We thank the reviewers for taking the time to carefully read our manuscript and provide constructive feedback. We value your efforts in helping us to make improvements to our paper. Below, we provide our responses to each comment and describe the corresponding revisions in the manuscript.

Our response is organized in the following order:

Reviewer Comment
Author Response
Paper Revision (if applicable)

Reviewer #1

Overall, I've found some confusion with the use of landslide "hazard" and "susceptibility" terms. I'd suggest checking the whole text and avoiding misuses of these terms.

We agree with the reviewer that both terms were used in the original manuscript. Because our modeling framework explicitly incorporates seismic and hydrologic forcing (e.g., peak ground acceleration and event-based recharge), the primary outputs of this study are landslide hazard maps. We have revised the manuscript to reflect this more clearly, and have removed or corrected instances where the terms were previously used interchangeably.

Was the Monte Carlo approach used in the Landlab LandslideProbability component somehow constrained considering the ranges of parameters typical of the study area?

The Monte Carlo approach is constrained by soil and vegetation parameter ranges informed by the broader literature for the soil, lithology, and vegetation classes represented in the study area. No region-specific field observations are currently available for parameter calibration at this site. Below, we summarize how gridded data sources and published literature were used to select parameter values and define the assumed statistical distributions. The paper provides further details on the Monte Carlo approach and parameter selections in sections 3.1.1 and 4.1.

The infinite slope stability model parameters were assigned using categorical soil texture, bedrock lithology and land cover and land use information obtained from gridded data sources, which was described in section 4.1. For each soil texture and vegetation category we used parameter ranges compiled from the literature as reported by Strauch et al (2018). For bedrock lithology, we used the literature-based values compiled and reported by Xi et al. (2024a, b). The parameters of the triangular distribution, mode, minimum and maximum were obtained based on the rules developed by Strauch et al (2018), relative to a mode calculated from the literature ranges. Since the purpose of our model is for rapid landslide hazard modeling following earthquakes, we preferred to use fixed rules for parameter selection without specific calibrations. The model performance reported in the study with this methodology shows reasonable model performance within acceptable ranges for the Receiver Operating Characteristics (ROC) analysis reported in the literature.

To improve clarity, on this issue several modifications are made.

In Section 3.1.1. "LandslideProbability component" we have revised the manuscript (Line 213–230.) to explicitly indicate the reasons for using triangular distributions and explain how the parameter ranges are identified. As this section indicates, sampling from parametric probability distributions to solve the factor of safety equation has been an established method, and the triangular distribution has commonly been used in a Monte Carlo approach in the literature.

Finally, at the end of the first paragraph of Section 4, "Data sources and model experiments," we reminded the reader that soil and vegetation parameters were assigned as modal values of triangular distributions using lookup tables linked to map classes, while the anisotropy multiplier was selected from the literature (near line 360).

Was the landslide inventory somehow validated in the field? I'm asking this given that the event is relatively recent.

We did not have direct field validation at the landslide sites. Reviewer 2 also brought up the fact that the bare soil index (dBSI) based methods may not be suitable for this region. To improve robustness and provide a more defensible validation, we have shifted our approach to a terrain-based mapping method over a 10 km^2 area within the Askerhan catchment (Fig. 2). Because this catchment experienced one of the highest coseismic landslide densities in the region and a widespread response to the AR event, lidar data were acquired both before and after the AR. This Lidar-base methodology is described in section 3.3. Validation results for the March 14-15 AR event were provided in section 5.2., and for cosesimic landslides in section 5.4.

Regarding the sampling of non-landslide grid cells, why an equal number of landslide and non-landslide grid cells was selected? I wonder if it would have been better to select a larger number of non-landslide cells than the same number of landslide inventory cells. This is a common approach in such types of analyses.

Following the comment, we revised our sampling approach to better reflect the spatial distribution of mapped landslides. Rather than selecting an equal number of landslide (LS) and non-landslide cells (NLS), we now sample NLS cells in proportion to the total area of mapped landslide polygons in the validation area. This ensures that the number of NLS samples is comparable to the total mapped landslide footprint, providing a more realistic assessment of model performance across the landscape.

This updated approach helps to align us with common practices in landslide susceptibility modeling (e.g., Strauch et al., 2018) and allows us to evaluate the model under conditions that better represent the imbalance between landslide and non-landslide areas. This is expressed to the readers in line 308:

"*Non-landslide cells were sampled outside of the mapped landslide polygons, with a total count equal to the number of cells within all landslide polygons in the validation area, ensuring a comparison proportionately consistent with previous studies while avoiding overlap with source cells (Strauch et al., 2018).*"

How was the slope threshold of 15° used to filter out landslides (line 285) selected?

The 15° slope threshold was selected based on physically-based slope stability analyses, which consistently show that slopes below approximately 15-20° remain stable across the full range of soil saturation conditions (Montgomery and Dietrich, 1994). Below this threshold, the infinite slope stability equation predicts that even fully saturated soils with minimal cohesion are unlikely to fail under rainfall-driven pore pressure alone. We apply this threshold to exclude mapped cells on gentle terrain from the source cell validation dataset. We have clarified this rationale starting in line 295 as follows:

"*This approach focuses the evaluation on locations where slope failure is most plausible, while excluding transition and depositional areas. A slope threshold of 15º and steeper is used to filter out landslide mapping on gentle slopes. Physically based analyses show that slopes below approximately 15–20° remain stable across the full range of saturation conditions (Montgomery and Dietrich, 1994).*"

Line 385: Please add here details on the temporal resolution of the IMERG data used.

Line 414 (Formerly line 385) has been updated to clarify the temporal resolution of the IMERG data. The revised text now reads:

"*Hydrological forcing is represented using daily-aggregated precipitation derived from the IMERG Version 7 product, which provides half-hourly global precipitation estimates.*"

Line 419: Please add here details on how the return period of the rainfall was calculated

Line 453 (Formerly line 419) has been updated to clarify how the return period of the rainfall was calculated. The revised text now reads:

"*Within the study domain, max peak daily rainfall observed by IMERG during the AR event was 104.6 mm d^{-1}, corresponding to an approximate 5-year return period estimated using Weibull* *exceedance probability applied to annual maxima of spatially maximum daily IMERG precipitation over the historical record (2000–2024; Fig. 8b).*"

Some technical suggestions for the figures:

Figs 1 and 2. An inset with the location of the study area would be useful

Insets showing the location of the study area have been added to Figures 1 and 2 to improve spatial context. These updated figures have been attached to this response.

Figs. 3-4. Please add details on the reference system (e.g. EPSG) of the maps

Figure captions for Figures 3 and 4 have been updated to include details on the reference system. The revised captions now indicate that all data are projected in WGS 84 / UTM Zone 10N (EPSG:32610) for Landlab modeling. Example from Fig. 3:

"Illustration of model-data integration of remote sensing products into Landlab as fields: a) Copernicus GLO-30 DEM, b) USGS ShakeMap PGA, c) ESRI Sentinel-2 land use/land cover map, d) root cohesion derived from LULC in panel (c) using literature derived values (Strauch et al., 2018), e) MTA geologic map, and f) HiHydro Ksat. All data are projected in WGS 84 / UTM Zone 10N (EPSG:32610) for Landlab modeling."

Fig. 6. Please check the readability of the text in panels c and d. Moreover, please define all variables in the caption (nnl and nl are missing)

For Figure 6, the font size of the text in panels (c) and (d) has been increased and the lettering bolded to improve readability. Additionally, the caption has been updated to define all variables: "*For each probability bin, the number of grid cells classified as legacy (n_l) and non-legacy (n_{nl}) is indicated in the figure.*"

We also applied similar improvements to Figure 10, increasing font size and bolding letters in panels to enhance readability. The caption now clarifies the variables: "*For each probability bin in (c), the number of grid cells classified as historic data based (n_h) and single-AR based (n_{ar}) is indicated in the figure.*"

Reviewer #2

The reviewer provided major comments in (4) subheadings, which are addressed in order below.

(1) Discussion section must be separated and expanded. The interpretation of results, comparison with previous studies, and implications for hazard response are currently embedded in the Results section. A stand-alone Discussion is needed to: Explain the physical meaning of the observed reduction in critical slope angle under legacy conditions. Situate findings in the context of documented post-seismic

landslide sequences (e.g., Wenchuan, Gorkha, Palu). Clarify how the proposed approach could inform rapid-response hazard assessment and early warning workflows. Explicitly discuss limitations, data uncertainties, and model assumptions.

We agree that a clearer separation and expansion of the Discussion improves the manuscript, and we have revised the structure accordingly by introducing a stand-alone Discussion section and moved some of the contents of the Result section into discussion. We note that direct comparisons with previous studies were not extensively presented in the Results section; nevertheless, we have moved implications for hazard response into the Discussion, where they are now more appropriately contextualized. We retained brief interpretation within the Results section to guide the reader, as these statements are concise and intended to provide immediate context for the presented outputs.

To address the specific points raised by the reviewer, we have expanded the Discussion as follows:

Physical interpretation of **reduced critical slope angle**: We added the following text to clarify the underlying mechanics (line 720):

"*The outcome of this model was reduction in critical slope for landslide probability classes. This implicitly represents the physical effect of seismic weakening on hillslopes, where shaking reduces slope shear strength through degradation of cohesion in soil and rock due to microfracturing, cracking, and regolith loosening, lowering the factor of safety threshold for failure and allowing previously stable slopes to fail at shallower angles (Brain et al., 2017; Keefer, 1984). This pronounced effect suggests that pre-seismic hazard maps likely underestimate post-earthquake risk under identical rainfall forcing, highlighting the need to update hazard analysis following major seismic events.* "

Context within documented post-seismic landslide sequences and implications for hazard response: We expanded the Discussion to better situate our findings within prior work and to clarify the contribution of our approach. Since our work is on modeling at a specific site and earthquake, we felt that one way to situate our findings with other earthquakes (Gorkha, Wenchen, Palu), would be to remind the reader the increased landslide and flood activity in the post-seismic period after these earthquakes, and that our modeling framework that employs historical rainfall data can be proactively applied to post-seismic rainfall driven landslides under earthquake legacy effects.

In addition, we expanded citations to prior studies of post-seismic landslide activity to strengthen this comparison.

Outside of the specific points of the Reviewer, the Discussion section has been expanded significantly to reflect the novelty of the work, implications, and comparison with the current literature. We believe these revisions fully address the reviewer's concerns and substantially improve the clarity, structure, and interpretability of the manuscript.

(2) Clarify novelty and contributions. The manuscript should more clearly articulate what is new relative to prior compound hazard or post-seismic weakening studies. The innovation claims can be strengthened by emphasizing: (a) use of historical extreme rainfall records for pre-event hazard forecasting, and (b) quantitative mapping of legacy-induced slope threshold shifts.

Substantial steps have been taken to improve the clarity of novelty and contributions of the manuscript.

Abstract: We revised the abstract to more clearly highlight the study's novelty, including the integrated treatment of seismic and rainfall drivers, the hazard implications of event sequencing, the incorporation of post-seismic legacy effects, and the pre-event hazard forecasting approach.

Introduction: To more clearly articulate the contribution of our paper we added two new paragraphs to the introduction. These paragraphs discuss the current approaches in the representation of earthquake legacy effects on slope stability (starting line 75), coseismic models and their limitations in the representation of hydrology and the need to develop a rapidly deployable framework that can address the various limitations of the aforementioned literature (starting line 85). These limitations include the absence of earthquake legacy effects, and compound seismic and rainfall drivers, as pointed out by the reviewer.

We then revised the research questions to directly address these needs and to develop models with improved capabilities (lines 122-130). We also revised the final paragraph of the introduction section to more explicitly map our analysis to each of the research questions (starting line 120). We think these modifications set a clearer tone of the novelties proposed in the paper and provides a more organized road map to model development and analyses.

Results:

Before we address the research questions, we edited the results section as needed to affirm that our findings are clearly communicated. First, we show a limited reality-check for the model for predicting hydrologic response (section 5.1), and landslide probabilities for both post-seismic and coseismic conditions (Section 5.2).

Following those, Section 5.3. responds to question (1) by comparing legacy and no legacy effects. Section 5.4. responds to question (2) by comparing model results for dry coseismic, AR-driven wet cosesimic, and AR-driven post-seismic, with earthquake legacy effects. Section 5.5. responds to question (3) by presenting the proposed pre-event forecasting of post-seismic landslide hazard ahead of major post-seismic storm events.

We think that revisions done in the Result section now clarifies the novelty of the paper in various fronts within the specific goal of developing a model that can be used for rapid emergency response and risk mitigation, at least at this stage of our model development.

We have updated the new **Conclusion** section to more clearly articulate the novel contributions of this study relative to prior work on compound landslide hazards and post-seismic landscape response.

In addition, we have expanded the **Discussion** into four subsections (6.1–6.4) to separate model framing, probability realism, sequencing implications, and forward-looking applications, replacing the single combined Discussion and Conclusions section in the original submission.

Section 6.1 retains and expands the original discussion comparing rainfall-driven and seismic landslide models developed separately (Baum et al., 2010; Strauch et al., 2018; Alvarado-Franco et al., 2017; Jibson, 1993; Zang et al., 2020), and adds new discussion of critical-zone hydrology controls on antecedent soil wetness (Montgomery et al., 1997; Anderson et al., 1997; Brooks et al., 2015; Collins et al., 2026; Moon et al., 2026), liquefaction-hazard precedents (Baise et al., 2006), and the decoupling of regional coseismic models from hydrology (Allstadt et al., 2022; Shao et al., 2022; Li et al., 2022; Djukem et al., 2024; Hartmann and Blume, 2024)."

We also expanded discussion of the earthquake-legacy literature (He et al., 2024; Tanyaş et al., 2021a,b; Leshchinsky et al., 2021; Xi et al., 2024a,b) to better position our PGA-linked legacy parameterization relative to prior work.

Section 6.2 discusses the hydrometeorological context before and after the February 6, 2023 earthquake (citing Görüm et al., 2025), the strengths and limitations of our single-layer soil-moisture representation, and reframes model evaluation around the realism/interpretability of predicted probabilities rather than discrimination skill alone (Reichenbach et al., 2018), supported by the new Fig. 11.

Section 6.3 discusses the sequencing result highlighted in the abstract and conclusions, framing compound hazard as a sequencing problem (seismic-then-rainfall vs. rainfall-then-seismic) and connecting it back to the Gorkha and Wenchuan examples discussed in the Introduction.

Section 6.4 is our closing subsection that discusses existing forecast-oriented rainfall-landslide systems (e.g., NASA's LHASA; Kirschbaum and Stanley, 2018), the role of evolving landscape memory in hazard forecasting, and frames our framework as an early step toward updateable 'living' hazard models consistent with emerging Digital Risk Twin concepts (Ghaffarian, 2025; Campforts et al., 2020).

As part of **(2)** the reviewer also listed more specific directed comments.
Model assumptions and uncertainties require further explanation.
The rationale for parameter distributions (triangular distributions for mechanical properties, scaling of hydraulic conductivity, soil thickness interpolation) should be discussed. A brief sensitivity analysis or statement about dominant sources of uncertainty would improve credibility.

In Section 3.1.1. “LandslideProbability component” we have revised the manuscript (Line 379-400) to explicitly indicate the reasons for using triangular distributions and explain how the parameter ranges are identified. As this section indicates, sampling from parametric probability distributions to solve the factor of safety equation has been an established method, and the triangular distribution has commonly been used in a Monte Carlo approach in the literature.

Finally, at the end of the first paragraph of Section 4, “Data sources and model experiments,” we reminded the reader that soil and vegetation parameters were assigned as modal values of triangular distributions using lookup tables linked to map classes, while the anisotropy multiplier was selected from the literature. (near line 360).

We did not do a brief sensitivity analysis or tried to further improve model performance by calibration, as we are presenting a reginal model for rapid deployment and the model should be realistic without any calibration. This perspective is also consistent with regional and global hydrologic models. A risk assessment tool should be able to provide reliable prediction before a hazard event. We added the following a line at the end of section 3.3. *Landslide inventory and model validation* to express this point:

“*This model was not site-calibrated as our goal is to develop a rapidly deployable regional framework that can provide reasonably accurate actionable model predictions. As such model validation only used a single set of results informed by parameter ranges obtained from the literature, described earlier*.”

The rationale for parameter distributions (triangular distributions for mechanical properties, scaling of hydraulic conductivity, soil thickness interpolation) should be discussed. A brief sensitivity analysis or statement about dominant sources of uncertainty would improve credibility.

We have expanded the manuscript to clarify the rationale for key parameter distributions and scaling assumptions, and to explicitly acknowledge their associated uncertainties.

Specifically, we added text in line 213 to justify the use of triangular distributions for mechanical hydrologic parameters: "*Triangular distributions were used to represent uncertainty in soil and vegetation parameters because they provide a way to approximate variability around a central value and defined minimum and maximum ranges when limited information is available. Compared to normal or log-normal distributions, triangular distributions also provide better control on the tails, preventing unrealistic sampling beyond plausible parameter bounds. This approach is commonly used in landslide hazard modeling where site-specific measurements are limited but reasonable parameter ranges can be inferred from previous literature (e.g., Hammond et al., 1992; Selby, 1993; Strauch et al., 2018). In this study, parameter bounds are defined relative to the mode and vary by parameter type, following the Landlab LandslideProbability implementation in Strauch et al (2018). Gridded soil, lithology, and vegetation data products are utilized to infer the mode parameters of the triangular distribution for Ct, Cs, hs, and T. If the grided information is categorical, we used lookup tables to assign parameter values. Soil texture and vegetation parameters are used from the values compiled and reported by Strauch et al (2018). Parameters corresponding to lithology are used from Xi et al. (2024a, b).*"

We further clarified the treatment of hydraulic conductivity (line 379), where scaling is applied to account for anisotropy in lateral versus vertical flow: "*Following the observations of Assouline and Or (2006), gridded K_{sat} values were multiplied by 10 to account for anisotropy in lateral flow. This scaling reflects the tendency for lateral hydraulic conductivity to exceed vertical conductivity in soils, which enhances lateral downslope drainage and reduces local pore pressure buildup. A minimum value of 0.5 m d^{-1} was imposed to avoid unrealistically low conductivity values that would otherwise promote excessive saturation leading to slope instability. These adjustments introduce uncertainty where site-specific constraints on hydraulic anisotropy are unknown.*"

In addition, we expanded the discussion of soil thickness parameterization (lines 369), including literature support for the adopted range and an explicit acknowledgment that this simplified representation does not capture fine-scale variability due to local controls such as lithology and vegetation: "*We enhanced our soil thickness dataset for high-elevation areas by applying an elevation-dependent linear relationship in high-slope regions (>15%), capturing the tendency for thinner soils in steep mountainous hillslopes due to reduced accumulation and increased erosion (Pelletier and Rasmussen, 2009). Applying these additional criteria provide soil thickness values in the 0.3-2.0 m range from low to high elevations. This range is consistent with the reported soil depths on steep mountainous hillslopes and is intended to avoid unrealistically thin or thick soil thickness values that could bias model stability predictions (Strauch et al., 2018). We note that this parameterization introduces uncertainty, as soil thickness can vary substantially due to local controls (e.g., lithology, vegetation) that are not resolved in this formulation.* "

Evaluation of the landslide inventory requires clarification. The rainfall-driven inventory derived from dBSI could be affected by vegetation phenology and bare-soil

misclassification. The authors should add discussion of potential biases and, if possible, provide a few qualitative site-based validation examples.

We agree with the reviewer. To improve robustness and provide a more defensible validation, we have shifted our approach to a terrain-based mapping method over a 10km2 area within the Askerhan catchment (Fig. 2). Because this catchment experienced one of the highest coseismic landslide densities in the region and a widespread response to the AR event, lidar data were acquired both before and after the AR. This Lidar-base methodology is described in section 3.3. Validation results for the March 14-15 AR event were provided in section 5.2., and for cosesimic landslides in section 5.4.

Sequence analysis requires stronger physical interpretation. The finding that "rainfall-before-earthquake" yields the highest hazard is interesting but currently under-discussed. The authors should relate this to known soil saturation effects on shear strength, crack propagation, and regolith weakening.

We have expanded the discussion to provide a stronger physical interpretation of the rainfall-before-earthquake scenario. New subsection 6.3 (line 782) elaborates on the sequencing result highlighted in the abstract and conclusions, framing compound hazard as a sequencing problem (seismic-then-rainfall vs. rainfall-then-seismic) and connecting it back to the Gorkha and Wenchuan examples discussed in the Introduction.

Some methodological details (e.g., component coupling, parameter tables) could be moved to Supplementary Material to improve readability.

We appreciate the reviewer's suggestion. Yes, we moved Table 1 to Supplementary Materials but kept model coupling details in the main text.

Several figures could benefit from clearer contrast between high and low hazard classes.

In the revised manuscript, we have adjusted the color schemes of Figures 5, 6, 7, and 10 to improve contrast between high and low hazard classes, enhancing the readability and interpretability of the hazard patterns. These updated figures have been attached to this response. Figure 8 color scheme has also been updated to reflect NHESS accessibility standards.

The Conclusion should be more concise and emphasize implications rather than repeat results.

We agree with the reviewer that the Conclusion (line 782) should be more concise and emphasize the broader implications of our findings rather than reiterate results. In the revised manuscript, we have streamlined the Conclusion to focus on novel contributions and important findings.

"Regional landslide hazard models have typically treated seismic and rainfall triggers separately. Compound drivers and earthquake legacy effects have mostly been examined in case-specific studies using data-intensive models with limited regional transferability. This paper bridges this gap by developing a rapidly deployable regional framework that integrates seismic and rainfall triggers with post-seismic legacy effects using global datasets.

We developed a rapidly deployable regional landslide hazard framework that integrates seismic and rainfall drivers with earthquake legacy effects using global gridded datasets.

Incorporating earthquake legacy effects substantially improves post-seismic hazard representation by shifting predicted failure out of unrealistically low probability ranges and into more physically meaningful and operationally useful probability classes, as well as provides moderate improvements to model prediction accuracy.

Sequencing of rainfall and seismic forcing shows that hydrologic preconditioning of extreme rainfall amplifies coseismic hazard, while post-seismic rainfall acts on mechanically weakened slopes to sustain elevated hazard during recovery.

Historical extreme rainfall records can be used to generate pre-event post-seismic hazard forecasts that closely reproduce the spatial pattern and probability structure of the March 2023 AR-driven hazard.

The framework provides a practical basis for early warning, emergency response, and mitigation planning in seismic regions, and can be extended toward updateable "living" hazard models as new post-event data become available. Our approach also demonstrates the following points.

- *Across the full study domain, the dry coseismic model yields a conservative performance that reflects dry antecedent conditions and provides a baseline for evaluating added effects of legacy and hydrologic forcing. Accuracy could likely be improved by calibrating soil saturation to February conditions typical to the area.*

- *Incorporating earthquake legacy effects leads to a ~13° reduction in median critical slope for landslide initiation compared to the non-legacy model, indicating that slopes considered stable before an earthquake may become highly susceptible afterward. This suggests that pre-earthquake hazard maps may significantly underestimate post-earthquake risk, reinforcing the need to update hazard assessments following major seismic events.*

- *The scenario where the AR event precedes the earthquakes shows the greatest landslide hazard, pointing to a potentially important hazard scenario not seen in the historical record and highlights the need for more research on how the timing of storms and earthquakes influences landslide risk.*

It should be noted that validating hazard maps remains uncertain, particularly in remote mountainous regions where landslide inventories are often incomplete. An additional and more comprehensive evaluation of model performance would benefit from further field investigations to identify landslides or signs of instability that may have been missed in our satellite and mapping derived inventory. "

Additional Revisions:

Section 3.4 (Model Experiments, line 397) has been updated to more directly address our research questions outlined staring on line 118:

"This study includes three groups of analyses. First, to address question 1, we present the modeled hydrologic response to the March 14-15 AR event and the resulting post-seismic landslide hazard with and without earthquake legacy effects, parameterized as a function of the Peak Ground Acceleration (PGA) following Xi et al. (2024a). These results are corroborated with satellite-derived soil moisture and mapped landslides. Second, we analyze how rainfall-earthquake sequencing influences spatial patterns of coseismic landslide hazard and compare these results with post-seismic legacy model (question 2). Finally, in response to question 3, we illustrate our pre-event forecast approach for post-seismic, rainfall-driven landslide hazards using historical satellite-derived precipitation together with earthquake legacy effects and compare the resulting hazard map with that derived from the March 14–15 AR event. By relying on historical extremes rather than real-time rainfall observations, this approach enables forecasting of high-risk zones before extreme precipitation events, supporting early warning and mitigation efforts."

These changes were made to make the flow of the manuscript more coherent and effective.

We have also added a new figure (Fig. 11) showing the distribution of modeled P(F) across the domain and at mapped landslide locations for the no-legacy, legacy, and historical-extremes scenarios, to visually support the discussion of how legacy effects shift predicted probabilities toward more physically meaningful ranges.

The abstract has been substantially rewritten to reflect the restructured paper: it now explicitly introduces the three research questions, the AR-before-earthquake sequencing result, and the pre-event forecasting approach using historical rainfall records, replacing the more general framing of the original abstract.

The Discussion has been reorganized into four subsections (6.1–6.4) to separate model framing, probability realism, sequencing implications, and forward-looking applications, replacing the single combined Discussion and Conclusions section in the original submission.