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General Comment:

In their submitted manuscript, Allen et al. propose a hazard assessment around two existing and one potentially forming proglacial lake in the Puiqu River basin, Himalaya. They document the glacial environment as well as its projected development under future climate conditions, potential outburst flood triggers and modeled discharges in vulnerable downstream communities. The study gives insights into different aspects of glacial lake outburst floods highlighting particularly interesting features of the investigated cases. However, it is difficult to grasp how relevant the scientific insights from this investigation are given that the authors' judgement is too often limited to qualitative assessment. For a scientific journal submission, I was expecting more substance in view of reproducibility and representativeness of the results

We thank the colleague for his critical feedback on the manuscript and apologise for the time taken to response. We designed this study to address what we see as a gap in common GLOF assessment scales (particularly in the developing world), fitting between large scale first-order approaches and comprehensive scenario based hazard mapping. We thereby aim to provide an illustrated case-study showing how potential new lakes can begin to be considered in DRR planning. Is a given lake really going to present an unprecedented threat to downstream areas as commonly implied (relative to existing lakes)? Does such a potential lake need to be considered in the design of response strategies such as EWS? Such questions are not answered by existing studies, which to our knowledge, have not gone beyond first-order assessment of future lakes.

The methodological approach was designed to be as simple as possible, with a view that methods could be attractive for upscaling to any number of potential future lakes. However, in view of the comments from all 3 reviewers, we accept that this was scientifically weak, as many assumptions were not well substantiated, and importantly, the susceptibility assessment and triggering processes were not directly linked to the outburst scenarios. Therefore, in the revised manuscript we will start with a more comprehensive assessment of lake susceptibility, to derive worst-case scenarios of ice-rock avalanche triggered GLOFs from all 3 lakes, including a new scenario for the artificially lowered lake volume of Jialongco. The use of r.avaflow for the modelling of the entire process chain will reduce the amount of qualitative assumptions regarding the link between the trigger event and outburst wave. We'd also note that we've also since been able to measure bathymetry for the two current lakes, which is important input for r.avaflow.

We must however acknowledge that many aspects of the lake susceptibility assessment will remain partially qualitative, as for some parameters like permafrost conditions, we simply lack high resolution modelling results for this area. Likewise we would note that there are few, if any, examples of quantitative likelihoods being applied in the hazard assessment of moraine dammed outburst floods, and related studies have typically applied qualitative high, medium and low likelihoods to large, medium and small magnitude events respectively. To better reflect the scope of our study, and acknowledge that the assessment of GLOF likelihood in our study is rather limited to a large worst-case scenario (and therefore can't be considered a full hazard assessment), we also propose to modify the title to *"Glacial lake outburst flood magnitudes under current and future conditions: implications for disaster risk reduction in a transboundary Himalayan basin".*

Major Comments:

My main point of criticism is that the reader of this manuscript is left with little information on how to assess validity or accuracy of the findings. In its current state, the study appears more like a presentation of important facts and qualitative judgements, which are typical for technical reports. For a scientific paper I would have expected some critical assessment of the flood risk, e.g., a benchmarking of the presented methods against previous occurrences of outburst floods. The authors cite accounts of previous outbursts in the area (Line 106). Could they be used for this?

Unfortunately there are very scarce details on the past GLOF events occurring in the upper basin (affecting village of Nyalam), beyond the fact that they occurred. The 2015 earthquake resulted in a lot of erosion around the village, so geomorphological evidence is also not clearly recognisable. However, further downstream towards the border with Nepal, an outburst from 2016 has been well documented (e.g. Cook et al. 2018) and we will attempt to compare our worst-case scenarios against this event. Likewise, we will also compare our results against those obtained in other model-based studies within the same basin (e.g., Shrestha et al. 2010, Zhang et al. 2021)

Cook, K. L., Andermann, C., Gimbert, F., Adhikari, B. R. and Hovius, N.: Glacial lake outburst floods as drivers of fluvial erosion in the Himalaya., Science, 362(6410), 53–57, doi:10.1126/science.aat4981, 2018.

Shrestha AB, Eriksson M, Mool P, Ghimire P, Mishra B, Khanal NR. 2010. Glacial lake outburst flood risk assessment of Sun Koshi basin, Nepal. Geomatics, Natural Hazards and Risk. Taylor & Francis 1(2): 157–169. DOI: 10.1080/19475701003668968.

Zhang, T.; Wang, W.; Gao, T.; An, B. Simulation and Assessment of Future Glacial Lake Outburst Floods in the Poiqu River Basin, Central Himalayas. Water 2021, 13, 1376. https://doi.org/10.3390/w13101376

The first part of the paper presents some motivation on why to study the chosen three lake basins. However, it is not possible to verify that this corresponds to a worst-case analysis. In this case, it would be necessary to show that no lake basins could produce more serious outburst floods. As the authors argue, this depends on moraine dam geometry, water volume and trigger potential. Under these aspects it cannot be argued that the presented set of lakes is representative for worst case scenarios.

Thanks for this comment. We struggled on this aspect as we did not want to reproduce results from our earlier study (Allen et al. 2019), that was primarily the basis for focussing on the two current lakes. In view of this, and comments from the other two reviewers, we will now add a physical description of the 3 lakes to the study area section, including key physical parameters and details on assessed hazard/danger levels provided in previous studies (for Jialongco and Galongco). This will also help better justify the focus on these lakes. We will add images of the lakes to Figure 1. Likewise, we'll add further description on why this particular potential future lake was identified as a priority for worst-case analyses in the study (large size, proximity to village, evidence of supraglacial lakes already forming etc).

At too many parts of the manuscript, the authors' qualitative judgement is presented as a scientific result. In particular, in the Section "GLOF likelihood", various factors influencing or triggering outburst floods are presented, but I could hardly find any objective arguments. It seems that the only one is the estimate of a dam-overtopping wave volume, which can be 10 times as high as the "incoming mass". Here and elsewhere in the manuscript, it has to be made clear that the conclusions are based on solid scientific grounds. Otherwise, a "low probability" could indicate one catastrophic event every 5 years as opposed to several ones per year. This is not what the authors imply. In a similar sense, it is not clear what the demanded "comprehensive and forward-looking approach to disaster risk reduction" is. To me, such an approach should always be taken and I see little connection to the present study or any finding, which made the suggested strategy particularly pertinent to the Poiqu River basin compared to any other place in the world.

Effort will be made throughout the manuscript to provide a more quantitative basis for the assessment findings. The section on GLOF likelihood (renamed to "lake susceptibility") will be redrafted, with more focus on the local geological and glaciological conditions that determine large ice/rock avalanche potential. Table 3 will be revised, and where possible, quantitative details will be provided with references cited. For example, the slope threshold-based avalanche calculations will be removed, and replaced with a more detailed assessment of local geological and glacial conditions. Again, we must acknowledge that many aspects of the lake susceptibility assessment will remain partially qualitative and subjective for those factors that lack quantitative data or can be inferred only. However, we will provide references in Table 3, that support the expert judgements being made.

On the final point, we are yet to see GLOF mitigation strategies anywhere in the developing world that can be considered forward-looking. There has now been several years' worth of first-order modelling studies showing that glacial lake area and number will increase in the future, yet in our interactions with authorities across several countries, we see no indication yet that these future threats are being considered in DRR planning. We fully agree that DRR planning ultimately requires more quantitative assessment of outburst probabilities etc. However, this remains the holy grail for the assessment of existing moraine dammed lakes, let alone future lakes for which conditions of both the lake and surroundings are highly uncertain. Particularly for DRR strategies such as an EWS, we would therefore argue that there is merit in our approach of simulating single worst-case scenarios, as these can be a first basis for planning response strategies that remain robust under an uncertain future. For example, by planning community awareness programmes around a worst-case of 70 minutes vs. 130 minute warning time, or ensuring critical infrastructure is located well away from a future GLOF path.

Specific Comments:

I suggest including a cartoon explaining different lake formation scenarios and specifically the hydrological base line. To be honest, I had to stare for some time at Figure 3 of Benn et al. (2012) to understand this concept. On the other hand, I never grasped the meaning of the "the lowest point where the glacier surface intersects the terminal moraine" (it seems that by definition, the glacier and the terminal moraine should not intersect). Similarly, when the future evolution of the lakes is described, longitudinal profiles would be extremely helpful. This would help the reader to understand Figures 7 and 8 and appreciate the shown information.

We thank the reviewer for their suggestions about how to improve the description of the concepts we employ to examine the likelihood of future lake formation on glacier RGI60-15.09475. To ensure that the approach we have followed is as clear as possible, we will improve the description of the lake formation scenarios in section 3.3 (Future lake development) and clearly refer to Figures 3 and 19 in Benn et al. (2012), as these two figures already provide a good visual representation of how supraglacial meltwater ponding can lead to glacial lake formation. We will be clear to distinguish between the initial stages of supraglacial meltwater ponding and lake formation (when the terminus of the glacier is still in contact with the terminal moraine) and the later stages of lake expansion (when, as the reviewer suggests, the terminus of the glacier would not be in contact with the terminal moraine).

We also thank the reviewer for their suggested improvements to Figures 7 and 8. Figure 7 already incorporates longitudinal profiles of glacier surface elevation, surface slope and surface elevation change, but we agree that Figure 8 would benefit from the addition of centreline profiles of glacier surface elevation under different scenarios of thinning in 2050, 2075 and 2100.

The flood model is a key ingredient to this investigation. Although I agree that too many technical and mathematical details are not appropriate for this study, I was wondering what the main parameters and boundary conditions of this model are. Apparently, the flood volume, some time scale of drainage initiation and dam geometry play a role and it would be interesting to hear how these parameters drive the model.

In view of the comments from all 3 reviewers, we accept that the previously used modelling approach was scientifically weak, as the triggering processes were not linked to the outburst scenario, leading to reliance on assumptions (e.g., concerning Potential Flood Volume) and empirical relationships to define parameters. Therefore, in the revised manuscript we will use r.avaflow to simulate worst-case ice-rock avalanche triggered GLOF process chains from all 3 lakes, including a new scenario for the artificially lowered lake volume of Jialongco. This has the advantage of the outburst process (and related parameters) being dynamically simulated based on the ice-rock avalanche triggering event and overtopping wave. Key model parameters (including the internal and basal friction angles of the solid material, the fluid friction number, and the coefficient of erosion) will be empirically defined, with references added.

Lines 45-47: "… most scientific attention has focused upon …" I do not agree with this statement. In the jökulhlaup literature, ice-marginal and subglacial lake drainages have also

received a lot of attention. Whereas I cannot say which scenario has been most prominent, I would refrain from an absolute statement on scientific attention.
Good point – we will qualify this statement as *"In Asia, most scientific attention has focused upon…."*

Lines 175-176: "B_w and h_b are fully obtained" measured?
Under the revised modelling approach with r.avaflow, these values will be dynamically simulated.

Line 178: Reference for HEC-RAS is needed.
Section to be revised, based on modelling approach with r.avaflow.

Line 185: Reference for DEM's is needed.
Will be added.

Line 189: Reference for Manning roughness value is needed.
Manning's n will not be required for the updated modelling with r.avaflow. Key model parameters (including the internal and basal friction angles of the solid material, the fluid friction number, and the coefficient of erosion) will be empirically defined, with references provided.

3.2 Lake susceptibility assessment: Presenting the likelihood calculations seems appropriate here.
Table 3 will be extensively revised and to the extent possible, quantitative assessment of the various GLOF susceptibility factors will be provided. We note, as in the general comments, that some qualitative expert judgment is inevitable in this field, as outlined in International Guidelines to Glacier Hazard Assessment (https://www.gaphaz.org/files/Assessment_Glacier_Permafrost_Hazards_Mountain_Regions.pdf).

Line 231: "considering the factors outlined by …" these factors should be specified.
We will amend the text here to reiterate that the surface slope and velocity of the glacier are both suitably low to allow for substantial meltwater ponding, as it already stated on lines 220 and 221.

Line 257: I suggest a paragraph break here.
Noted.

Line 457: "recent removal of much of the frontal moraine …" this needs a reference.
I reference to Figure 9 will be made here.

Lines 466: What are "capacity building programs"?
Will be revised to "programs to strengthen local response capacities"

Figures:

Figure 1: The lakes at Cirenmaco and Jialongco are difficult to discern.
Noted – we will improve the colours and symbology

Figure 2: The font sizes are a bit small, but h_b defined in Panel A seems to disagree with Panel B.
Noted – figure will be replaced to reflect new modelling approach.

Figure 4A: Where is the lake? The blue outline or the light blue polygon? A different color scale for maximum flow height would help.
Noted – we will improve the colours and symbology

Figure 5: It is difficult to tell where the lakes are. I do not see any blue patches.
Noted – will try to improve contrast of the colours in the images.

Figure 6: The two images in Panels C need some annotation. What does the reader see in these images? Why is one so dark and the other one bright? What happened between the two?

Caption will be expanded for panel C. The significant expansion of infrastructure seen between 2015 and 2017 should be obvious from these panels, despite the differences in colouring from the two satellite images.

Figure 8: This is the future lake site, right? Where will the lake form? The colored outlines make little sense and are hard to sea. Which direction does the ice flow?
Red line is the maximum projected overdeepening, and the blue/purple colourings are extent to which the lake is projected to emerge under the different future scenarios. We will improve colouring and information in the caption. Direction of ice flow will be added, thanks!

Figure 9: Some arrows and annotation as well as scale bars are needed.
Noted – will be added.