To the Editor Office of Natural Hazards and Earth System Sciences

Thank you for giving us the opportunity for the minor revision of the manuscript entitle "Constructing physical-based rainfall landslides prediction model: Insights from rainfall threshold curves database of slope units" to your prestigious journal.

All the revised sections have been clearly highlighted in this version. The authors express their deep gratitude to the editors and reviewers for their concern and assistance with the manuscript.

Thank you for your interest and consideration.

Best Regards,

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Author's response

Comment 1: At line 5 you cite a situation in US from He et al., 2016, but they also cite (Spiker and Gori, 2003). This approach is not feasible. You should cite the authors who presented the situation directly. In this situation, I would recommend more recent statistics or another way of presentation.

Authors： Thanks for this comment. We have revised the citation way of text in the line 5.

Comment 2: The manuscript still has many errors: a lack of spaces between words and parentheses. Please resolve all these issues.

Authors： Thanks for this comment. We checked the manuscript and revised these errors.

For RC1 comments

Comment 1: The reviewer asks what are the resolutions of QPE and QPF. You respond that it is 1 km, but you did not mention that in the modified text.

Authors： Thanks for this comment. We have added content regarding resolution in the revised version, please see the text in the line 4 and line 7-8, page16.

Comment 5: The reviewer asks to be consistent with the abbreviations, but you are not: the missing alarm rate appears several times as an abbreviation and as it is spelled throughout the paper. Please resolve this issue.

Authors： Thanks for this comment. We checked the consistency of the abbreviations, and the inconsistencies (such as missing alarm rate) were corrected in the revised version.

Comment 6: The reviewer asks for a brief justification for the number of soil layers, and you agree to include some references. Where is the value justified? Please indicate in the tracked changes.

Authors： Thanks for this comment. We have included relevant references to clarify this issue, and they are highlighted in the References section. The references are as follows:

Zhang, S., Ma, Z., Li, Y., Hu, K., Zhang, Q., Li, L.: A grid-based physical model to analyze the stability of slope unit, Geomorphology, 391, 107887, https://doi.org/10.1016/j.geomorph.2021.107887, 2021.

Zhang, S., Zhao, L., Delgado Tellez, R., Bao, H.: A physics-based probabilistic forecasting model for rainfall-induced shallow landslides at regional scale, Nat. Hazards Earth Syst. Sci., 18, 969-982, https://doi.org/10.5194/nhess-18-969-2018, 2018.

Comment 7: The reviewer requests the removal of several references cited incorrectly. Which were these? Please indicate in the tracked changes.

Authors： Thanks for this comment. We have corrected the citation format errors pointed out by the reviewers and highlighted the changes in the reference section. The corrected reference is as follows:

Pradhan, A., Lee, S.R., Kim, Y.T.: A shallow slide prediction model combining rainfall threshold warnings and shallow slide susceptibility in Busan, Korea, Landslides, 16: 6. 47-659, https://doi.org/10.1007/s10346-018-1112-z, 2018.

For RC2 comments

Comment 1: The reviewer asks for more clarity in the novelty and its scientific contribution in the introduction and discussion sections. You agree and list some info, but it is not clear where this appears in the manuscript. For example, in the Introduction section, I only see references added, but no text regarding the above observation. Please indicate in the tracked changes.

Authors: Thanks for this comment. The authors have carefully considered this comment and cautiously believe that it may be more appropriate to add the relevant content in the Discussion section. In Section 5.4, we have clearly stated the novelty and scientific contributions of this manuscript, as follows:

"The method proposed in this paper reduces dependence on landslide inventory data and avoids the issue of excessive computation time associated with physical models. This offers promising application prospects for emergency early warning in regions of third-world countries where landslide inventory data are scarce" .

Comment 2: The reviewer says: "I think the introduction section does not provide a detailed picture of limitations in existing approaches and how your model addresses them". You added some references, but there is no clear reference to the limitations. Please indicate in the tracked changes.

Authors： Thanks for this comment. We carefully examined the reviewers' comments. In the revised manuscript, we have provided references regarding the limitations of machine learning and PINN methods, as the reviewers were primarily concerned about the application limitations of these methods. The corresponding references are as follows:

Li, D., Wang, Z., Guo, H., Zhang, Y., Cheng, X., Yu, Q.: Deep Learning in Slope Stability Ana lysis: Evolution, Challenges, and Future Directions, Geotech. Geol. Eng., 43(8), 1-48, https://doi.org/10.1007/s10706-025-03424-4, 2025.

Moeineddin, A., Seguí, C., Dueber, S., Fuentes, R.: Physics-informed neural networks applied to catastrophic creepping landslides, Landslides, 20(9), 1853-1863, https://doi.org/10.1007/ s10346-023-02072-0, 2023.

Comment 3: The reviewer is skeptical about the slope unit partitioning, but you only list references without clearly resolving this issue in the manuscript. Please indicate in the tracked changes where you discuss this.

Authors： Thanks for this comment. In the revised version, we have added the relevant content. Please see line 26-30, page 4. The relevant content is as follows：

"Currently, there are various methods for extracting slope units, each requiring distinct key parameters and yielding different extraction results (Wang et al., 2023). Among these, the MIA-HSU method generates slope units with homogeneous slope gradient and aspect within each unit. In recent years, it has been utilized in rainfall landslide early warning systems in the mountainous regions of southwestern China(Wang et al., 2021,2025) ".

Comment 4 : The reviewer asks for a sensitivity analysis, and while you mention using it previously, you don't make modifications to the manuscript to reflect this.

Authors： Thanks for this comment. We have supplemented the corresponding modifications to reflect the previous research work on sensitivity analysis. Please refer to line 15-18 at page 20. The modified text is as follows：

"Given that existing research indicates that antecedent effective rainfall (AER) and daily accumulated rainfall are crucial sensitive factors affecting the forecasting performance of HSU-based physical models(Wang et al., 2023), the AER of these HSUs, the AER levels assigned by the database and daily accumulated rainfall are presented in Table 4".

Comment 5: The reviewer asks if the Monte Carlo runs are enough, and while you argue for that number, you don't make modifications to the manuscript to reflect this.

Authors： Thanks for this comment. We have made the corresponding modifications, please refer to line 14-15, page 9. The relevant content is as follows:

"The random search times for the sliding surface of each HSU are set to 500 times, then the infinite slope model was used to calculate the safety factor Fs of each potential slip surface as follows"

Comment 6: For this comment, you misunderstood the reviewer's position, and you mention the classification error indices for the ROC analysis, which you argue against.

Authors： Thanks for this comment. The whole authors have carefully examined the reviewers' comments. We cautiously consider that the reviewers are interested in a comparison between the model presented in this paper and other models using performance metrics such as ROC; we have included a comparison between our model and statistical models, please see Section 5.3. Furthermore, evaluation metrics from the ROC method, including accuracy and precision, have been employed to assess the performance of the two models, demonstrating that the model proposed in this paper delivers more satisfactory results.

Comment 9: You should also point out the limitations section that field sampling and experiments are needed for these methods, and how that influences the usability in scarce data areas.

Authors： Thanks for this comment. In the discussion section, we pointed out the limitations of the method proposed in this paper and the future tasks. Please refer to Section 5.4. The relevant content is as follows:

"However, it should be noted that the calculation of HSU_{prob} relies on regional-scale mechanical parameters. Therefore, establishing the distribution of mechanical parameters in Southwest China through field sampling, experiments, and spatial analysis is an important task for the future. Another important task lies in optimizing the model algorithm, because the physical mechanisms of rainfall landslides are extremely complex, and the soil-water coupling process under rainfall involves significant nonlinearity (Apip et al., 2010). Some machine learning methods that incorporate physical frameworks are expected to accurately describe this issue (for example the PINN) .Therefore, improving the model algorithm by combining machine learning with physical approaches is another important task for the future."