

Reviewer 2

1. General Evaluation and Contextual Importance

This manuscript presents an extensive, multi-institutional investigation of the catastrophic June 21, 2024 torrential disaster that devastated the village of La Bérarde in the Écrins Massif (French Western Alps). The authors have assembled an exceptionally rich, perishable post-event dataset combining synoptic meteorology (ERA5), quantitative radar precipitation reanalysis (ANTILOPE), detailed snowpack physics modeling (S2M / SAFRAN-Crocus), multi-temporal high-resolution airborne LiDAR and satellite stereo-photogrammetry (IGN LiDAR-HD, Pléiades, Pléiades Neo), regional broadband seismology (EPOS-France), 1D/2D hydrodynamic reconstructions (Telemac-1D/2D), and semi-distributed hydrological modeling (MORDOR-SD). The overarching goal is to reconstruct the multi-hazard sequence, quantify water and sediment fluxes, evaluate driver recurrence intervals, and interpret the event through the lenses of compound climate extremes, sediment connectivity, and paraglacial catchment adjustment.

The core scientific thesis of the paper is compelling and of outstanding interest: the extraordinary severity of the disaster was not driven by a single, off-scale meteorological anomaly or by an explosive glacial lake outburst surge alone. Rather, it resulted from the synchronous compounding of moderate-magnitude hydrometeorological forcings (a ~10-year return period rainfall event superimposed onto a ~20–50-year snowmelt rate over an exceptionally deep, ripe isothermal snowpack), the progressive subglacial drainage of an expanding decadal supraglacial lake (~100,000 m³) and associated cavity storage on the Bonne Pierre glacier, and critical geomorphic conditioning. Crucially, abundant unconsolidated paraglacial sediment stores directly coupled through a steep, confined proglacial channel (Bonne Pierre torrent, slopes 20–40%) delivered over 300,000 m³ of coarse debris directly to the fan apex, whereas the adjacent, much larger Upper Étançons branch acted as a sediment buffer along wide, low-gradient braided reaches.

I wish to emphasize my sincere appreciation for the collaborative effort deployed by the authors to capture and synthesize this perishable event. The work is already very good, scientifically robust in its observational foundation, and highly relevant for the international geomorphological, hydrological, and alpine hazards communities. In an era where mountain catchments globally are adjusting to accelerated cryospheric degradation (recalling also recent events in the Himalayas), La Bérarde provides a textbook benchmark of compound paraglacial hazard. The conclusions of the manuscript could even more explicitly emphasize the wider applicability of these findings for risk assessment in similar high-relief deglaciating valleys. The observations and recommendations detailed below are offered with a purely constructive intent to

assist the authors in streamlining the paper's architecture, strengthening the geomorphological process interpretation, and eliminating redundancies. The exact depth to which each point is developed is left to the discretion of the authors and the editor.

Thank you for your comments and suggestions. We sincerely thank you for recognizing the value of what we presented in this article. We have worked in understanding the key processes at play and characterizing the nature of the event. The changes you suggest, will improve and strengthen the message of the manuscript. They also corroborate closely what RC1 suggested.

We reworked the whole structure, bringing clarity and intent in the message. The conclusion was strengthened to better answer the questions asked in the introduction. Redundancies and inconsistencies will be removed and homogenized.

2. Core Methodological and Geomorphological Critique

A primary aspect that requires substantial revision is the overall structure and narrative flow of the manuscript. In its current state, the text resembles an unedited research report rather than a tightly organized journal paper. The narrative is burdened by significant length and repetitive statements (**for instance, the destruction of the village and key sediment volumes are restated multiple times across Sections 3, 4, 5, and 7**). Furthermore, the structural sequence is counterintuitive: the geomorphological impacts (Section 3) are presented before the physical drivers (Section 4), and the chronological reconstruction (Section 5.4)—which forms a central pillar of the study—is subordinated at the end of the river response section. I strongly encourage the authors to restructure the manuscript following a logical process chain: Section 2 should establish the study site, geomorphic setting, and historical context; Section 3 should be a dedicated, self-contained Methods section synthesizing the core workflows (S2M, MORDOR-SD, Telemac-2D, LiDAR DoD, seismology, and PyMC GEV modeling), rather than the current brief Section 2.5 that merely delegates all procedures to separate appendices; Section 4 could pool the empirical findings under a unified 'Results' framework (initiating with the full multi-hazard event chronology, followed by meteorological forcings, hydrological/seismic responses, glacial lake evolution, and sediment budgeting); and Section 5 should focus on the overarching scientific discussion (hydraulics, connectivity, paraglacial adjustment, and compound risk governance).

These suggestions are consistent with those made by Reviewer 1. We reworked the structure in depth (see below). The method section is now more complete. One part we could not move was the Event Chronology. This section wraps up elements coming from primary evidence (observations / facts) but also from deduced elements (i.e.

hydrological modeling) to infer the most likely chain of events. We therefore kept it at the end of results as many elements needed to be introduced.

- **Intro**
- **Study site**
- **Methods and Data**
 - **Data**
 - Monitoring networks
 - Reanalysis datasets
 - Geospatial data
 - **Methods**
 - Spatial analysis
 - Snowpack modeling
 - Hydraulic reconstruction
 - Hydrological modeling
 - Seismic analysis
 - Statistical analysis
 - Debris flow analysis
- **Results**
 - **Preconditions to the event:**
 - Geomorphological context
 - Glacier
 - Snowpack
 - **Triggers of the 19-21 June 2024 Event**
 - Synoptic and meteorology
 - Snowmelt
 - Glacial lake drainage
 - River's response
 - Hydrometric observation
 - Hydrological modeling
 - Seismic analysis
 - Reconstructing the event's chronology
 - **Impact of the event**
 - Human and infrastructure impacts
 - Geomorphological changes
 - **Qualifying the rarity of the event (a transferer dnas la discussion?)**
 - Historical events
 - Event probability
 - Comparative sediment production
- **Discussion**

- Role of the GLOF
- Paraglacial adjustment
- Spatial connectivity
- Compound nature of the event
- Climate change
- **Conclusion**

From a geomorphological standpoint, the characterization of catchment morphometry and flow regimes should be grounded in established quantitative metrics. In particular, computing the simple Melton ruggedness number for the Étançons torrent at the La Béarde fan apex provides could provide valuable clues regarding long-term flow processes (conceptually the same result could be obtained by mean or median IC values, commenting on the spatial arrangement of the values though). With a total basin relief $H_b \approx 2300$ m (from the 1700 m fan apex to the 4000 m peaks) and a drainage area $A_b = 34.4$ km², the Melton number is approximately 0.39. Following regional thresholds for Alpine catchments (e.g., Wilford et al., 2004; Bertrand et al., 2013; Marchi et al., 2019), a Melton number around 0.39–0.40, combined with the fan slope of 7–9%, places the overall Étançons basin right at the transition boundary between bedload/debris-flood dominated systems and debris-flow basins. In contrast, the Bonne Pierre subcatchment (slopes 20–40%, high relief, small area) exhibits a much higher ruggedness typical of debris flow generation. This morphometric duality perfectly mirrors the observed physical behavior: the authors should explicitly articulate the spatial and temporal rheological cascade, wherein an upstream granular debris flow and intense channel scour in the steep (20–40%) proglacial gorge (Parts I and II) transitioned through the confined bedrock canyon (Part III) with substantial liquid dilution from the Upper Étançons, into an intense hyperconcentrated, bedload-dominated debris flood across the unconfined alluvial fan (Part IV).

Some simple morphometric analyses will be added to the site description, showing that the entire catchment and the Bonne Pierre torrent are classified as “debris-flow” systems. The upper Étançons, which contributed a large amount of water at the confluence and thus facilitated the efficient transfer of material eroded from the Bonne Pierre torrent to the downstream fan, lies at the transition between fluvial and debris-flow processes. The transition from highly concentrated to more dilute flows, and the associated increase in flow mobility, are discussed in the section on spatial connectivity.

Regarding the alluvial fan setting and the sediment budget derived from airborne LiDAR differencing, several points merit attention. The area of the La Béarde alluvial fan should be explicitly stated in Section 2.1 to allow readers to appreciate its modest size

relative to the total catchment, and the truncation of the fan toe by the energetic lateral erosion of the Vénéon river should be highlighted immediately when discussing sediment evacuation.

Will be done, adding it to the site description section.

In Table 1, the sediment balance indicates -280,000 m³ from Bonne Pierre, -10,000 m³ from Upper Étançons, and +199,000 m³ deposited on the fan, leaving an unresolved deficit of ~91,000 m³. Line 496 simply asserts that the missing 100,000 m³ were transported to the Vénéon. This exported volume must be explicitly formalized in Table 1 as a closed mass-balance term, accompanied by a brief discussion of its morphological impacts on the Vénéon trunk stream (such as bed aggradation, temporary damming, or bar formation).

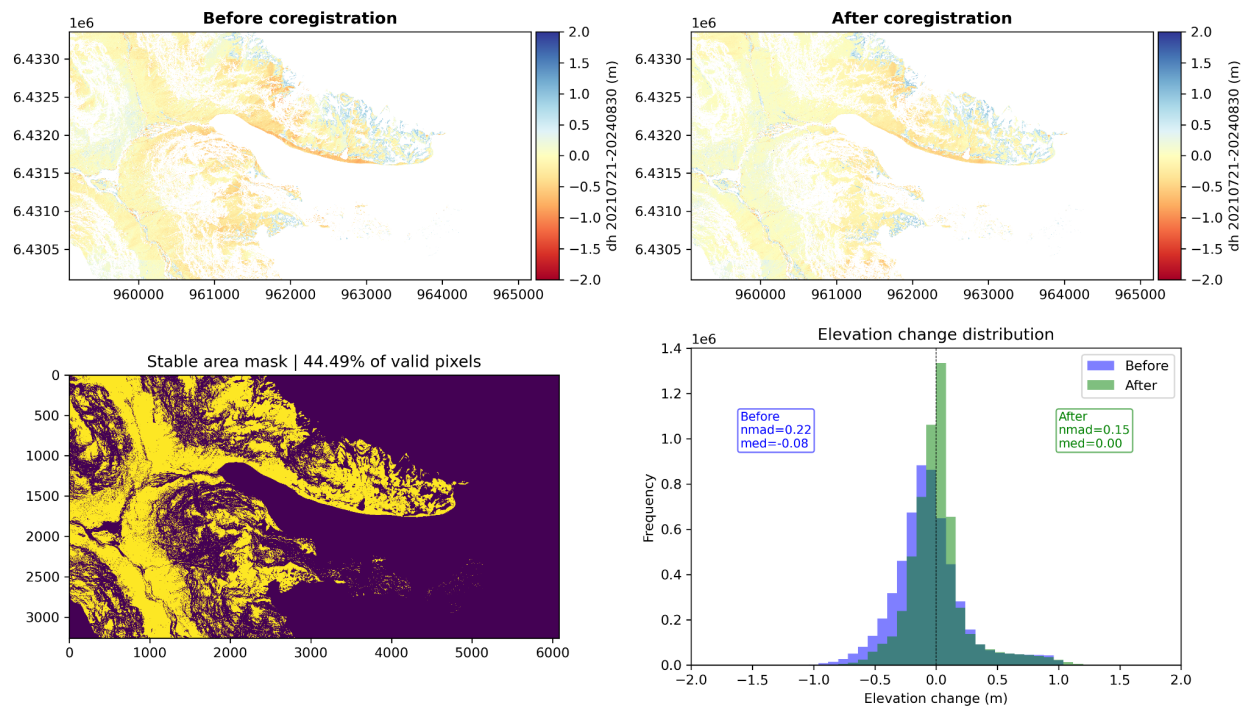
Will be done, removing the confusion, and the rough roundings.

Furthermore, regarding the Level of Detection (LoD) applied to the Difference of DEMs (DoD), the authors adopt a constant LoD of 0.1 m based on literature citations. A constant LoD is a perfectly valid and practical approach when morphogenic signals are massive. However, rather than simply asserting the 0.1 m threshold by citation, the authors should provide data-driven evidence justifying this choice (such as reporting the standard deviation of elevation residuals or vertical RMSE on stable bedrock surfaces). Additionally, it would be geomorphologically valuable to include a comparative map illustrating the raw DoD versus the LoD-thresholded DoD (or a noise mask), allowing readers to visually appreciate the spatial distribution of noise removal across the complex alpine topography.

Thanks to your suggestion, we estimated the empirically uncertainties for the DoDs to estimate sediment volume change and glacier geodetic mass balance using stable areas (bedrock) as references. After differencing, we can have an empirical uncertainty associated with each pair of DEMs. It is to be noted, these estimates are not necessarily homogeneous across the domain as they can have some dependencies to elevation and slope. We will add a sentence on how we evaluated uncertainty in the method section, and add these uncertainties in their corresponding result sections; the paragraph mentioning the LoD from literature removed.

From the airborne lidar DEMs from June 28 2024 and July 21 2021, we obtained an uncertainty of ± 0.15 m (NMAD in the figure below).

Coregistration of LiDAR DEMs | 20240628 - 20210721



This figure will not be added to supplements as we are only interested in the NMAD value.

Spatial sediment connectivity is highlighted in the manuscript title and Section 7.3 as a governing determinant of the disaster. The application of the Cavalli et al. (2013) Index of Connectivity (IC) provides an elegant illustration of why the Bonne Pierre branch acted as an efficient, directly coupled sediment conveyor, whereas the Upper Étançons was effectively decoupled from the fan by wide, low-gradient (4–9%) braided reaches that acted as sediment sinks. The authors could elevate this insight by discussing the interplay between structural and functional connectivity: the Upper Étançons acted as a 'water pump'—it contributed massive liquid runoff from snowmelt and rain without substantial bedload export, which entered the confluence and supplied the excess transport capacity required to flush the massive sediment pulse supplied by the Bonne Pierre torrent through the canyon and onto the fan. This paraglacial response should also be contextualized within the broader literature on postglacial evolution of alpine valleys (e.g., Ballantyne, 2002; Jarman et al., 2011; Brardinoni et al., 2018), highlighting how glacier retreat since the Little Ice Age (~1850) has exposed bounded, highly mobilizable sediment stocks governed by paraglacial exhaustion dynamics. Please also

clarify the computational parameters used for IC in Figure 11, including DEM grid resolution, weighting factor computation and at which scale and why, the target definition (fan apex vs. catchment outlet... or hydro-net), and justify the 250 m grid choice.

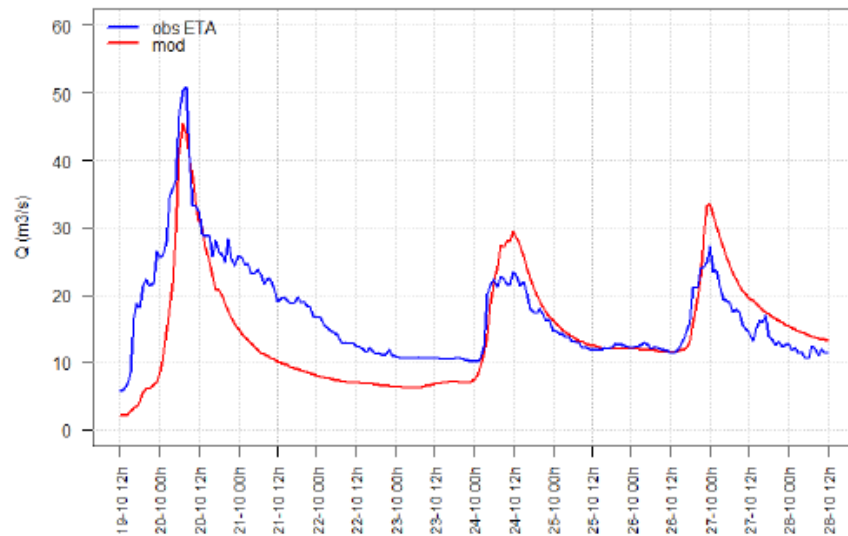
We will add further details on the calculation of IC and its overlap with the loose-material data. We will also specify throughout the discussion whether structural or functional connectivity is being considered. We'll add several sentences discussing the interplay between the two, which is an important observation from this event.

The glacio-hydrological and seismological analyses provide crucial constraints on the event chronology. The authors present an elegant hydraulic back-calculation in Section 7.1 and Appendix A12 showing that an instantaneous breach (<30 min) of the ~100,000 m³ supraglacial lake is ruled out: empirical debris flow equations and 2D Voellmy routing indicate that a sudden outburst would have generated peak discharges of 1,200–3,000 m³/s and surge depths of 10–17 m on the fan, which starkly contradict post-event flood marks (3.0–3.5 m in the gorge) and the survival of the main bridge (clearance 5.5 m). This hydraulic deduction is reinforced by the observed flow rheology: the fact that the distal channel and fan experienced a sustained debris flood rather than a catastrophic debris flow surge confirms that the subglacial conduit network acted as a hydraulic choke, delivering a prolonged release over several hours. However, when attributing the residual discharge peak (the gap between the MORDOR-SD simulated 66 m³/s and the hydraulic peak of 85–110 m³/s) to the lake and cavity drainage, the authors should acknowledge alternative or compounding hydrological uncertainties, such as radar underestimation of localized orographic convection (Mont Moucherotte radar was down, leaving Mont Colombis at 50 km), rapid runoff from bare rock faces, and sediment bulking inflating stage at the destroyed gauging station.

We agree that the uncertainties of the hydrological modeling of this event are potentially significant. However, some complementary comments can be provided regarding the two sources of uncertainties that are mentioned:

- Precipitation reanalysis for model forcing: several options and settings have been tested for the precipitation reanalysis regarding the smoothing of the radar guess fields, and the choice of ground rain gauges to be assimilated in the reanalysis to correct the possible radar underestimation. The reference reanalysis for the model forcing provides the closest 48h precipitation amount to the measurements at the nearby mountain huts (used as control values, see Table A1 reference). The alternative reanalysis tend to provide lower estimates than the reference documented in the manuscript.

- Rapid runoff response during an intense event : the MORDOR-SD model has been calibrated on the complete 2019-2023 period, during which a severe flood has been recorded in October 2023, with three consecutive peaks, the highest reaching 50 m³/s (16 m³/s below the modeled value of June 2024). The following figure shows the ability of the calibrated model in modelling such a rapid runoff event (observed discharges are blue, modelled are red).



- Sediment bulking inflating stage at the destroyed gauging station : as stated in §5.1 of the original manuscript , *“the sensor recorded the river water level until June 21, 03:00 (UTC), reaching a maximum hourly water level close to 2.80 m (eq. 80 m³ s⁻¹) [...] . After that, the station was destroyed during the event by unidentified floating objects and therefore stopped recording”*. Later in this paragraph, we consider that *“given the historical flood events at ETA (four reaching 2.4 m), the rating curve being validated to 2.4 m (eq. 50 m³ s⁻¹), we are confident in the observation up to 60 m³ s⁻¹ [...] . Beyond this discharge value, as the station was eventually destroyed (without knowledge of the time), our confidence in the measurements and the associated discharge dynamic passed June 20 22:00 UTC (dashed black line in Fig. 9a) decreases”*. Then we mentioned the uncertainties of the post-event hydrological modeling, based on a lidar survey of the modified bed : *“because of large uncertainties due to this morphogenic event (though constrained by the flood marks), we estimated a range of possible instantaneous discharge peak from 85 to 110 m³ s⁻¹”* leading to *“[being] confident that the peak discharge reached at least 85 m³ s⁻¹”*.
- Large pieces of wood were found around the site and marked the maximum flow level along with mud marks and water marks, some debris were also found nearby the gauging station. We do not know how these materials might have increased the flow level inducing a deviation of the rating curve. We now clearly

state in the text that the modelling is done neglecting the effect of large pieces of wood and floating debris.

We will summarize these sources of uncertainty in the appendix.

Similarly, the seismological interpretation of the 0.4–0.5 Hz signal at station OGAG (25 km away) warrants caution: while high-frequency noise (2–10 Hz) are correctly referred to fluvial turbulence and bedload collisions in the adjacent Durance River, the 0.4–0.5 Hz band suffers from overlaps with secondary oceanic microseisms; The authors should consider and discuss whether this low-frequency energy (and the corresponding polarization shift at OGAG) could instead reflect discrete, large-scale mass movements—such as major bank collapses, moraine slumps, or bedrock gorge failures along the Bonne Pierre branch—rather than continuous fluvial bedload transport.

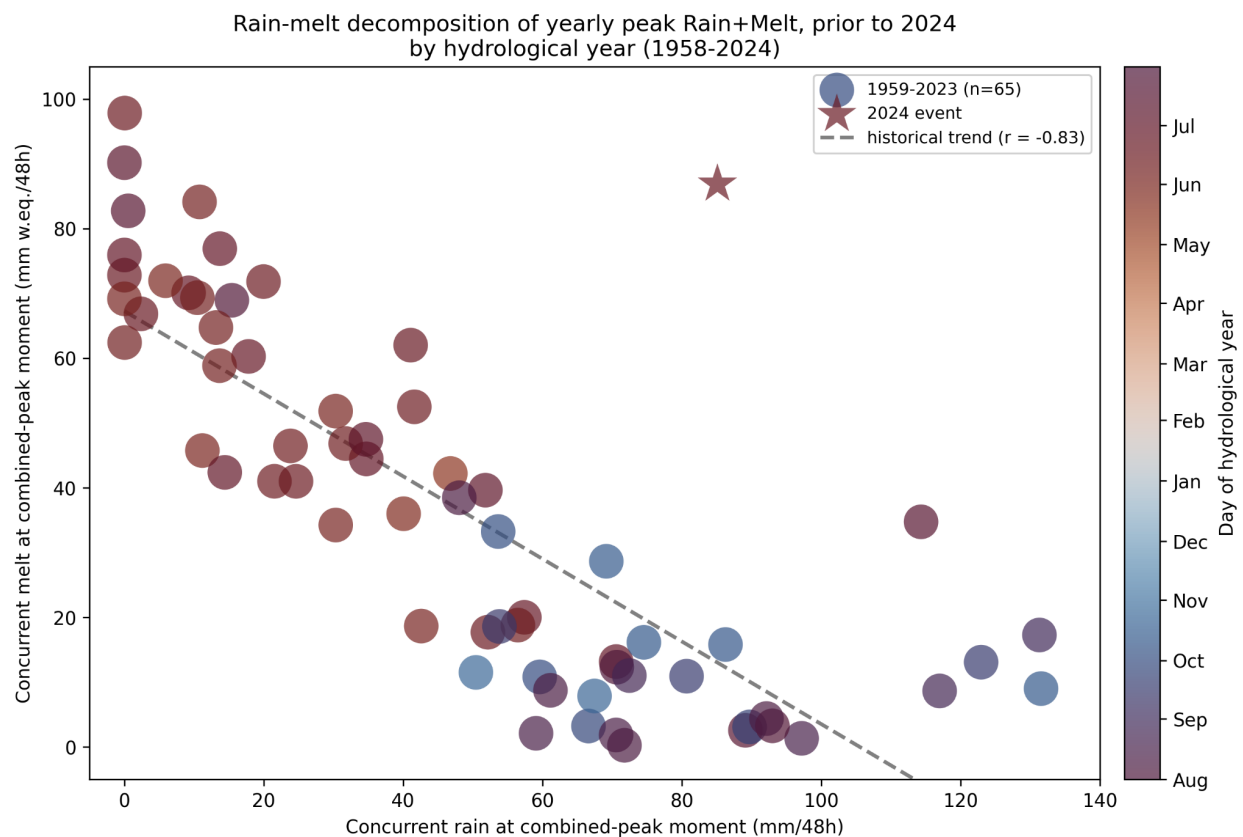
In our analysis, we ruled out oceanic noise as the sole source of the signal, as the WMSAN package (Tomasetto et al., 2024) failed to reproduce the observed energy increase in the 0.4–0.5 Hz band during the event. Additionally, no large-scale mass movements were observed in the Bonne Pierre glacier vicinity or elsewhere in the study area during the event period. In addition the mass movement mentioned (eg., major bank collapses, moraine slumps, or bedrock gorge failures), would have generated a shorter duration signal compared to the observed ones. The signal's timing, polarization shift at OGAG, and coherence across the three stations align closely with the rapid increase in river discharge at ETA, supporting our interpretation of flow-related activity in the Vénéon valley. To further clarify, we will add a sentence in the revised manuscript explicitly stating the absence of observed sudden mass movements.

Acknowledging this potential mixed source (slope failures vs. fluvial dynamics) and the complexities of 3D wave propagation in steep alpine terrain would make the seismological interpretation much more robust.

Thanks for the comment. See our answer to the above comment

Regarding the statistical return periods in Section 6.2 and Appendix A14 (estimated at ~7–15 years for rainfall, ~22–57 years for snowmelt, and ~153–157 years for total runoff), the authors rightly note that fitting a univariate GEV to aggregated runoff ignores seasonality. In the Western Alps, intense Mediterranean rainstorms typically occur in autumn, whereas peak snowmelt is confined to late spring. The co-occurrence of a 10-year rainstorm over a ripe, 90th-percentile snowpack in late June is an exceptionally rare joint event. The authors should frame this limitation clearly, presenting the 153-year estimate as a simplified baseline and highlighting multivariate copula or joint probability approaches as the appropriate conceptual frontier for compound mountain hazards.

Thank you for the suggestion. You are right to point out that because of the seasonality of both intense rain and of maximum snowmelt, we most likely underestimate the actual return period. But because we have no historical precedent to such an event in our datasets, we cannot rely on a sufficiently representative historical record to accurately estimate such probability. Below is a modified version of the plots in the manuscript. This plot shows the decomposition of the yearly peak melt+rain runoff into its constituent rain and melt water equivalent, for a window of 48h. We can observe that the June 2024 event is completely out of the range of historical records. The dashed line shows a fitted linear regression ($r=-0.7$) to the records until hydrological year 2023. These data speak for themselves as how, based on our historical records (from August 1958), this event is an outlier. This is why we believe that it would not be appropriate to apply the statistical bivariate copula.



3. Specific Recommendations and Concluding Remarks

A series of specific textual, editorial, and typographical corrections will help polish the manuscript. In the title and throughout the text, replace 'evidences' (lines 6, 592, 608, 621, 796) with 'evidence' (uncountable noun), and in line 20 replace 'colluding in time and space' with 'co-occurring' or 'synchronizing' (colluding implies deliberate human

conspiracy).

Good catch. Will be corrected.

In lines 14–15, correct the grammatical agreement to 'an era in which the Alps are entering a phase of renewed paraglacial adjustment'.

Will be done

In the introduction (lines 39–44), shorten the description of the Chamoli event as the citation to Shugar et al. (2021) is sufficient, and delete the redundant sentence 'The detached mixture of rock, ice and water collapsed...'; ensure a clear physical distinction between massive rock-ice avalanche cascades and hydrometeorological-thermokarst compound triggering over paraglacial sediment stores, perhaps mentioning the 2023 South Lhonak/Sikkim GLOF (Sattar et al., 2025).

Thank you for the suggestion. Sattar et al 2025 will be added and the redundancy removed.

Figure 2 (the historical painting by Abbot Guétal) provides cultural context but does not convey essential scientific information; it could be moved to the site description or relegated to the Appendix.

If you agree, we propose a compromise, combining figure 1 and 2. Showing this historical painting in the main paper is a deliberate and, to our opinion, a sensible choice. While not adding materialistic data to the study, it embodies all elements of the event: the village, the torrent, the snow, the glaciers, and the threat one may feel in this place. This painting has therefore the ability to draw in and contextualise the event to a broader audience than what purely rational arguments may accomplish. Paintings from the impressionist period have that ability to capture attention.

In Section 3.1, it would be valuable to add brief operational details on the warning procedures, civil protection coordination, and the timing of the helicopter evacuation that successfully prevented loss of life, as this is a key takeaway for disaster risk management.

Thank you for the suggestion. This will be added.

Catchment and subcatchment areas must be reported consistently and explicitly: in lines 103–106, please provide the absolute drainage areas (in km²) for the Upper Étançons and Bonne Pierre subcatchments rather than stating that the former is '2.5 larger', and harmonize the 34.4 km² in line 100 with the 33.8 km² in Table 2 and the 8.7 km² and 3.3 km² values in line 503.

Good catch. This was a typo and will be corrected.

Correct the typographical error 'alluvion fan' to 'alluvial fan' (line 130) and 'progacial cone' to 'proglacial cone' (lines 124–128).

Will be done

In lines 139–143, justify the physical rationale for selecting a 48-hour integration window for precipitation and melt (explaining whether it matches storm duration, moving the explanation from lines 211–213 here).

Will be done

In Section 5.1, clarify the hydraulic control and failure threshold at the Les Étages (ETA) station (exact timestamp and stage at 2.87 m / ~80 m³/s where bed aggradation invalidated the rating curve). In the hydrological modeling description, report the Kling-Gupta Efficiency (KGE) alongside the Nash-Sutcliffe Efficiency (0.86) and describe how the -0.6°C/100m temperature lapse rate was verified against high-altitude loggers.

The Kling-Gupta Efficiency (KGE) computed on the calibration period is 0.93.

This has been added in the section 3.2.4 of the revised manuscript.

The temperature lapse rate of -0.6°C/100 m used for the modelling of the whole event has been estimated by averaging the hourly lapse rates of the event computed by linear regression of the local temperatures against altitude at six neighbouring weather stations (including ECR, the highest one).

This will be added in the section A10 of the revised manuscript.

In line 594, replace 'we may incriminate the role of the GLOF' with 'we can attribute an important role to the GLOF'.

Will be done.

In the figures, increase the font size and contrast on cross-sections AA' and BB' in Figure 4 to ensure elevation labels are legible in print.

Will be done.

In conclusion, this manuscript documents an extraordinary alpine disaster with an invaluable interdisciplinary empirical foundation. The work is already very strong, and the recommendations provided in this report are intended solely as constructive suggestions to assist the authors in refining the structural cohesion, geomorphological depth, and conceptual clarity of their study. By addressing these points, the authors will further strengthen an already important paper, turning it into a benchmark reference for compound and cascading hazards in deglaciating mountain environments.

The depth and detail with which each of the raised points may be addressed are left to the authors' discretion; on a "base-level straightforward scenario" several specific observations may well be bypassed or addressed simply by adding concise clarifying commentary directly in the text.

Declaration on AI tools: Google Gemini 3.7 was used as an assistive tool exclusively for text editing, stylistic polishing, and syntactic refinement of the reviewer's original scientific comments and evaluation.