Author Comment in response to Referee #1

Comments from Referee #1:

The presented article has a very broad scope and considerable synthetic value. It attempts to combine physical mechanisms, environmental conditions, social and economic impacts, risk management, as well as technical measures and nature-based solutions. This is fully justified given the interdisciplinary nature of the problem. The extensive literature base of 1,967 publications provides potential for identifying global trends. The paper describes well the problem of non-stationarity, highlighting the limited transferability of rainfall thresholds, emphasizing the initial state of the catchment, and pointing out the non-stationarity of climate. The causes of flash floods are correctly presented as a combination of rainfall, soil moisture, topography, hillslope–channel connectivity, sediment, and anthropogenic changes. The article also successfully links science and practice by emphasizing the importance of warning systems, spatial planning, catchment management, and hybrid retention solutions.

Response to General Comments:

We sincerely thank the reviewer for the careful and constructive assessment of our manuscript. We appreciate the reviewer's recognition of the broad scope and synthetic value of the review, as well as the comments on its interdisciplinary structure, literature base, treatment of non-stationarity, discussion of rainfall-threshold transferability, and connection between scientific understanding and practical risk-reduction measures.

We also appreciate the reviewer's detailed identification of issues that require further revision. These include conceptual precision, case-selection transparency, representativeness, interpretation of publication and event trends, regional comparison, uncertainty propagation, machine-learning limitations, cost and trade-off considerations, institutional constraints, and the need to clarify the main object of the review.

In the revised manuscript, we will address these points through targeted revision and re-reading of relevant evidence within the existing corpus. The main focus will be on clarifying definitions, qualifying the interpretation of bibliometric and event evidence, strengthening regional and methodological limitations, and making the discussion of management measures more balanced and context-specific.

Detailed responses are provided below.

Comment 1

However, the excessively broad scope of the analyzed problem comes at the expense of conceptual precision and clarity. The authors use the terms hazard, disaster, risk, impact, vulnerability, and resilience in a manner that is not fully differentiated. In a study of this type, clearer conceptual structuring would be expected.

Response to Comment 1

We thank the reviewer for this important comment. In the revised manuscript, we will further develop the conceptual framework in the Introduction and align the later sections with this framework. In particular, the revised structure will more clearly follow the sequence from physical hazard formation, to exposure – vulnerability – capacity conditions, to realised impacts and cascading consequences, and finally to risk-reduction and governance measures.

Specifically, we will clarify that flash-flood hazard refers to the physical flood process and its characteristics, including rainfall forcing, runoff generation, flow velocity, inundation extent, sediment load, and timing. Disaster risk will be defined as the potential for loss arising from the interaction of hazard, exposure, vulnerability, and capacity. Realised impacts will refer to the observed social, economic, infrastructural, and ecological consequences once an event occurs. Vulnerability, capacity, and resilience will be discussed in relation to the ability of exposed communities, infrastructure, and ecosystems to resist, cope with, respond to, and recover from flash-flood events.

We will also revise the impact and management sections so that this terminology is used consistently. The new section will distinguish exposure, vulnerability, capacity, and realised consequences, while the section on risk-reduction measures will explain how different interventions act on different components of risk, including hazard processes, exposure, vulnerability, response capacity, and residual losses.

Comment 2

The criteria for selecting bibliometric cases are also unclear, and there is no discussion of their representativeness. The study covers the years 2000 – 2025, yet some conclusions regarding publication trends, risk growth, and climate change are formulated as if all periods had equal data quality. In reality, the increase in the number of publications may also reflect the development of science rather than solely the growing importance of the problem, and the rise in the number of documented events may partly result from improved reporting.

Response to Comment 2

In the revised manuscript, we will clarify the distinction among the 1,967-paper bibliometric corpus, the documented flash-flood cases identified from the literature, and the smaller event subset used to illustrate rainfall-threshold variability. Specifically, we will expand the methodological description of the literature search and screening process. We will explain how records were retrieved from Web of Science and Scopus, how duplicate records were removed, and how studies were screened according to the mountain flash-flood scope. Documented case counts will be interpreted as evidence concentrations in the peer-reviewed literature. We will clarify that these counts are affected by database coverage, language, monitoring density, institutional research capacity, reporting practices, and post-disaster documentation. Therefore, they will not be presented as direct measures of global flash-flood frequency or disaster risk.

We also agree that the 2000–2025 period should not be treated as uniform in data quality. In the revised manuscript, we will qualify statements about publication trends, documented event patterns, risk growth, and climate-change-related interpretations. Publication growth will be discussed as an indicator of research visibility, scientific development, and knowledge accumulation. Documented case patterns will be interpreted more cautiously. This revision will make the temporal interpretation more cautious and will reduce the risk of overinterpreting bibliometric growth or documented case counts as direct evidence of increasing hazard occurrence.

For climate-change-related statements, we will make clear that the evidence comes from cited observational, modelling, or projection studies within the reviewed literature; it is not inferred from our bibliometric trend analysis.

Comment 3

There is also some lack of clarity in the presentation of the analyzed regions. Examples are drawn from China, India, and Europe, but there is no systematic comparison between areas with similar climate, geomorphology, or levels of urbanization.

Response to Comment 3

In the revised manuscript, we will retain the country-level analysis for bibliometric outputs and documented-case distributions, because this is the most consistent spatial unit available across the current corpus. We will then add a short subsection after the current Section 3 to discuss regional contrasts in flash-flood triggering and amplification, using representative cases from the existing corpus.

This new subsection will compare several broad settings represented in the literature, including humid monsoon mountains, arid wadi systems, Mediterranean and semi-arid mountain basins, cryosphere-influenced high mountains, and urbanising or engineered mountain-front catchments. The comparison will focus on triggering mechanisms and process controls. We will also clarify that rainfall- or runoff-driven mountain flash floods remain the main focus of the review, while snowmelt, GLOFs, landslide-dam breaches, dam failures, wildfire effects, and infrastructure failures will be discussed as modifiers, triggers, or cascading pathways in specific mountain settings.

Comment 4

The article also lacks discussion on uncertainty propagation, input data errors, model validation in poorly monitored regions, and the issue of overfitting in machine learning models.

Response to Comment 4

In the revised manuscript, we will further consolidate the discussion of uncertainty in the monitoring – modelling – warning chain. This will include uncertainties related to rainfall observations, radar and satellite precipitation products, DEM resolution, soil and land-cover parameters, event inventories, impact records, and model structure.

We will also strengthen the discussion of model validation and machine-learning applications in data-scarce mountain regions. The revised text will clarify that sparse gauges, limited post-event surveys, incomplete historical records, and uneven training samples can constrain model calibration, validation, and transferability. For machine-learning methods, we will add a more cautious discussion of overfitting, interpretability, and dependence on representative training data, especially where extreme-event samples are small or regionally biased.

Comment 5

Furthermore, there is no analysis of costs, cost-effectiveness, trade-offs between objectives, or institutional and political constraints.

Response to Comment 5

We thank the reviewer for pointing out this important limitation. In the revised manuscript, we will make the existing discussion of costs, maintenance, trade-offs, and institutional constraints more explicit in the management section. This will include maintenance and service-life issues for structural measures, sediment filling and design exceedance, establishment time and land requirements for Nature-Based Solutions, and trade-offs among flood reduction, sediment continuity, ecological function, local livelihoods, and long-term management capacity.

We will also clarify institutional and implementation constraints where supported by the reviewed literature, including local management capacity, funding limitations, coordination between agencies, and the feasibility of long-term maintenance. The above discussion will be presented as a qualitative synthesis based on existing literature.

Comment 6

The article strongly promotes integrated, hybrid, and multi-scale approaches; however, greater integration does not always necessarily lead to better outcomes. The intention to include events of different origins that may trigger flash floods—such as classical flash floods, flood cascades, dam-break floods, and glacial lake outburst floods — is understandable. Nevertheless, this approach tends to blur the main object of the study.

Response to Comment 6

We agree that integrated, hybrid, and multi-scale approaches need to be discussed with more caution. In the revised manuscript, we will tone down broad statements that may imply that greater integration always leads to better outcomes. We will clarify that the effectiveness of integrated approaches depends on local hazard processes, exposure patterns, sediment regime, available land, funding, maintenance capacity, institutional coordination, and community acceptance. We will also explain more clearly how different measures act on different components of risk, including hazard reduction, exposure control, vulnerability reduction, response capacity, and residual-risk management.

We also agree that the inclusion of flash floods, flood cascades, dam-break floods, and glacial lake outburst floods requires a clearer definition of the review scope. In the revised manuscript, we will clarify that rainfall- or runoff-driven flash floods in mountainous catchments remain the main focus of the review. Dam-break floods, glacial lake outburst floods, landslide-dam breaches, debris-flow transitions, wildfire effects, and infrastructure failures will be discussed only where they act as triggering factors, amplifying mechanisms, or cascading pathways connected to mountain flash-flood risk. We will also revise the terminology and related sections to make this distinction clearer throughout the manuscript.

Author Comment in response to Referee #2

Comments from Referee #2:

This manuscript offers a broad synthesis of flash-flood research in mountainous regions, drawing on 1,967 peer-reviewed studies published between 2000 and 2025. The combination of bibliometric mapping, dynamic topic modelling, process-based synthesis, and governance perspectives is genuinely interdisciplinary, and the overall structure — from global trends to hydrological mechanisms, cascading impacts, and adaptive governance — is logical and well organised.

However, the manuscript does not fully deliver on several stated ambitions. The most significant issue is the incomplete integration between the bibliometric component and the qualitative synthesis. The Dynamic Topic Modelling results function largely as a standalone section rather than shaping the subsequent process discussion. Rainfall thresholds, early warning systems, and cascade mechanisms are often framed in general terms, despite being derived from a spatially unrepresentative corpus.

A related tension concerns the claim to establish a "globally comparable baseline." Semi-arid mountain environments, African highland systems, and Andean catchments remain underrepresented, meaning several key findings rest on a corpus more regionally specific than the framing implies. The two conceptual contributions highlighted in the abstract — the "state-dependent triggering function" and the "threshold surface" framework — are promising but remain underformalised and disconnected from the operational sections that follow.

In my view, the manuscript could become suitable for publication in NHESS, but it still requires major revision. The comments below aim to help the authors strengthen its conceptual depth, geographic balance, and critical rigour.

Response to General Comments:

We sincerely thank the reviewer for the detailed, constructive, and thoughtful assessment of our manuscript. We are grateful for the positive evaluation of the manuscript's interdisciplinary scope, bibliometric foundation, process-based synthesis, governance perspective, and relevance to the NHESS community. We also fully acknowledge the reviewer's concerns regarding the incomplete integration between the bibliometric component and the qualitative synthesis, the need for greater methodological transparency, the uneven geographic representativeness of the evidence base, and the underdeveloped treatment of sediment connectivity, cascading hazard interactions, machine-learning limitations, and Nature-Based Solutions.

We also agree that the phrase "globally comparable baseline" may overstate the degree of geographic representativeness that the current corpus can support. Our intention was not to claim that the existing literature provides an evenly distributed global evidence base. Rather, we aimed to organise the available cross-regional evidence and identify where it is concentrated, fragmented or missing. In the revised manuscript, we will therefore replace this wording with a more cautious formulation, such as "a structured synthesis of available cross-regional evidence while explicitly identifying geographic and thematic biases." This change will be made consistently in the Abstract, Introduction and contribution statements.

We also appreciate the reviewer's observation that the "state-dependent triggering function" and "threshold surface" concepts are promising but insufficiently connected to later operational sections. In the revised manuscript, we will clarify these concepts as a conceptual framework rather than as a calibrated predictive model.

In response to the reviewer's comments, we will undertake a substantial revision of the manuscript. The revision will focus on:

(1) strengthening the integration between bibliometric results and qualitative synthesis;

(2) improving the methodological transparency and limitation discussion of the DTM framework;

(3) clarifying the representativeness and limitations of the 42-event rainfall-threshold subset;

(4) integrating geographic imbalance into process interpretation and management recommendations;

(5) developing sediment connectivity and cascading hazard chains more explicitly; and

(6) providing a more critical and evidence-based treatment of machine learning and Nature-Based Solutions.

Below we respond to each comment in detail.

Comment 1 :

Machine learning discussion is insufficiently critical

In Section 4.1, machine learning approaches are presented mainly through performance improvements. Important limitations receive little attention: model transferability across hydro-climatically contrasted basins, uncertainty propagation in data-sparse environments, interpretability constraints, and dependence on large training datasets. These issues are

especially relevant for the poorly gauged mountain regions discussed elsewhere. The claim that deep-learning models can "skilfully predict extreme events in ungauged watersheds" (ll. 480–482) deserves more cautious framing. A more balanced treatment of capabilities and limitations would considerably strengthen this section.

Response to Comment 1:

We thank the reviewer for this important comment. In the revised manuscript, we will substantially revise Section 4.1. We will reframe machine learning as a conditional decision-support tool rather than as a stand-alone solution for ungauged-basin prediction. Specifically, we will add a more critical discussion of four limitations: model transferability across hydro-climatically contrasted basins, uncertainty propagation from sparse observations and remote-sensing inputs, limited interpretability of deep-learning models, and dependence on large and representative training datasets. These limitations are particularly important for mountain flash floods because extreme events are rare, monitoring networks are uneven, and training samples are often regionally biased.

We will also tone down the statement that deep-learning models can “skilfully predict extreme events in ungauged watersheds.” The revised wording will clarify that such models may improve predictive skill in data-rich or well-calibrated contexts, but their transferability to ungauged mountainous basins remains uncertain. This revision will make the section more balanced and more consistent with the manuscript’s broader emphasis on data scarcity, uncertainty and non-stationarity.

Comment 2 :

Methodological transparency of the DTM framework

The Dynamic Topic Modelling approach is informative, but its limitations are not discussed. The sensitivity of topic classification to keyword selection, corpus composition, and differential coverage between WoS and Scopus is not addressed. Although the authors mention "deterministic training protocols" (l. 86), the implications for topic boundaries and temporal trends are unexplored. Greater methodological transparency would improve reproducibility and allow readers to assess the robustness of the thematic structure underpinning the review.

Response to Comment 2:

We agree that the Dynamic Topic Modelling framework was useful but not sufficiently transparent in the previous version. The manuscript did not adequately explain how topic boundaries were affected by keyword selection, corpus composition, WoS–Scopus coverage differences and deterministic training choices.

In the revised manuscript, we will expand the Methods section and Supplement to describe the DTM workflow more clearly. We will provide additional information on corpus construction, WoS–Scopus deduplication, inclusion and exclusion criteria, topic-number selection, topic-label interpretation, manual validation and the limitations of interpreting topic trends from an uneven literature corpus. We will also clarify that DTM results represent the thematic structure of the indexed literature, not the actual global frequency or severity of flash-flood events.

More importantly, we will strengthen the link between DTM results and the subsequent qualitative synthesis. The DTM themes will be used as an organising bridge into later sections, including rainfall forcing, arid wadi flash flooding, sediment processes, special geological settings, risk management and eco-climatic impacts. This will prevent the DTM section from functioning as a stand-alone bibliometric result and will make it more clearly support the process and governance synthesis.

Comment 3 :

Representativeness of the 42 flash-flood events

The synthesis of 42 events in Figure 3 is visually effective, but the representativeness of this sample is insufficiently discussed. Given the dominance of Asian case studies, it is unclear whether the reported intensity-duration relationships capture hydro-climatic variability across semi-arid Mediterranean, North African, or South American environments. The selection criteria for these 42 events from 1,967 studies are not stated, and potential biases are not acknowledged. This limitation directly affects the validity of the derived thresholds and their transferability.

Response to Comment 3:

We thank the reviewer for this helpful comment. We agree that Figure 3 is useful for illustrating rainfall intensity–duration variability, but the representativeness of the 42-event sample was not sufficiently explained in the previous manuscript.

In the revised manuscript, we will clarify that these 42 events are an illustrative reported-event subset rather than a statistically representative global sample. We will add explicit selection criteria, including whether the event was reported in peer-reviewed literature, whether the location and mountainous catchment context were clear, and whether rainfall intensity, duration or accumulated rainfall information was available. Events lacking sufficient triggering-rainfall information will not be used for threshold interpretation.

We will also add a limitation paragraph explaining that Asian and well-monitored catchments are overrepresented, while semi-arid Mediterranean basins, North African mountain systems and South American Andean catchments remain underrepresented. Therefore, Figure 3 will be reframed as evidence of threshold variability and state dependence rather than as a basis for a globally transferable rainfall threshold to response concern about the validity and transferability of the derived thresholds.

Comment 4 :

Nature-Based Solutions lacks comparative evidence

Section 4.2.2 presents NbS positively but remains largely conceptual. Key dimensions are underdeveloped: long-term effectiveness under increasing rainfall extremes, maintenance costs over decadal timescales, scale dependency of flood attenuation effects, and comparative performance relative to structural measures. The manuscript acknowledges that plant growth takes time (l. 619) but does not explore how this time-lag interacts with near-term flood risk. A more evidence-based treatment would improve the practical relevance of this section.

Response to Comment 4:

We agree that the previous version presented Nature-Based Solutions in a relatively positive and conceptual way, without sufficiently discussing comparative evidence, long-term effectiveness, maintenance requirements, scale dependency and performance under increasingly extreme rainfall.

In the revised Section 4.2.2, we will restructure the discussion around "effectiveness, time lag and implementation limits." We will clarify that NbS should not be interpreted as universally effective substitutes for structural measures in mountain flash-flood mitigation. Instead, their effectiveness depends on event magnitude, implementation scale, catchment position, vegetation maturity, available storage and maintenance.

We will add evidence from the corpus showing that green infrastructure and vegetation-based measures can attenuate runoff and flood peaks when sufficiently extensive and properly located, but that their benefits are often delayed and spatially variable. We will explicitly discuss the time lag of vegetation-based NbS, because improvements in soil structure, infiltration and hydrological regulation may require years to decades. This means that NbS alone cannot fully address near-term flash-flood risk during the establishment period.

We will also compare NbS with structural measures such as detention basins, check dams and torrent-control works. Structural measures can provide more immediate and quantifiable peak-flow reduction, but they also require maintenance, may lose storage capacity through sediment filling, and can alter sediment connectivity or ecological conditions. The revised manuscript will therefore frame NbS as part of an integrated grey–green portfolio.

Comment 5 :

Sediment connectivity is never conceptualised

Hillslope–channel connectivity is repeatedly cited as a key control on flash-flood initiation in Sections 3.1.1–3.1.2, yet sediment connectivity is never developed as a conceptual framework. A clearer distinction between hydrological and sediment connectivity would strengthen the process synthesis. Integrating frameworks such as the sediment cascade concept, the connectivity index, and source–transfer–sink organisation would help clarify the conditions under which flash floods transition toward hyperconcentrated flows or debris flows — a distinction that remains underspecified throughout.

Response to Comment 5:

Sediment connectivity was not sufficiently developed as a conceptual framework, even though it is essential for explaining when mountain flash floods remain clear-water floods and when they transition toward hyperconcentrated flows, debris floods or debris flows.

In the revised Sections 3.1.1–3.1.2, we will explicitly distinguish hydrological connectivity from sediment connectivity. Hydrological connectivity will be defined as the event-scale activation of flow pathways linking hillslopes, gullies, headwater channels and the downstream drainage network, thereby controlling runoff concentration and flood timing. Sediment connectivity will be defined as the coupling between sediment sources, transport pathways and downstream sinks, thereby controlling whether loose material can be mobilised and delivered during a flash-flood event.

We will further introduce a source–transport–sink framework. Sediment sources may include shallow landslides, gully erosion, bank erosion, wildfire-affected slopes, earthquake-disturbed deposits, road cuts and pre-existing channel alluvium. Transport pathways include gullies, steep channels, confined valleys and high-energy torrent systems. Sinks include alluvial fans, floodplains, reservoirs, check dams, low-gradient reaches and temporary channel storage.

This revision will clarify that strong hydrological connectivity does not necessarily imply strong sediment connectivity. A catchment may generate rapid runoff but remain sediment-limited if loose material is disconnected from channels or trapped by local sinks. Conversely, when intense runoff intersects with abundant loose material, steep channels and high transport capacity, a clear-water flash flood may develop into a sediment-laden flood, hyperconcentrated flow, debris flood or debris flow. This will provide the missing conceptual bridge between rainfall-runoff generation and geomorphic hazard outcomes.

Comment 6 :

Cascading hazard interactions need structured treatment

Section 3.2 introduces cascading impacts but treats interacting hazard types largely as separate illustrative examples rather than components of coupled hazard chains. The multi-hazard relevance of the review would be strengthened by explicit synthesis of cascade pathways such as wildfire-induced hydrophobicity driving debris flow generation, GLOF triggering sediment-rich flood waves, landslide dam breach flooding, or flash flood-driven infrastructure failure. Figure 4, intended to represent cascading impacts, does not illustrate dynamic coupling between hazard components, limiting its conceptual value.

Response to Comment 6:

We thank the reviewer for this valuable comment. We agree that the previous Section 3.2 treated cascading impacts mainly as separate illustrative examples rather than as coupled hazard chains. We also agree that Figure 4 did not sufficiently show dynamic coupling among hazard components.

In the revised manuscript, we will restructure the cascading-hazard section around explicit pathways. We will also revise Figure 4 from a circular impact inventory into a structured hazard-chain framework. The revised figure will distinguish triggering factors, intermediate hydrological and geomorphic processes, exposed receptors, direct impacts, secondary cascading effects and longer-term consequences. This will better represent the dynamic coupling between hydrological, sedimentary, infrastructural, ecological and social components.

Comment 7 :

Geographic imbalance is not integrated into the synthesis

The manuscript recognises the underrepresentation of Africa and South America but does not integrate this observation into process findings or management recommendations. This creates an internal inconsistency: the Boolean query explicitly targets data-scarce environments through terms such as "wadi" and "steep slope", yet the synthesis remains dominated by European and Asian evidence. Figure 1b documents events in Morocco, Ethiopia, and South Africa, but these regions receive minimal attention in subsequent sections. Mountain environments such as the Atlas ranges, Maghrebian semi-arid basins, and Andean catchments — characterised by ephemeral channels, high sediment availability, sparse gauging networks, and extreme rainfall intermittency — offer important complementary perspectives necessary to substantiate the claim of a "globally comparable baseline."

Response to Comment 7:

We thank the reviewer for this fair and important criticism. We agree that although the previous manuscript recognised the underrepresentation of Africa and South America, this observation was not sufficiently integrated into the process findings or management recommendations.

In the revised manuscript, we will address this issue in three ways. First, we will tone down claims of global comparability and clarify that the review synthesises available evidence while explicitly identifying its geographic and thematic biases. Second, we will include a dedicated discussion of hydro-climatic and geomorphic contrasts among mountain environments, including humid monsoon mountains, Mediterranean and semi-arid basins, arid wadi systems, cryosphere-influenced high mountain basins, African highlands and Andean catchments. Third, we will revise the governance section to avoid implying that evidence derived mainly from European and Asian settings can be transferred directly to underrepresented regions.

We will emphasise that ephemeral channels, high sediment availability, sparse gauging networks, short-duration convective rainfall and intermittent runoff in semi-arid and arid mountain environments may alter rainfall thresholds, sediment connectivity, early-warning feasibility and mitigation choices. This will make the synthesis more geographically cautious and internally consistent.

Comment 8 :

Terminological inconsistency

The terms "flash flood", "debris flow", "hyperconcentrated flow", and "mountain torrent" are used interchangeably in several passages. A brief terminological framework in the Introduction would improve consistency throughout the manuscript.

Response to Comment 8:

We thank the reviewer for pointing this out. We agree that the terms “flash flood,” “mountain torrent,” “hyperconcentrated flow” and “debris flow” were not always used with sufficient precision.

In the revised Introduction, we will add a brief terminological framework. We will clarify that the main focus of the review is rainfall- or runoff-driven flash floods in mountainous catchments. “Mountain torrent” will be treated as a related regional or geomorphic expression of steep-channel flash flooding. “Hyperconcentrated flow” and “debris flood” will be discussed as transitional sediment-laden processes between clear-water floods and debris flows. “Debris flow” will be treated as a distinct but connected geomorphic hazard that may occur when sediment supply, slope, confinement and transport capacity are sufficiently high. GLOFs, dam-break floods and landslide-dam breach floods will be discussed as triggering or cascading pathways rather than as the main review object.

Comment 9 :

Figure 2 readability

The thematic pillar figure is visually dense. Improving label readability and clarifying hierarchical relationships between sub-topics would make this figure more accessible to readers.

Response to Comment 9:

In the revised manuscript, we will redesign Figure 2 as a clearer two-part figure. One panel will summarise the six Dynamic Topic Modelling themes and their publication counts, while the other panel will present the main sub-topics under each theme in a simplified card-based layout. We will reduce the number of labels, enlarge the font size, remove crowded radial text, and revise the caption to explain how the thematic pillars connect to the subsequent qualitative synthesis. This revision will make the figure easier to read and will better show the relationship between the DTM results and the structure of the review.

Comment 10 :

Figure 4 impact levels

The circular layout of Figure 4 does not clearly differentiate between primary flood impacts and secondary cascading effects. A clearer visual distinction between impact levels would improve its analytical value.

Response to Comment 10:

In the revised manuscript, we will redesign Figure 4 as a layered cascading-impact framework. The revised figure will distinguish the flash-flood hazard process, primary impacts, secondary cascading effects, and longer-term consequences. Primary impacts will include inundation, high-velocity flow, erosion, sediment deposition, debris impact, and direct damage to buildings, roads, bridges, farmland, and other exposed elements. Secondary cascading effects will include road interruption, rescue delay, power or communication failure, water contamination, evacuation difficulty, and disruption of services. Longer-term consequences will include economic loss, livelihood disruption, ecosystem degradation, recovery burden, and public-health or social impacts.

Comment 11 :

Transition between Sections 3 and 4

The transition between process mechanisms and management measures is abrupt. A short linking paragraph connecting process understanding to governance implications would improve the logical flow of the manuscript.

Response to Comment 11:

In the revised manuscript, we will add a short bridging paragraph between Sections 3 and 4. This paragraph will explain how process understanding informs risk-reduction strategy. Specifically, it will link storm forcing, antecedent catchment state, hydrological connectivity, sediment connectivity and cascading pathways to intervention points such as monitoring, early warning, spatial planning, structural protection, NbS and emergency response. This will improve the logical flow from hazard formation to disaster-risk reduction.

Comment 12 :

North African and Mediterranean references

Despite the presence of wadi-related systems in the Boolean query, references from the Moroccan High Atlas, Algerian Tell, and Tunisian Dorsale are largely absent. A more balanced citation base would strengthen the geographic credibility of the review.

Response to Comment 12:

We agree that, although the Boolean query included terms such as “wadi,” the previous manuscript did not sufficiently incorporate evidence from North African and Mediterranean mountain systems.

In the revised manuscript, we will re-screen the corpus and strengthen the regional synthesis with available evidence from the Moroccan High Atlas, Maghrebian semi-arid basins, the Algerian Tell, the Tunisian Dorsale and other Mediterranean or wadi-related systems. We will pay particular attention to ephemeral channels, short-duration convective storms, high rainfall intermittency, sparse gauging networks and high sediment availability. Where evidence remains limited, we will explicitly state this rather than presenting the corpus as geographically balanced.

Comment 13 :

Future research priorities

The conclusion summarises findings effectively but does not include a structured discussion of future research directions. A short subsection identifying priorities — high-resolution monitoring in data-scarce regions, coupled hydro-sedimentary modelling, uncertainty quantification in ML-based systems, and integration of AI with physically based models — would add significant value for the research community.

In relation to the NHESS review criteria, I consider the manuscript to address a relevant topic within the scope of the journal and to have clear scientific potential. However, its scientific quality is currently limited by insufficient integration between the bibliometric and qualitative components, incomplete methodological transparency, and an underdeveloped treatment of geographic representativeness, sediment connectivity, and cascading hazards. The presentation is generally clear, but some figures and sections require clarification and restructuring.

Response to Comment 13:

Future research priorities

We thank the reviewer for this constructive suggestion. We agree that the conclusion would benefit from a more structured discussion of future research priorities.

In the revised manuscript, we will add a short subsection on future research directions. This subsection will identify priorities including high-resolution monitoring and event documentation in data-scarce mountain regions; coupled hydro-sedimentary modelling to link rainfall-runoff generation, sediment connectivity and debris-flow transitions; uncertainty quantification in rainfall thresholds, remote-sensing inputs, DTM-based synthesis and machine-learning early-warning systems; integration of physically based models with AI; and improved evaluation of grey–green mitigation portfolios, including costs, maintenance, time lag and long-term effectiveness.

Recommendation

major revision

This manuscript is scientifically ambitious and addresses a topic of clear relevance for the NHESS community. The bibliometric foundation is solid, the governance framework is well structured, and the interdisciplinary scope is commendable. However, several structural and conceptual weaknesses require substantive revision before the manuscript can be considered for publication. These include the incomplete integration of bibliometric findings into the qualitative synthesis, the underdevelopment of key conceptual frameworks, the insufficient treatment of sediment connectivity and cascading hazard interactions, and the geographic imbalance that currently undermines the claim of global comparability. Addressing these issues carefully would substantially strengthen a manuscript that has clear potential for significant impact in the field.

Overall response to the recommendation

We sincerely thank the reviewer for recognising the scientific ambition, interdisciplinary scope, bibliometric foundation, and relevance of the manuscript to the NHESS community. We also appreciate the reviewer's clear identification of the structural and conceptual issues that need to be addressed before the manuscript can be considered for publication.

We agree that a major revision is required. In the revised manuscript, we will give particular attention to the integration between bibliometric findings and the qualitative synthesis, the methodological transparency of the Dynamic Topic Modelling framework, the conceptual development of sediment connectivity and cascading hazard interactions, and the geographic imbalance that affects the interpretation of global comparability. We will also revise the relevant figures and sections to improve clarity, structure, and analytical value.

We are grateful for the reviewer's constructive guidance, which provides a clear direction for improving the manuscript.