Point-by-Point Response to Review Comments

Manuscript Title: Increased Nonstationarity of Stormflow Threshold Behaviors in a Forested Watershed Due to Abrupt Earthquake Disturbance

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(**C** and **R** denote Comment and Reply, respectively)

Response to Editor (Genevieve Ali) Comments:

C: Two reviewers, who had seen the original version of your manuscript, have now had the opportunity to look at your revised manuscript as well. While they both acknowledge that the revised version is an improvement over the original version, some presentation issues remain and some interpretations still need to be clarified or expanded upon. I am therefore returning your manuscript for further revision. Should you be able to address the reviewer comments and submit a newly revised manuscript, please note that this newly revised manuscript will be sent out for another round of review.

R: We are very grateful for valuable suggstions from the editor and the reviewers for their positive assessments of our work. At present, we have completely addressed the comments point by point from two Reviewers as below. We also hope that this study with the interconnection of hydrological sciences and flash flood disasters could be considered for publication in the "*Hydrology and Earth System Sciences*".

Response to Reviewer #1 Comments:

C1: Summery: The general summary of the manuscript remains the same from the first version: Increased Nonstationarity of Stormflow Threshold Behaviors in a Forested Watershed Due to Abrupt Earthquake Disturbance assessed changes in hydrologic response of a forested experimental watershed in the eastern Tibet Plateau following an earthquake. The authors characterized longer-term changes in threshold behavior in the watershed and introduced a new metric to quantitatively express thresholds for watersheds with areas of disparate land use, ecology, and physiography. The authors found that lower threshold values were observed in disturbed landslide regions and that non-stationarity in thresholds was mainly controlled by changes to the dominant runoff generation mechanisms of subsurface stormflow and the variable source area.

Significance: This work is significant in several ways:

It contributes to our growing understanding of threshold-mediated hydrologic response. It contributes to the further advancement of a unified threshold-based hydrologic theory. It assesses longer-term trends in threshold behavior following an environmental disturbance. It introduced a new metric to quantify and compare thresholds.

R1: We are very grateful for having the summary of your positive assessments and appreciation of our work. The point-by-point comments have been addressed below. We also hope that this study with the interconnection of hydrological sciences and flash flood disasters could be considered for publication in the "*Hydrology and Earth System Sciences*".

C2: General Comments: I appreciate the significant effort taken by the authors to address public comments and comments of the two formal reviewers. Significant improvements were made throughout the manuscript, namely:

- the Abstract is more clear and presents a more accessible pitch to prospective readers
- the clarity of the introduction is also improved and the requisite definitions are now present
- suggestions for future work are clearly articulated
- the overall study findings are presented clearly and reflect the analytic results.

R2: Thanks for your valuable comments and summary. Additionally, We have also addressed all the detailed suggestions and comments you proposed in the revised manuscript below.

C3: Some general comments that could further improve the manuscript: I think that using Tr as the abbreviation for the threshold is somewhat confusing since it is commonly used to describe the time of rise in hyetograph-hydrograph analysis.

R3: Thanks for your serious comment. We agree with you, and the "T_r" **has been revised** as the reasonable abbreviation **"TH_r".** To avoid the confusion triggered by the time of rise in hyetograph-hydrograph analysis, the abbreviation **"TH_r"** has been used in the whole revised manuscript.

C4: Throughout the manuscript, the placement of inline citations is unusual and inconsistent. In some cases, it is unclear if the text is new information being presented by the authors or if the reference indicates similar findings in other studies.

R4: Thanks for your logical comments and kind reminder. The inline citations in the whole revised manuscript have been reasonably arranged. The information being presented bu us or some findings in other studies has been clearly represented in the revised manuscript.

C5: Specific Suggestions: Abstract L17: Please add units to the lower rising threshold value.

R5 Thanks for your kind reminder. This unit of lower rising threshold value is added and revised as 210.48 mm.

C6: Abstract L18: I think that "…a stormflow response faster…" is ambiguous, as it is unclear if the authors are referring to the velocity of the response or the delay between event and response. Please clarify.

R6: Thanks for you pointing it out. It is a faster response rate of stormflow. The sentence of "⋯a stormflow response faster…" **has been revised as** "⋯a faster response rate of stormflow…". (Please see **line 18** in the revised manuscript)

C7: Abstract L23: Clarify what is meant by "turning time".

R7: Thanks for your comments. It is an important turning point along the hydrologic disturbance-recovery timescale following the earthquake. The corresponding sentence "The year 2011 was an important turning point along the hydrologic disturbance-recovery timescale following the earthquake" has been modified. (Please see **lines 23-24** in the revised manuscript)

C8: Introduction L32: Can remove first word "Appropriately".

R8: Thanks for your suggestion. The word "Appropriately" has been removed. (Please see **line 31** in the revised manuscript)

C9: Introduction L72: missing units for $2x10^5$

R9: Thanks for your pointing it out. The original sentence **has been revised as** "the famous Wenchuan earthquake on 12 May 2008 triggered nearly 2.0×10^5 coseismic landslides". (Please see **lines 72-73** in the revised manuscript)

C10: Section 2.2: A more rigorous explanation of how the DASI is obtained would be helpful, especially given its importance in the calculation of the IWA index.

R10: Thanks for your suggestion. The formula with detailed parameters to calculating *DASI* is listed in the revised manuscript, presenting a more clear explanation. The corresponding sentences **have been added and revised below:**

"The *depth equivalent antecedent soil water index* (*DASI*) at the start of each rainfall event was obtained (Zhang et al., 2021a). It is indicative of the initial shallow soil water storage (Wei et al., 2020), and is generally calculated from the eight-layer soil moisture measurements at each soil profile as (Farrick and Branfireun, 2014):

$$DASI = \sum_{i=1}^{n} \theta_i \left(D_i - D_{i-1} \right) \tag{1}$$

where θ_i indicates the average soil content between i and i-1 soil layer, cm^3 cm^{-3}. i =1, 2, 3, 4……n, and n indicates the number of soil layers below the surface for the monitored soil depth of 80 cm. D_i indicates the soil depth at the i^{th} layer (10, 20, 30, 40, 50, 60, 70, and 80 cm, D_0=0). The index is utilized to exploit the effects of antecedent wetness on the magnitude of hydrological thresholds and emergent behavior at the hillslope and watershed scales."

(Please see **lines 134-141** in the revised manuscript)

C11: Section 2.4: The calculation of the index is fairly clear. With that said, there is little explanation about interpreting the index. I think that some details on this would be beneficial.

R11: Thanks for your valuable and logical suggestions. We have clarified the detailed of the index, and the corresponding sentence "The earthquake-induced landslides can destroy the soil-vegetation system, reducing the water storage of shallow soil and vegetation canopy and leading to the change in hydrologic threshold of the sum of *DASI+P* in the distubed land-use type of landslide. The hydrologic threshold in the landslide is different from other undisturbed land-use types in the watershed. Meanwhile, as long-term evolutions and recovery of landslides (Figure 1), the mutual conversions in land-use types further influence the magnitudes in water storage of shallow soil and vegetation canopy in each land-use type, possibly altering the magnitudes in hydrologic threshold of the sum of *DASI+P* at watershed scale. Herein, to clearly understand long-term threshold evolutions and integral hydrologic emergent behaviors variations pre- and post-earthquake at the watershed scale, an *integrated watershed average* (*IWA*) index for the thresholds considering different land-use types was proposed to characterize the watershed stormflow emergent behaviors" **has been illustrated**. (Please see **lines 157-165** in the revised manuscript)

C12: Section 4.1 L249-251: I find this sentence very hard to follow.

R12: Thanks for your serious comments. The sentence has been changed as "The bedrock depression storage on the soil-bedrock interface (Fu et al., 2013b;McDonnell et al., 2021) and antecedent soil moisture storage (Cain et al., 2022;Zhang et al., 2021a;Fu et al., 2013a) are as the main factors controlling the magnitude of the generation threshold (TH_g), influencing the initial emergent behavior of rainfall-runoff.".(Please see **lines 265-268** in the revised manuscript)

C13: Section 4.2 L290: I think you mean severely, not "severally".

R13: Thanks for your reminder. The word "severely" has **been revised as** "severally". Meanwhile, The details in the revised manuscript has been seriously modified.

Response to Reviewer #2 Comments:

C14: The authors describe an interesting system dynamic reconstructing changes in threshold runoff behavior from a cataclysmic seismic disruption of a watershed and subsequent landslide expansion, and a decade long recovery as the forest canopy re-establishes. The paper would provide a significant contribution to the hydrologic science community.

However, as pre-event data, and the ability to access the area to collect additional data in the time following the event is limited, they need to carefully consider what the data they have supports, and what is speculation beyond their empirical evidence and HEC-HMS modeling. The degree of speculation is not necessary as the story they can tell with available data and model results is compelling. There also remain a set of areas that need to be clarified. I provide a set of comments to these ends:

R14: Thanks for your positive assessments and logical comments for our work. According to some comments you provided, the point-by-point comments have been addressed below. We also hope that this study with the interconnection of hydrological sciences and flash flood disasters could be considered for publication in the "*Hydrology and Earth System Sciences*".

C15: Figure 3 is much improved and more understandable as it has been separated into pre-event forest and grassland, and post-event (presumably mixed forest and grassland). Do the gauges used to separate out the pre-event forest and Grassland behavior have sufficient post-event data to investigate how recovery differed between grassland and forest? The catchments for each of these should be described (land cover, area, elevation, steepness) to assess whether differences in behavior are due to vegetation type or coincidental geomorphology, soils, etc. Is the gauge for the post-event threshold analysis one of the gauges for the pre-event analysis or is this now a different gauge? If so, the authors need to demonstrate that the change is due to the event dynamics and recovery (which appears reasonable), and not the difference in watersheds.

R15: Thanks for your good assessments and logical comments. The sufficient post-event data (such as soil physical properties, vegetation canopy, etc.) to investigate how recovery differed between grassland and forest compared to pre-event data is very significant to reasonably identifying long-term evolution of watershed hydrologic processes following the earthquake. However, in our study area, it's almost impossible to compare samples from the same location before and after the earthquake, such as hydrological properties of soil and vegetation. We only compare their spatial difference of hydrologic properties between disturbed landslides and undisturbed forest or grass-shurb to reflect the evolution along the timescale of hydrologic properties pre- and post- earthquake. Just after the earthquake, the sampling at the earthquake-induced landslides is really dangerous for us due to frequent debris flow and flash floods. In the next study, we will design resonable plan to sample in time and space and persue the disturbance-recovery process of different land-use types in an area prone to earthquakes, and to more scientific understand the evolution of hydrological disasters after the earthquake.

In our revised manuscript, we really just consider the hydrologic properties between disturbed landslides and undisturbed forest or grass-shurb at a same watershed, keeping other conditions equal, such as area, elevation, and steepness. We demonstated that the change in hydological behavior pre- and post-earthquake really is due to the event dynamics and recovery based on same gauge for the threshold analysis. Follong your good suggestions, in the next time, we can further exploit the effects of tectonic, topography, landform, vegetations, and climate on hydrological proceeses on flash floods at multiple watersheds.

C16: Eq.1 appears to be misinterpreted as directly providing the threshold. The RHS shows the storage and precip term which can be used as the independent variables to determine the threshold values but the RHS does not equal the threshold. The use of this equation needs to be clarified.

R16: Thanks for your logical comment. We have calrify the hydrologic threshold of storage + precipitation at each land-use type, and use integrated watershed average (*IWA*) index for the thresholds considering different land-use types to exploit the nonstationarity in pre- and post-earthquake threshold behaviors.

The corresponding sentence "The catchment threshold behaviors between Q_q and the variable of the sum of *DASI* + event precipitation amounts (*P*) at each land-use type were quantitatively assessed using *piecewise regression analysis* (*PRA*), and the hydrologic threshold values of the sum of *DASI*+*P* and slope parameters from each linear segment of the *PRA* function were calculated (Zhang et al., 2021a;Oswald et al., 2011)" has been revised and added. (Please see **lines 148-151** in the revised manuscript)

C17: In section 2.3, provide the time period for the empirical data collection.

R17: Thanks for your comment. The time period for the empirical data collection is provided in section 2.3. The corresponding sentence "A total of 47 events in this experimental watershed were identified during a time period of June ~ August of every year from 2018 to 2020, ……" has been illustrated. (Please see **lines 144-145** in the revised manuscript)

C18: In figure 4, define the flood volume. I presume this is limited to the storm flow volume following hydrography separation, but this should be stated. The longer recession limb and base flow would also be of interest as recharge may have changed with lower canopy ET.

R18: Thanks for your logical comments. Actually, the flood volume is total flood flow in a single event, which is illustrated in the revised manuscript. The corresponding sentence "……including peak discharge (Q_p) and event flood volume (*V*, total flood flow in a single event)" has been revised (Please see **lines 237-238** in the revised manuscript). In the next time, we will pay more attention to the long recession limb and base flow you suggest, and well exploit the characteristics of flow recharge related to canopy ET based on our collected data.

C19: Define "confluence." Does this indicate routing time or time of concentration?

R19: Thanks for your pointing it out. The word "confluence" is the hydrological process of flow concentration. In the revised manuscript, the vague sentence has been changed as "However, we also acknowledge a limitation that only the dominant hydrological process of runoff generation was considered while the important hydrological process of flow concentration was mostly ignored. In a future study, such metrics will be involved in two hydrological processes of runoff generation and flow concentration to more efficiently reflect the watershed's hydrologic behavior.". (Please see **lines 254-257** in the revised manuscript).

C20: Line 280-285 discusses filling of bedrock depressions and expansion of variable source areas. It appears these are assumed rather than demonstrated for this paper. This is reasonable, but as it is not clear these processes are directly supported by data this section can be significantly shortened. This is some of the speculation beyond the data I refer to . By the way, its fine to clearly pose this in your conceptual diagram, but be careful of implying these were observed. To this point we don't know what the soil/bedrock interface is like in this watershed, and whether it would support this mechanism, especially if the bedrock is highly fractured.

R20: Thanks for your logical comments and good suggestions. I agree with you, and have shorten this section for discussion about filling of bedrock depressions and expansion of variable source areas. The corresponding sentences "Below and above the generation and rising thresholds, the changes stormflow discharge (Q_q, with mean values of 3.14 mm, 22.5 mm, and 138.3 mm, respectively) are significant (Dickinson and Whiteley, 1970; Zhang et al., 2021a). At the TH_g value, the bedrock depressions on the hillslope could be filled with water from rapid rainfall infiltration while water spilled over the undulating soil-bedrock interface (Figure 6a-b) and generated higher streamflow (Figure 6c). Once above the TH_r value, the variable source areas (*VSA*) close to the channel or impermeable surface under high rainfall intensity showed a significant expansion (Figure 6d).” has been revised (Please see **lines 271-276** in the revised manuscript).

Some investigation for profile data at hillslopes in our study area could illustrate the process of subsurface stormflow on the soil/bedrock interface with highly permeable soils and low permeability of bedrock, but, in the next study, we also need to collect more field data and samples of the soil/bedrock interface and bedrock properties from our study area to better support this machnism. Meanwhile, we will apply the indoor potential scaled model test or runoff plots with bedrock depression to quantificationally exploit the effects of bedrock depression at hillslopes with expansion of variable source areas on runoff generations in different scenarios.

C21: Minimum contributing area (MCA) is first mentioned on line 285. No prior mention or context for this appears to have been given, although it may have been discussed in a prior paper. Either provide the context here, or remove.

R21: Thanks for your logical suggestion. The corresponding contexts about minimum contributing area (*MCA*) have been removed in the revised manuscript.

C22: Line 317 - SEVERELY is misspelled.

R22: Thanks for your comment. The word has been corrected as “severelly” in the revised manuscript.

C23: Clarify where Hortonian overland flow is expected. I presume it is limited to the exposed bedrock from landslide scars given the very high soil conductivities.

R23: Thanks for your logical comment. Yes, you are right, and the Hortonian overland flow is mainly stimulated by the exposed bedrock in the trailing edge of the landslides. In the lower part of the landslides, the generated loose deposition is with very high soil conductivities and generally increased subsurface stormflow with the microporous flow, where is different from the Hortonian overland flow. The corresponding sentence "After the abrupt disturbance, the exposed bedrock in the trailing edge of the landslides easily induced the Hortonian overland flow, and the generated loose deposition with high soil conductivities in the lower part of the landslides generally increased subsurface stormflow with the microporous flow" is illustrated. (Please see **lines 77-79** in the revised manuscript)

The corresponding sentence "the Wenchuan earthquake-induced ~ 2.0×10^5 landslides decreased by ~30% the forest coverage (Cui et al., 2012), altering the infiltration runoff processes and the contribution of Hortonian overland flow from the exposed bedrock in the trailing edge of the landslide and subsurface stormflow from landslide-generated loose deposition to flood hydrograph in the channel" is illustrated. (Please see **lines 228-291** in the revised manuscript)