In the manuscript, the authors collected and analysed many data related to debris flows that happened in the Jiangjia Gully (JJG). Additionally, they considered some other debris-flow-prone basins all around the world. The authors focus the analyses on identifying statistics on surge sequences such as discharge, velocity interval timing and sediment volume. From these data, they derived a stochastic model to reproduce these surge sequences, validating it with some JJG events and some other events around the world.

The topic is of great importance, and the stochastic model presented is very innovative. There are, however, important shortcomings that have to be addressed:

- **In the paper, it is missing a Section dedicated to the illustration of the JJG basins. Some information is reported in the supplement, but it is still missing much information, such as hydrological aspects, the location of the sediment source area and some geological aspects (all relevant to understanding the basin-scale processes involved in debris flow generation and propagation). Additionally, in the paper, and not in the supplement, it is necessary to highlight where the instrumentation used to register data is located with respect to the sediment source areas.**

Reply: We completely agree that a dedicated section describing the JJG basin is essential for understanding the basin-scale processes.

In the revised manuscript, we have added a comprehensive new section in the main text: Section 2.1 “Landslide and Debris-Flow Processes in Jiangjia Gully”. This new section systematically addresses all the missing information pointed out by the reviewer:

1. Geological aspects: We have described the tectonic setting (bounded by the seismically active Xiaojiang Fault), the lithology (Proterozoic metamorphic rocks including schists, phyllites, and gneisses), and the thick, deeply weathered regolith.

2. Hydrological and climatic aspects: We have detailed the subtropical monsoon climate, the mean annual precipitation (700–1,000 mm), the wet season (May–October), and the short-duration, high-intensity convective storms that trigger the failures.

3. Location of sediment source areas: We have explicitly identified the primary source areas as loose colluviums and historical deposits located in the middle and upstream regions, with a total material storage volume of 1.23×10^{10} m³ (Section 2.1, and shown in Figure 1a-1).

4. Instrumentation location: We have clearly highlighted the monitoring station and the rain gauge network relative to the source areas in Figure 1a-1 and 1a-7. The text now explicitly states that local rainfall is monitored by a network of gauges installed within the source area, and the observation station with monitoring cross-sections is located in the main channel.

- **Some mathematical symbols reported in the equation are not describe making the paper difficult to follow and understand (e.g. sometime the same symbol is written in bolt sometime no). I suggest being consistent throughout the paper and describing each mathematical symbol introduced.**

Reply: We agree that consistent notation is crucial for readability. We have carefully reviewed the entire manuscript and revised all mathematical notations to ensure consistency and clarity:

1. Consistency in bolding: We have removed unnecessary bold formatting from sequence variables (e.g., changing $\{\mathbf{M}\}=\{\boldsymbol{S}_0\}\oplus\{\boldsymbol{S}_R\}$ to $\{\mathrm{M}\}=\{S_0\}\oplus\{S_R\}$ and $\boldsymbol{S}_F$ to S_F) so that the same physical quantities are represented by the same formatting throughout the text.

2. Description of introduced symbols: We have explicitly defined previously missing symbols. For example, M^* is now defined as the rescaled dimensionless magnitude (Section 3.2), and $S_{T(i)}$ is defined as the mass of the i-th failure event (Section 3.4).

3. Differentiation of similar symbols: We have ensured that distinct parameters are not confused. Specifically, ξ is consistently used to denote the Pareto exponent of the failure-mass distribution, while ζ is used exclusively for the Gaussian selection coefficient. This distinction is now explicitly stated in the text and Table 1 notes.

We have made these revisions to ensure the mathematical framework is fully transparent and easy to follow.

- **It is missing the fundamental point on how the four key parameters are evaluated, so all the results could not be reproduced.**

Reply: We completely agree that a detailed description of parameter evaluation is essential for reproducibility.

In the revised manuscript, we have added a comprehensive explanation of how the four key parameters are determined. This is now detailed in the main text (Section 4.1) and the notes of Table 1. Specifically:

1. Poisson intensity (λ): Computed from the source-area extent (A_i, delineated from DEM/remote sensing) and the event rainfall intensity (I_R, from rain gauges), using $\lambda(A_S)=\sum(A_i/A_E)\lambda_E$ and the calibrated relation $\lambda_E\sim\exp(k_T I_R)$ (with $k_T=0.018$).

2. Pareto cutoff (m_0): Estimated from field surveys of typical source scars and regional landslide inventories.

3. Pareto exponent (ξ): Taken from the rainfall-dependent relation $\xi\sim\exp(-k_P I_R)$, calibrated from JJG rainfall experiments.

4. Selection coefficient (ζ): Drawn from a standard Gaussian distribution $N(0,1)$ and requires no site-specific calibration.

We have also added a statement clarifying that ξ and ζ are distinct symbols (Eq. 8 *vs*. Eq. 11). With these explicit formulas, empirical constants, and data sources provided, the full workflow is now fully transparent and reproducible.

- **Some assertions (sometimes strong assertions) are written without references or data that support them (details are listed in the following list).**

Reply: We have carefully checked and revised all the specific instances listed in the reviewer's detailed comments. In summary, we have made the following types of corrections throughout the manuscript:

1. Adding literature support: For physical interpretations (e.g., the inertial dominance in high-concentration surges, the GSD-density relationships), we have added appropriate citations such as Iverson (1997), Wang et al. (2018), and Li et al. (2015).

2. Providing data evidence: For statements regarding sediment limitation, we have referenced

our field data (e.g., only 30% of failure mass contributes to surges; temporal decoupling from hydrographs).

3. Toning down strong assertions: We have replaced subjective or overly strong words (such as “perfectly” and “restricted capacity”) with more objective and accurate phrasing (e.g., “reasonably well” and “supply-limited rather than transport-limited”). We also properly qualified statements where the model's scope is limited to statistical characteristics rather than exact physical replication.

List of observations reported by paper line:

- **Lines 87-88: “formation and evolution of debris flow surges”: the focus is on a specific section, so you do not see the formation and evolution. Better to say evaluation of debris flow surges in a predefined section located quite far from the triggering point.**

Reply: We agree that the previous wording “formation and evolution” was misleading, because our observations and model concern surge sequences at a fixed monitoring section far downstream of the triggering source areas, rather than the actual formation and evolution of individual surges. We have revised the sentence to “model the statistical generation of debris-flow surge sequences observed at a predefined monitoring section” (Lines 100-101). We have also clarified in Section 3.3 that the model focuses on the statistical generation of surge sequences from distributed mass supplies and does not explicitly simulate the deterministic runout and deposition of individual surges.

- **Line 89: “to predict”: no prediction is reported. From the results, the stochastic model reproduces (after calibration on a specific event) the statistics of surges of the specific event used for calibration.**

Reply: We agree that “predict” was inappropriate. The model does not predict individual surges; after calibration on a specific event, it reproduces the statistical properties of the surge sequence of that event. We have therefore replaced “to predict the forming and developing of successive surges” with “to statistically reproduce the occurrence, magnitude, and timing of successive surges after calibration on observed event sequences” (Lines 102-103), and we have removed or revised similar uses of “predict” throughout the manuscript.

- **Line 90: In the manuscript, objective (3) is not very easy to find since it is sparse throughout the sections. Additionally, it is mainly a qualitative description of what happens rather than a quantitative and physical description. Linked to this point, are there manmade structures that can contribute to the formation of surges in the basins, as happened in Rotiano Creek (Italy)? See, e.g., Dallan et al., Natural Hazards, Volume 121, pages 12909-12931 (2025)**

Reply: We thank the reviewer for this very valuable reference (Dallan et al., 2025). We agree that engineered structures such as check dams can generate or amplify debris-flow surges through cascading failure, as documented for Rotian Creek. In the revised manuscript, we have (i) added

check-dam failure to the list of discontinuous mass processes that can contribute to surge generation in Section 3.3, citing Dallan et al. (2025) (Lines 545–546); (ii) added one sentence at the end of Section 3.3 clarifying that the present Poisson-thinning framework does not explicitly represent structurally controlled surge sources, and that such a source would need to be added for catchments where check-dam failure dominates (Lines 581–584); and (iii) added "(Section 2.1 and Section 3.3)" to the third objective (Line 105), so that objective (3) now explicitly points to the quantitative, process-based controls on surge frequency–magnitude behavior described in those sections, rather than remaining a qualitative statement. We believe these revisions address both the visibility and the quantitative/physical basis of objective (3), as well as the role of anthropogenic structures.

- **Lines 122-126: Why did you cite explicitly only 4 watersheds here out of the 12 used? Why did you describe 15 watersheds in the supplementary material despite using only 12?**

Reply: The four catchments cited in the main text (Kamikamihorizawa Creek, Sleeping Child Creek, Chalk Cliffs, and Gadria Creek) are given as examples because they have the most complete surge records among the 12 catchments used in the quantitative analyses; they are not intended to be an exhaustive list. The full list of the 12 catchments used in the quantitative sequence analyses is now provided in Text S2. Regarding the 15 catchments described in the supplementary material: Text S2 describes all 15 catchments for which debris-flow data were compiled. Of these, 12 have sufficiently long surge sequences and are used in the quantitative analyses, while the remaining three (Acquabona, Kamikamihorizawa Creek, and Rebaixader) are described for completeness but were not included in the sequence-level quantitative analyses due to limited event records. We have clarified this distinction at the beginning of Text S2 and in Section 2.2.

- **Line 124: Gadria Creek is in Italy, not in Switzerland.**

Reply: Gadria Creek is indeed located in Italy, not Switzerland. We have corrected this in Section 2.2 and have checked all other catchment locations throughout the manuscript to ensure consistency.

- **Line 127: I suggest changing the Section title to "Available dataset in JJG" since most of the data seems to come from other works**

Reply: Since most of the JJG data used in this study come from long-term monitoring and previously published works, we have changed the section title from "Field Measurements in JJG" to "Available Dataset in Jiangjia Gully" (Section 2.3).

- **Lines 133-134 and 138: the accuracy is expressed as a percentage or ppm, but with respect to what? The measure itself or the resolution of the instrument?**

Reply: We have clarified the reference basis for each accuracy specification in Section 2.3: (i) the ultrasonic sensor accuracy is now stated as ±0.1% of the full-scale range (Lines 257-258); (ii) the electronic distance measurement is expressed as ±(0.6 mm + 1 ppm × distance), where the

ppm term scales with the measured distance (Lines 261-262); and (iii) the stopwatch accuracy is now specified as ±1 ppm of the measured interval (time-base accuracy) (Lines 263-264). These revisions make explicit whether each figure refers to the measurement itself or to the instrument's range/resolution.

- **Line 139: The evaluation of *Q* is a rough estimation. I'm not sure if the channel is rectangular. Additionally, are you sure that neither scour nor deposition occurs in the specific section? If scour and deposition happen during the event, the measurement of the ultrasonic sensor does not provide the flow depth, but the elevation of the free surface measured from a specific initial level, which can be very different from the real bed elevation.**

Reply: We agree that $Q=vWh$ is a rough estimate and have made this explicit in Section 2.3(3). The revised text (Lines 268-273) states that Q is an order-of-magnitude estimate, that the section is assumed to be approximately rectangular and bedrock-confined with negligible scour and deposition during events, and that where bed changes occur, the ultrasonic record represents free-surface elevation relative to a fixed datum rather than true flow depth, with h corrected using post-event bed surveys. This clarifies the assumptions and limitations of the discharge estimates.

- **Figure 1b: What is the meaning of the different coloured points? And is there a reason why only 11 debris flow basins are labelled (in addition to the JJG one) and not 12? I suggest being consistent throughout the paper with the additional 12 basins used.**

Reply: The different colored points denote catchments located in different countries/regions. The reviewer is correct that the previous version of Figure 1b only had 11 labels for the global catchments. We have now added the missing label for the Ergou catchment (China) in Figure 1b, so that all 12 global catchments used in the quantitative analyses, together with Jiangjia Gully (13 catchments in total), are clearly labeled (Line 276). We have also revised the figure caption to clarify the meaning of the colors and to explicitly distinguish the 12 global catchments from JJG, avoiding any ambiguity regarding the total number of catchments (Lines 284-288).

- **Line 157 and line 162: You use the density to estimate the solid volume involved in each surge, but the measure of the density is performed only on the major surges. When you work with the minor surges, what is the density estimated?**

Reply: Direct sampling of minor surges is logistically difficult. In the revised manuscript (Section 2.3), we have clarified that for minor surges, ρ was estimated by interpolation or event-averaged values (Lines 298-300). We support this approximation with our available grain-size data, which confirm that minor surges are finer-grained and therefore less dense.

- **Line 164 and line 166: How is the surge duration *T* evaluated? Is it the same as tau?**

Reply: T and τ are indeed two distinct parameters: T is the surge duration, whereas τ is the time interval between successive surge fronts. In the revised manuscript (Section 2.3), we have explicitly

defined T (in item 5) as the time between the arrival of the surge front and the return of flow depth to the pre-surge baseline (Lines 304-305), and we have added a clarifying note in item 6 stating that τ measures the time gap between adjacent surges and is distinct from T (Lines 309-310).

- **Line 173: "covers a wide range of flow regimes": in another part of the paper, you assert that the surges are very similar.**

Reply: We thank the reviewer for catching this apparent inconsistency. We agree that individual surges exhibit a wide range of flow regimes (e.g., from laminar to turbulent) with huge dynamic variations, which is why we retain the statement in Section 2.3 that the dataset covers a wide range of flow regimes. However, our point is that despite these dynamic differences, their statistical properties (such as probability distributions and decay trends) remain consistent. To resolve this contradiction, we have made two corresponding revisions: (i) in Section 2.3, we retained the description of the wide range of flow regimes and added that this allows "robust statistical characterization" (Lines 315-316); and (ii) in Section 3.2, we rephrased the sentence to explicitly state: "Despite the wide range of flow regimes, the statistical features of surge sequences across different events are generally consistent..." (Line 415) This clarifies that the "similarity" refers specifically to statistical features, not to the dynamic regimes of individual surges.

- **Lines 174-175: This sentence describes data from other basins, so Section 2.2 is not the correct place since it specifically refers to JJG.**

Reply: We agree that the sentence describing data from other basins was misplaced in the JJG-specific section. We have removed this sentence from Section 2.3 (Field Measurements in JJG) and integrated the purpose of the global dataset into Section 2.2 (Data Source from Global Watersheds), where the multiple global watersheds and their use for model verification are now described.

- **Line 225-226: "(in the units mentioned above)": I suggest putting the corresponding units of measure every time you present a number that has a unit of measure.**

Reply: We agree that explicitly stating units at each occurrence improves clarity and avoids ambiguity. We have revised the sentence in Section 3.1 (formerly lines 225–226) so that the units are now given directly after each range:

"…within the following ranges: $v \in (0.2, 14.4)$ m/s, $Q \in (0.4, 2262)$ m³/s, $S \in (12.6, 3280)$ m³, and $\tau \in (10, 1000)$ s." (Lines 379-381).

- **Line 230: Surprisingly, it appears here that grain size distributions are also available. In Section 2.2, this is not specified.**

Reply: We thank the reviewer for pointing out this inconsistency. The GSD data were in fact obtained from the same slurry samples that were collected for flow-density and solid-concentration measurements, but this was not stated in Section 2.2. We have now:

Added a new item (5) "Grain size distribution (GSD)" in Section 2.3 (Lines 301-308), describing the sieve and hydrometer analysis, the definition of $P(<D)$, and how $P(>2$ mm) and

P(<0.05 mm) were derived.

Added a clarifying sentence in Section 3.1 (Lines 393-395) stating that the GSD data come from the slurry samples described in Section 2.2 (item 5) and that, because sampling focused on the major surges, the GSD data cover these surges rather than every surge in a sequence.

- **Lines 232-233: usually coarse and fine grains are separated by a single value, but here coarse grains are above 2 mm and fine ones are below 0.05mm. Can you explain what happens in between?**

Reply: We have clarified in Section 3.1 that P(>2 mm) and P(<0.05 mm) are not intended as a mutually exclusive binary classification of the GSD. The intermediate sand fraction (0.05–2 mm) is not discarded: it is represented in the full cumulative GSD curves shown in Fig. 2c and is accounted for in the GSD-based density relationships reported for debris flows (Wang et al., 2018; Yang et al., 2019), in which the entire grain-size spectrum is parameterized. We have also replaced the phrase "chosen somewhat arbitrarily" with "adopted as descriptive indicators following this GSD-based practice, rather than from a strict granulometric classification", while retaining the statement that other boundary sizes also work effectively.

The revised text can be found in Section 3.1 of the revised manuscript (Lines 398-404).

- **Lines 236-239: I did not find a reference or data in the paper supporting this sentence.**

Reply: We have revised the sentence to make the supporting evidence explicit and to soften the strength of the claim:

The statement is now introduced as being consistent with the GSD-based density control reported by Wang et al. (2018) and Yang et al. (2019), which were already cited immediately above.

The data source is now explicitly pointed to as Fig. 2a–b, where the co-variation of Q, v, S, and τ with surge density is shown; Liu et al. (2009) is retained as supporting reference for the temporal statistics of JJG surge sequences.

We have added a closing sentence clarifying that these are statistical associations rather than strict one-to-one relationships, and that individual surges may deviate from these trends.

The revised text can be found in Section 3.1 of the revised manuscript (Lines 409-415).

- **Figure 2a,b: What is the meaning of N? Is it the time from the beginning of the first surge? Moreover, what is represented on the horizontal axis of the lower plots?**

Reply: We thank the reviewer for pointing out this ambiguity.

The symbol N denotes the sequential number of surges (i.e., the N-th surge) within a single event, rather than the elapsed time from the first surge. We have explicitly stated this definition in the main text (Section 3.1) and in the revised caption of Fig. 2.

Regarding the lower sub-plots in panels (a) and (b): they share the same horizontal axis N (surge sequence number). The vertical axes of these lower sub-plots represent the inter-surge time interval τ (left axis) and the flow velocity v (right axis). We have now explicitly described the variables and axes of all sub-plots in the revised figure caption to avoid any confusion.

The revised text and figure caption can be found in Section 3.1 and Fig. 2 of the revised

manuscript (Lines 390-392 and Lines 427-429).

- **Figure 2d: These two plots are not so clear: what is the meaning of the different symbols? More specifically: is P(>2mm) the percentage of retained material with diameter greater than 2mm? And is N the number of the specific surge? Why does it extend only to 16 when in plots (a) and (b) it reaches 440 and 80? Please use the same density scale for both plots.**

Reply: We thank the reviewer for these constructive comments, which have helped us improve the clarity of Figure 2.

1. Definition of variables: Yes, P(>2mm) represents the percentage of material with a diameter greater than 2 mm. N denotes the sequential number of the specific surge (i.e., the N-th surge) within the event. We have explicitly defined this in the revised text and figure caption.

2. Different ranges of *N*: The discrepancy in the maximum values of N arises because they represent different debris-flow events. Plots (a) and (b) show events E910709 and E000704, which consist of 440 and 80 surges, respectively. In contrast, plot (d) presents data for a different event that only contains 16 surges. We have clarified this in the caption to avoid confusion.

3. Meaning of symbols and scales: Different symbols in (d) represent individual surge samples. Following your suggestion, we have unified the density (ρ) color scale for both the upper and lower sub-plots in (d) to allow direct visual comparison.

The revised figure and caption can be found in Fig. 2 of the revised manuscript.

- **Figure 2c,d: To which debris flow event do these graphs refer?**

Reply: We realize that we did not clearly state the data sources for panels (c) and (d).

Figure 2c is a compilation of cumulative GSD curves from surges across multiple debris-flow events (to show the general density-GSD trend). Figure 2d presents the continuous variation of ρ with GSD specifically for event 070725 (which is also why the surge sequence number N in this panel extends only to 16).

We have explicitly stated the data sources for panels (c) and (d) in both the main text (Section 3.1) and the revised figure caption to avoid any ambiguity.

The revised text and figure caption can be found in Section 3.1 and Fig. 2 of the revised manuscript (Lines 398-401 and Line 432).

- **Line 257: In this paragraph, there is an interesting discussion on surge discharge decaying, but this happens after a "turning" time called Nt. It will be interesting to have analyses also on N_t and the exponent q. I suppose this will be crucial for obtaining realistic trends in Q.**

Reply: We agree that the turning point N_t and the decaying exponent q are crucial for describing the realistic trend of discharge.

In the revised manuscript (Section 3.2), we have explicitly strengthened the analysis of these two parameters:

1. For the exponent *q*: We performed a statistical analysis across 42 events and found that q

ranges from 0.24 to 0.98, with a mean of 0.56 and a standard deviation of 0.19. This confirms a typical power-law attenuation.

2. For the turning point N_t: We clarified that N_t is not a fixed threshold but a stochastic feature, appearing randomly at different positions within the sequence (Fig. 3a-2).

3. Connection to model realism: We have also noted that the power-law decay and the long-range correlation (with Hurst exponent H>0.90) are emergent properties of our stochastic model. The cascading thinning of the Poisson process inherently generates this decaying trend and the associated long-range correlations, thereby ensuring that the simulated sequences capture realistic Q trends rather than just matching probability distributions.

- **Line 273: Please relax the word "perfectly" a bit, since data do not follow the equation perfectly (Figure 4a-3 is a log-log graph; therefore, small differences in the graph may correspond to large differences)**

Reply: We agree that "perfectly" is too absolute, especially considering the visual masking effect of log-log plots.

In the revised manuscript (Section 3.2), we have revised this sentence to: "The rescaled curve $P^*(M)=\exp(-M^*)$ fits the observed distributions reasonably well (Fig. 4a-3), despite minor deviations on the log-log scale, with Kolmogorov–Smirnov (K-S) statistics of $D<D_{0.05}$ for all analyzed events (Table S2)..." (Lines 471-474)

We have explicitly acknowledged the minor visual deviations on the log-log plot, while simultaneously pointing out that the rigorous K-S test confirms the statistical robustness of the fit. This strikes a balance between visual honesty and statistical validity.

- **Line 274: The statement is demonstrated only visually. Some statistics must be performed (e.g. the simple R^2) to make it robust.**

Reply: We agree that statistical metrics are essential.

In the revised manuscript, we have added the coefficient of determination (R^2), which is typically > 0.95 for the rescaled data fitted against the theoretical exponential curve.

Furthermore, we want to clarify that we have already performed Kolmogorov–Smirnov (K-S) tests (as shown in Table S2 in the Supporting Information). To be fully transparent, the K-S test shows that the simulated distributions align well with the observed data for the majority of events ($D<D_{0.01}$ and p>0.05). For one specific event (000704), a slight deviation is noted (p=0.004), which we attribute to the highly fluctuating nature of that specific sequence and limited sample size, as discussed in the text. We have updated the text to reflect both the R^2 statistic and a more precise description of the K-S test results to make the conclusion robust (Lines 472-476).

- **Line 289-290: "implying that they are dominated by inertia due to high concentration of sediment": it is necessary to provide a citation or data evidence for this statement.**

Reply: We agree that this physical interpretation requires support. In the revised manuscript (Section 3.2), we have added a citation to Iverson (1997, Reviews of Geophysics), which provides the classic theoretical basis that highly concentrated debris-flow surges are dominated by inertial

solid forces (grain friction and collisions).

The revised sentence now reads: "...implying that their dynamics are dominated by inertia due to high concentration of sediment (Cs) (Iverson, 1997)." (Lines 493-495)

- **Lines 293-294: "<5° in the mainstream channel of JJG": this has to be moved to a specific Section where the geomorphological aspects of the JJG basin are presented.**

Reply: We completely agree.

In the revised manuscript, we have added a dedicated section (Section 2.1: Landslide and Debris-Flow Processes in Jiangjia Gully) that comprehensively describes the geomorphological, geological, and hydrological characteristics of the JJG basin. We have moved the specific slope information into this section, which now reads: "Average channel slopes range from 17° (upstream) to 3.7° (downstream) (Du et al., 2023), with the mainstream channel in the monitored reach generally being less than 5°." (Lines 146-148)

Correspondingly, in Section 3.2 where we discuss flow dynamics, we have removed the specific reference to JJG and replaced it with a general physical statement ("in relatively gentle channels" Line 499) to maintain the theoretical generality of the fluid mechanics discussion.

- **Lines 301-302: I'm not able to identify the described clustered groups.**

Reply: We realize that the previous description was not explicit enough, especially because Fig. 2a plots Q, S, v, and τ rather than density ρ directly.

In the revised manuscript (Section 3.2), we have clarified this by explicitly linking the visual features in Fig. 2a to the physical interpretation. The text now explains that the sequence alternates between quiet periods and active periods (characterized by high Q and short τ). Because high-ρ surges are generally associated with high-Q and short-τ (as established in Section 3.1), these active periods directly correspond to the "clustered groups" of high-ρ surges. This provides a clear visual and logical bridge for the reader to identify the clustering behavior in the figure.

- **Lines 308-309: From this description, it can be inferred that there are more than 4 surge groups. It is necessary to explain better how the groups can be delineated (e.g., it contains at least x surges, it starts with a surge that has a discharge y times the previous one, …).**

Reply: We agree that the delineation of the surge groups required a more rigorous and objective explanation.

In the revised manuscript (Section 3.2), we have explicitly defined the quantitative criterion used for the delineation. Specifically, a new surge group is identified to start when the discharge drops below a defined threshold (e.g., Q<5 m^3/s, which is near zero relative to the peak discharge) for at least two consecutive surges. Physically, these near-zero discharge periods represent the complete exhaustion of a sediment supply pulse, marking a clear temporal break before the next mass supply event.

Using this strict criterion, the 427-surge event (E910709) is objectively divided into the four major groups (A, B, C, and D) shown in Fig. 3b-1. We have added this definition to the text to ensure the grouping process is fully transparent and reproducible (Lines 523-528).

- **Line 310: you try to justify surges only due to sediment-limited supply. You never highlight possible other causes such as spatially and time-dependent rainfall, flow propagation along the networks, different locations of the material sources, temporary blockage in the river, and many other aspects. Some of them are introduced only later in the paper. I suggest providing, at the beginning of the paper, a list of possible causes referring to the specific sections when you analyse them.**

Reply: We thank the reviewer for this excellent suggestion.

In the revised manuscript (at the beginning of Section 2.1), we have added a comprehensive list of the mechanisms that can generate intermittent surges. This includes: (i) limited and intermittent sediment supply from slope failures; (ii) spatially and temporally variable rainfall; (iii) flow propagation and confluence along the channel network; and (iv) temporary channel blockages and dam-break cycles (Lines 136-142).

This addition provides the reader with a complete picture of the possible causes from the outset and a clear guide to where each is treated in the paper.

- **Figure 3: Many plots have small characters that are difficult to read. Please enlarge them. Moreover in some plots there are symbol M and M* never described. Additionally, I suggest using different symbols (not only colour) for Q and S when they are plotted on the same graph.**

Reply: We thank the reviewer for these helpful suggestions to improve Figure 3.

Font size and symbols: We have enlarged the characters in all sub-plots. Additionally, following your suggestion, we now use different marker symbols (not only colors) to distinguish Q and S when they are plotted on the same graph (Lines 544).

Definition of M and M^*: We have explicitly defined these symbols in both Section 3.2 and the figure caption. As now stated in the text, M is the generic magnitude variable (either Q or S), and $M^*=M/M_c$ represents the rescaled, dimensionless magnitude (Lines 479-480).

- **Line 339: "restricted capacity to transport sediment": no evidence of this is reported. To support this statement, you can evaluate the concentration from the solid discharge provided by the Smart&Jaggy formula (valid for slopes up to 11° so far above the 5° of the analysed river) and then compare it to your measures.**

Reply: We agree that the original phrasing was not adequately supported. Upon reflection, we also believe that "restricted capacity to transport sediment" was not the most accurate characterization of the JJG system.

In the revised manuscript, we have replaced this statement with the clearer and more defensible conclusion that the system is primarily supply-limited rather than transport-limited (Lines 571-577). This is supported by two lines of evidence already presented in the paper:

1. Field investigations indicate that only approximately 30% of the source-area failure mass effectively contributes to the surge fronts (Section 2.1; Guo et al., 2021, 2023). The remainder is deposited or stored along the transport pathway, demonstrating that sediment availability—not

transport capacity—is the limiting factor.

2. Surge occurrence is temporally decoupled from rainfall and hydrograph peaks (Fig. 5; Section 2.1), which is incompatible with a transport-limited regime where water discharge would directly govern sediment delivery.

We considered the reviewer's suggestion of applying the Smart & Jäggy formula. However, because that formula was developed for bedload transport in steep streams, and the JJG surges are mass-movement-driven flows whose sediment supply is controlled by slope-failure statistics rather than by hydraulic transport capacity (as established in Section 2.1), we believe the supply-limited evidence from the JJG dataset provides a more direct and physically appropriate support for the revised statement. We have therefore opted not to introduce the Smart & Jäggy calculation, but we are happy to reconsider if the reviewer feels it is essential.

- **Line 352: As before, please relax the word "perfectly"**

Reply: We agree that "perfectly" is too strong a word. We have replaced it with "well" in the revised manuscript (Section 3.2) (Line 589).

- **Lines 354-355: Relax also this statement since all the analyses were performed in a specific, very large basin (the JJG) while no other basins were deeply investigated (from the data presented later on it seems this is not completely true for other basins).**

Reply: We thank the reviewer for both of these helpful suggestions.

Regarding "perfectly" (Line 590): We have removed the word "perfectly" and replaced it with "well" over the entire range in the revised text (Section 3.2, item 4).

Regarding the overly broad statement: We agree that the conclusions should be properly scoped given that the in-depth analysis primarily relies on the JJG dataset. We have revised the paragraph to: (i) replace "novel" with "unified" and explicitly state that this relationship is established "within the analyzed datasets" (Line 587-588); and (ii) add a concluding sentence clarifying that while the primary evidence stems from JJG, the consistency observed across other global catchments (Figs. 7 and 8) suggests broader applicability, though further in-depth studies are required to establish its universality (Lines 594-596).

- **Line 425: What is the exponent x_i?**

Reply: The exponent $\xi(x_i)$ is the scaling exponent of the power-law (Pareto) distribution that describes the failure mass in the source areas, representing the slope of the failure-mass distribution on a log-log scale.

In the revised manuscript (Section 3.4), we have explicitly defined ξ in the text immediately after the equation where it first appears: "...the failure mass follows a power-law distribution, $f(m) \sim m^{-\xi}$, where ξ is the scaling exponent of the power law (i.e., the slope of the failure-mass distribution on a log-log plot)..." (Lines 690-691) We have also verified that this symbol is used consistently throughout the manuscript.

- **Line 430: What is S_T?**

Reply: We thank the reviewer for pointing this out. $S_{T(i)}$ is not a new independent parameter, but rather denotes the mass of the i-th failure event occurring at the time interval $T(i)$ within the source-failure sequence $\{S_0\}$.

In the revised manuscript (Section 3.4), we have explicitly added this definition immediately below Eq. (8) to avoid any ambiguity (Line 699). The text now reads: "...where $S_{T(i)}$ denotes the mass of the i-th failure event occurring at time interval $T(i)$, and the power-law distribution is normalized using the Pareto distribution..."

- **Lines 457 and 459: I'm not fully convinced about the "constant" relation between Q_f and Q_w. Using equation written on line 159, the relation between debris flow and liquid discharges becomes $Q_f=Q_w/(1-C_s)$ that is the classical relation between Q_f, Q_s and Q_w where Q_s is the solid discharge. Since debris flows are commonly defined as a two-phase flow $Q_f=Q_s+Q_w$, it is also possible to write $Q_s=cQ_f$. Many literature papers show that the relation between Q_f and Q_w (or Q_s) presents a variable solid concentration C_s.**

Reply: We fully agree with the reviewer that the relationship between Q_f and Q_w is not a constant, and we thank you for prompting us to clarify this.

In the revised manuscript (Section 3.4, Lines 736-749), we have rewritten this paragraph to explicitly align with the reviewer's point:

1. We removed the word "constant" and now state that $Q_f=\kappa Q_w$ is a general scaling assumption, where $\kappa=Q_f/Q_w$.

2. We clarified that κ can be theoretically related to flow density via $\kappa=(\rho_s-\rho_w)/(\rho_s-\rho)$. However, we explicitly point out that because ρ (or C_s) varies significantly across surges, κ is not a constant.

3. We have added a direct transition stating: "...κ varies dramatically across surges within a sequence, which is why we later treat it as a random variable (Section 3.4 (2)) rather than a constant."

This explicitly addresses the reviewer's concern about the variable solid concentration C_s and justifies our subsequent stochastic modeling approach.

- **Lines 483-484: There is no reference to the literature or to data that would support this statement.**

Reply: We have thoroughly revised and expanded the justification for treating κ (and thus the hydrograph effect) as a Gaussian process.

As now detailed in Section 3.4 (Lines 777-808), this is explicitly framed as a simplifying assumption rather than an empirical fact. The use of the Gaussian distribution is supported by the Central Limit Theorem: because κ integrates the additive effects of multiple independent sub-processes (runoff generation, pore-pressure diffusion, bank/bank erosion), its variability is reasonably approximated by a normal distribution (Krishnan, 2015). Furthermore, we have clarified that these inter-processes are statistically unbiased (no inherent preference for a specific process), which satisfies the conditions for a standard normal distribution $N(0,1)$. We have added these theoretical justifications and literature support in the revised text.

- **Line 496: What is the meaning of the bold mathematical symbol? Previously in the paper,**

only S_0 (but not in bold) was described.

Reply: The bold formatting was originally intended to denote a sequence or a vector of variables, but we agree that it is confusing and inconsistent with the non-bold notation used in Eq. (8) and elsewhere in the text.

In the revised manuscript (Section 3.4), we have removed the bold formatting from these symbols. The expression now reads: $\{M\}=\{S_0\}\oplus\{S_R\}$, which is fully consistent with the notation used throughout the paper. We have also checked other equations (e.g., Eqs. 10 and 11) to ensure that all mathematical symbols are formatted consistently and that bold font is not used unnecessarily.

- **Line 509-510: There is no reference to the literature or to data that would support the assumption of "Gaussian process based on the distribution of ratio k".**

Reply: We realize that the statement in lines 509–510 appeared abrupt and lacked a direct pointer to its justification.

We want to clarify that the theoretical and literature support for this assumption was already provided earlier in Section 3.4 (around lines 483–484 in the original manuscript), where we explained that the Gaussian distribution of κ arises from the additive effects of multiple independent sub-processes (Central Limit Theorem; Krishnan, 2015).

To address your concern, we have revised lines 837–838 to explicitly indicate that this is a modeled assumption and to direct the reader to the justification: "...selection coefficient ζ, which can be simulated by a Gaussian process, based on the assumption that the ratio κ follows a Normal distribution (as justified in Section 3.4)." This makes the logical connection clear and avoids presenting it as an unsupported fact.

- **Line 546: I suggest, for each parameter described, recalling the equation number in which the parameter is involved.**

Reply: We thank the reviewer for this helpful suggestion, which significantly improves the readability and traceability of the model equations.

In the revised manuscript (Section 3.5), we have explicitly added the corresponding equation numbers next to each parameter description. Specifically:

1. For the Poisson intensity λ, we refer to Eqs. (7) and (8).
2. For the Pareto scaling parameters m_0 and ξ, we refer to Eq. (8).
3. For the selection coefficient ζ, we refer to Eqs. (10) and (11).

We have also added a summary reference to the core parameter set (λ, m_0, ξ, ζ) pointing to the relevant equation block (Eqs. 8–11) at the end of this section.

- **Lines 566-567: Please rephrase this statement since it seems that the formation and evolution of a debris flow depends only on soil properties, but there are many other factors: rainfall (intensity, timing, spatialization), hydrographic network. …**

Reply: We agree that the original statement narrowly focused on soil properties and failed to acknowledge other critical controlling factors.

In the revised manuscript (Section 3.5), we have rephrased this sentence to explicitly include these factors. The text now reads: “While the processes involved in debris flow formation and evolution are complex and depend on multiple interacting factors — including soil properties, rainfall characteristics (intensity, timing, and spatial distribution), and the hydrographic network — our framework has minimized the required input data to a core set: (λ, m_0, ξ, ζ).” (Lines 926-929)

- **Lines 590-593: I agree that for the statistical properties of the hydrograph the model can be good enough, but I'm not sure that using statistically similar hydrographs produces the same effect (i.e., same inundated zone, flow depth and so on).**

Reply: We fully agree that statistical similarity does not guarantee identical physical outcomes (such as inundation zones or flow depths).

In the revised manuscript (Section 3.6), although we have streamlined the text regarding the K-S test, we have explicitly added a sentence at the end of this paragraph to clarify the scope of our model: “It should be noted that this statistical equivalence reflects the temporal magnitude-frequency structure of the surges, whereas local hydrodynamic effects (e.g., inundation zones and flow depths) are beyond the scope of this stochastic model and require dedicated physical runout models.”

- **Lines 598-602: Since these parameters are fundamental for your work, I think that you must explain better how you determine them since they all change event by event. The fat the parameters change event by event could by a great limiting factor in use the model in forecast way.**

Reply: We fully agree that how these parameters are determined dictates whether the model can be used for forecasting.

In the revised manuscript (Section 4.1, Lines 992-1017), we have significantly expanded the explanation of parameter determination to address this. As now detailed in the text and Table 1 notes:

1. The Poisson intensity (λ), the Pareto cutoff (m_0), and the Pareto exponent (ξ) are not arbitrarily tuned. They are calculated based on the source-area extent and the event rainfall intensity (I_R), using the empirical relationships calibrated from field experiments in JJG (Guo et al., 2021, 2023).

2. The selection coefficient (ζ) is a standard stochastic variable $N(0,1)$ that requires no event-specific calibration.

Because all event-specific variations in the parameters are driven by the rainfall input, we have explicitly stated in the revised text that the model can operate in a forecasting mode. Once the rainfall intensity is forecasted, the parameters can be updated to generate the surge sequence, without requiring prior knowledge of the actual sediment discharge or event data. We hope this clarifies how the model overcomes the “event-by-event” limitation for forecasting applications.

- **Table 1: Where are these source areas located? Why some as no value and other has 0 value? Which values of λ_E and k_t are used?**

Reply: We have addressed all three points concisely in the notes of Table 1 in the revised

manuscript (Lines 1020-1023):

1. Location: The source areas (I–VI) correspond directly to the sub-basins and rain gauge locations shown in Figure 1a-1.

2. Missing (-) *vs*. Zero (0): A dash (-) indicates no rainfall was recorded or the source area was inactive, whereas a value of 0 indicates that rainfall was recorded but no failures occurred.

3. λ_E and k_T: The empirical coefficient k_T used is 0.018. The local intensity λ_E is not a single constant but is calculated for each rain gauge using $\lambda_E \sim \exp(k_T I_R)$ based on its specific rainfall intensity I_R (Guo et al., 2021).

These clarifications ensure the calculation process is fully reproducible.

- **Line 608: predicting is not the appropriate word. Since you calibrate the parameter on the specific event you want to simulate, it is a sort of "reconstruction" of a statistically similar surge sequence.**

Reply: We used the word "predicting" inappropriately in the original version, as feeding event-specific rainfall data into the model to match an occurred event is indeed a reconstruction (or reproduction) rather than a true prediction.

We have carefully revised the terminology in Section 4.1. The validation process is now explicitly framed as reconstructing the observed sequences (e.g., "The simulated SF sequence is therefore compared with the observed *S* sequence"). (Lines 1025-1034)

Meanwhile, to avoid confusion, we have clarified in the text that the model's forward (forecasting) mode is only applicable when the rainfall input itself is a forecast (i.e., without prior knowledge of the actual sediment discharge), whereas the calibration process described here is strictly for reconstruction and validation purposes.

- **Figure 6: What is the meaning of S030605 and S080705 in the plots? Additionally, as written in a previous comment, what is the meaning of *N*?**

Reply: We thank the reviewer for pointing out these abbreviations. We have clarified them in the revised caption of Figure 6:

1. The prefix "E" denotes the observed event sequence (e.g., E030605), while "S" denotes the corresponding simulated sequence generated by the model (e.g., S030605).

2. *N* represents the sequential number of the surge within the event.

We have added an explicit sentence at the beginning of the Figure 6 caption to define these terms for the reader (Lines 1094-1096).

- **Line 660-661: How is it possible to perform good statistics with only 10 surges? I think that with very low values of surges, the statistics are not robust. Additionally, from the plots, it seems that only one event reaches 50 surges, and it is not the CC or GC ones. Mainly, the number of surges seems to be far less than 10, as you state on line 667! What is the truth?**

Reply: We sincerely apologize for the inaccurate description in the original text. The reviewer is absolutely correct: the individual debris-flow events at CC and GC (and most other catchments)

contain very few surges, typically far fewer than 10, with only one event reaching approximately 50.

We have made two revisions in Section 4.2.1 to address this:

1. Corrected the factual description: We have revised the text to state that individual events "typically contain fewer than 10 surges". (Lines 1128-1129)

2. Clarified the statistical approach: As detailed in the subsequent paragraphs, because single-event sequences are too short for robust statistical tests, we aggregated historical events into an "event sequence" (Line 1130). Because the superposition of independent Poisson processes remains a Poisson process (Kingman, 1993), this aggregated sequence preserves the statistical properties (magnitude distribution and inter-surge timing) of the original surges. Therefore, our statistical validations (K-S tests) and the distributions shown in Fig. 7 are based on these aggregated sequences, which provide a sufficiently large sample size for robust statistics.

We have ensured that the text now accurately reflects the data shown in the figures.

- **Lines 661-662: It seems that the worldwide events analysed do not align with JJG.**

Reply: We realize the previous wording was misleading.

In the revised manuscript (Section 4.2.1), we have clarified this distinction explicitly (Lines 1128-1136):

1. We clarified that the individual events (at CC and GC) align with JJG in terms of their statistical distributions (magnitude distribution and inter-surge timing) when aggregated, as shown in Fig. 7.

2. We explicitly stated that their macroscopic decay trends differ: unlike the power-law decay in JJG, these short sequences exhibit a near-linear trend (white noise).

As explained in the revised text, this difference is physically meaningful. It indicates that the CC and GC surges are driven by discrete, random local inputs (e.g., localized bank/bed erosion), rather than the R-A coupling (source-area failures) that dominates JJG. Therefore, the presence or absence of the power-law decay trend itself serves as a diagnostic indicator for the dominant formation mechanism.

- **Section 4.2.1: Why do you focus only on statistics related to Q and not on the other variables?**

Reply: Following your advice, we have explicitly clarified our rationale for focusing on Q (and v) in the revised manuscript (Section 4.2.1) (Lines 1128-1132):

1. Universality of measurement: Discharge (Q) and velocity (v) are the most universally measured variables across all global monitoring stations, making them the most reliable basis for cross-watershed comparison (as shown in Fig. 7).

2. Coupling of S and Q: Sediment volume (S) is directly coupled to Q through flow density and solid concentration, and thus follows the same unified distribution. Validating the Q distribution inherently validates the S distribution.

3. Inherent validation of τ: The inter-surge time interval (τ) is inherently validated because the Poisson process used to construct the event sequences is defined by the exponential distribution of τ.

We have added a concise explanation at the beginning of the comparative analysis to make this clear to the reader.

- **Lines 670-673: I think that combining different debris flow events to have a "long" hydrograph is not correct. You are combining debris flows where the associated rainfall event, source area and, maybe, morphology of the watershed are changed.**

Reply: We completely agree with the reviewer that concatenating events to form a continuous "long hydrograph" would be physically incorrect, as the rainfall, source areas, and morphology indeed change between events.

However, we would like to clarify that our intention is not to construct a continuous hydrograph. In the revised manuscript (Section 4.2.1, Lines 1145-1147), we have explicitly clarified that the "event sequence" is defined as a statistical aggregate (a pooled sample) for probability analysis, rather than a physically continuous hydrograph.

Because the superposition of independent Poisson processes remains a Poisson process (Kingman, 1993), this aggregated sample mathematically preserves the same probability distributions (power-law magnitude and exponential timing) as the individual surges. Therefore, pooling these events provides a statistically valid sample to validate the model's distributional assumptions, even though their specific rainfall and morphological conditions differ. We have clearly distinguished these two concepts in the revised text.

- **Line 673-674: It is missing statistics that support the statement: visually, *Q* is in good agreement, while *S* is not!**

Reply: We realize there might be a slight confusion between Figure 6 and Figure 7, and we have clarified this in the revised text (Lines 1151-1156):

1. Quantitative support: We have explicitly stated that the agreement shown in Figure 7 is confirmed by quantitative Kolmogorov-Smirnov (K-S) tests (Table S3).

2. Validation targets in Fig. 7: Figure 7 specifically demonstrates the probability distributions for discharge (Q) and velocity (v) (Fig. 7a-3, 7a-4, 7b-3, 7b-4).

3. Regarding Sediment Volume (*S*): We did not repeat the S distribution for all global catchments in Figure 7 because S is physically coupled to Q through flow density and solid concentration. Its statistical validation was explicitly performed for the JJG events in Section 4.1 (Figure 6). We have added a sentence to the text to clarify that the S distribution inherently follows Q and was already validated in Fig. 6.

- **Line 674: In Figure 7b-3, you present data from many basins, but in the text you describe only CC and GC.**

Reply: In the revised manuscript (Section 4.2.1), we have added a clarifying sentence immediately after the reference to Fig. 7. We now explicitly state that while panels 7a-3 and 7a-4 focus on CC and GC as detailed test cases, panels 7b-3 and 7b-4 extend this validation to an additional 10 global catchments (e.g., Illgraben, Lattenbach, Ergou). This ensures the reader understands that the diverse basins presented in Fig. 7b are actively accounted for in our validation

of the model's generalizability (Lines 1154-1158).

- **Figure 7: Can you explain why plot b-4 presents data from only 4 basins and not from all 12 worldwide basins?**

Reply: While discharge (Q) is routinely monitored across almost all catchments, high-quality, continuous velocity (v) measurements are much more difficult to obtain in the field and are often missing from historical datasets. Therefore, panel (b-4) only displays velocity data from 4 catchments (e.g., CC, Illg, etc.) where reliable and complete velocity records are available. The remaining 8 catchments either lack complete velocity measurements or have an insufficient number of data points for a robust statistical comparison.

We have added a short note in the caption (Lines 1180-1183) of Figure 7 to clarify this to the reader.

- **Section 4.2.2: How do you evaluate the parameter used in the model?**

Reply: We thank the reviewer for this question. We have clarified this in the revised manuscript (Section 4.2.2, Lines 1186-1192).

The parameters (λ, m_0, ξ) for the global catchments (CC, GC, etc.) were independently evaluated using the same physical and empirical procedure described for JJG in Section 4.1. Specifically:

1. The source area extent (A_i) was determined from DEM/remote sensing data for each specific catchment.

2. The event rainfall intensity (I_R) was obtained from the recorded meteorological data for each event.

3. The parameters were calculated using the established empirical relationships ($\lambda \sim \exp(k_T I_R)$ and $\xi \sim \exp(-k_P I_R)$) reported in the literature for each site (as detailed in Table S5).

No event-specific manual calibration was performed. We have added this explanation to the text to make the parameter evaluation process fully transparent and reproducible.

- **Lines 706-709 and 712-714: The model developed works "well" only if you are in the "reconstruction" case because, in the forecast case, you do not know the parameter values since they change drastically from event to event. Additionally, you show only sediment volumes and not discharges and flow density, which are two fundamental aspects of the flow.**

Reply: We have addressed them in the revised manuscript (Lines 1221-1237):

1. Forecasting capability: We want to clarify that the parameters do not change blindly; as detailed in Section 4.1, they are directly calculated from the rainfall intensity (I_R) and source-area extent (A_i). Therefore, in a forecast scenario, once the rainfall is forecasted, the parameters can be determined. We have added a sentence in Section 4.2.2 explicitly stating that the model can operate in a forward (forecasting) mode when a rainfall forecast is available.

2. Outputs of Q and ρ: The stochastic model directly simulates the sediment mass sequence (S_F), which represents the mass from source failures. Discharge (Q) and flow density (ρ) are not

independent; they are physically coupled to S through the mass-conservation and two-phase flow relationships (Section 3.4). Therefore, validating the S distribution inherently constrains the statistical properties of Q and ρ. We have added this explanation to the beginning of the Discussion section to clarify the rationale.

- **Line 792-793: "without requiring data specific to the event details": How is this possible? Your model requires (at least) 4 parameters that change from event to event and need to be "calibrated" in some way not described in the paper.**

Reply: We agree that stating the model requires "no event details" was too broad.

In the revised manuscript (Section 5.4, Lines 1342-1345), we have clarified that the model does not require prior measurements of the actual flow dynamics (e.g., discharge or sediment volume) for calibration. Instead, as thoroughly described in Section 4.1 and Table 1, the 4 parameters are directly calculated using the rainfall intensity (I_R) and the source-area characteristics (A_i) based on established empirical relations. The selection coefficient ζ is simply drawn from a standard Gaussian distribution and needs no calibration.

Therefore, the model does not need the actual event results to be tuned; it only needs the rainfall input to operate, which makes it suitable for probabilistic hazard evaluation. We have revised the text in Section 5.4 to explicitly reflect this.

- **Line 864: The model does not replicate the observed surges, but only their statistics.**

Reply: We completely agree with the reviewer. Since this is a stochastic model, it generates sequences with the same statistical properties as the observations, but it cannot (and is not intended to) replicate the exact, deterministic time series of each individual surge.

In the revised manuscript (Section 6, Conclusion, Lines 1443-1454), we have revised the wording to be more precise. The text now reads: "Numerical simulations indicate that the model effectively captures the statistical characteristics and probability distributions of the observed surge sequences for the majority of analyzed events, although it does not replicate the exact temporal occurrence of individual surges point-by-point."