:

5. **Introduction:** The Introduction provides a large overview of the relevant literature. However, several paragraphs are rather long and could be shortened to improve readability. In this respect, some concepts-particularly the role of matric suction and the evolution of physically based slope stability models-are discussed repeatedly.

Reply: *We thank the referee for this helpful suggestion. We have revised the Introduction to improve its readability and reduce conceptual repetition. However, we should note that Referee #2 and Referee #3 asked us in parallel to broaden the coverage of recent physically based distributed models and of hydro-meteorological early warning approaches, which required additional material. As an effect, the Introduction is more tightly organized but of comparable overall length.*

6. **line 115 "a completely dry situation":** this expression may be misleading from a soil mechanics perspective, since natural soils always retain a residual water content. Since the proposed approach explicitly adopts the van Genuchten retention curve, it would be more appropriate to refer to "groundwater-free conditions" or "residual moisture conditions" rather than completely dry soil.

Reply: *We thank the referee for this suggestion. We slightly modified the terminology through the manuscript depending on the context.*

7. **Line 365:** there is a typo

Reply: *We thank the referee for identifying this typographical error. We have edited it.*

8. **"Discussion and conclusions":** this section would benefit from a clearer distinction between the Discussion and the Conclusions. In its current form, methodological considerations, comparison with previous studies, interpretation of the results, limitations of the proposed framework, and concluding remarks are presented together, making the final part of the manuscript somewhat difficult to follow. Separating the Discussion from the Conclusions-or, alternatively, restructuring the current section with a more explicit organization-would considerably improve readability and help to highlight the main "take-home" messages of the study.

Reply: *We thank the referee for this suggestion. We have split the Discussion and Conclusion sections. Additionally, we have structured the Discussion into subsections for the sake of clarity:*

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.

9. **Figures 5 and 10:** The colour scales adopted in these figures are somewhat difficult to distinguish in some portions of the maps. Increasing the colour contrast would improve readability.

Reply: *We thank the referee for this helpful suggestion. We have revised both figures by adjusting the colour scales to improve visual contrast and facilitate clearer distinction between the mapped stability classes. The figures have also been regenerated at improved graphical quality to ensure a clearer and sharper presentation in the revised manuscript.*

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