Reviewer 1

Major Comments:

Allen et al. used a suite of well-established tools to assess possible trigger mechanisms of GLOFs, and to model hydro-mechanical characteristics and runouts of the flood waves. Unfortunately, the authors missed the opportunity to couple such triggers with the subsequent failure of moraine dams. Recent software such as r.avaflow have recently improved the understanding of GLOF processes, allowing for pseudo-probabilistic assessments of GLOF triggers and impacts under a range of adjustable and testable boundary conditions. However, given that the authors considered only three worst-case scenarios of moraine-dam failure, largely decoupled from the initial triggers, I see only limited progress in calculating the likelihood of observing a GLOF of a given magnitude, though this is repeatedly stated in the manuscript. Clearly, the authors present a thorough expert-based assessment of potential conditioning or triggering factors. Yet it remains unclear how this traditional, rather subjective procedure brings GLOF hazard and risk assessment forward with regards to the many previous case studies that we saw in the past years. Thus, the authors' goal to estimate "the core hazard dimensions of GLOF magnitude and likelihood (or probability)" (L245-246) only remain partly fulfilled in my opinion. If this manuscript is intended to improve early warning and risk management, then the reader (at best a person involved in such tasks) might expect a more objective use of the terms hazard or probability, that is a numeric value between zero and one. A probability or return period might provide a robust baseline for decision makers compared to the current distinction between a 'high' or 'low' level of susceptibility or impact. Fortunately, most of the data (including high-resolution DEMs and satellite images) for such an extended appraisal already come with this manuscript. I thus would like to encourage the authors to revise their manuscript accordingly, considering a much wider range of scenarios in order to proceed towards a more objective assessment of current and future GLOF hazard in their study region.

We thank the reviewer for this critical feedback on the manuscript, and apologise for the time taken to respond. The comment from the reviewer gets right to the fundamental contribution our study is trying to make, and unfortunately this has perhaps not been adequately introduced or fulfilled. We are aware of the value of r.avaflow and other approaches for undertaking detailed, scenario-based hazard assessment, and are using exactly these models in other studies, where comprehensive hazard mapping is the desired outcome. However, what we are aiming to achieve in this particular paper is to address a gap in approaches/scales, fitting between large scale first-order approaches and scenario based hazard mapping, 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 (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 (e.g. GIS-based approaches).

On a practical level, we see this gap as an important niche to fill. 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. While it would be academically possible to generate detailed scenario-based hazard maps for a potential future lake, we consider a more practical intermediate step is needed to first demonstrate that a given future lake warrants such attention and further monitoring.

We agree that ultimately, DRR planning can be best informed by more quantitative assessment of outburst probabilities. However, this remains the holy grail for the assessment of existing 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 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.

The decision to use HEC-RAS for modelling downstream flood impacts was initially driven by a desire to keep the approach 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 the triggering processes were not linked to the outburst scenario, leading to reliance on assumptions (e.g., concerning PFV) and empirical relationships. Therefore, in the revised manuscript we will use r.avaflow to simulate worst-case ice-rock avalanche triggered GLOFs process chains from all 3 lakes, including a new scenario for the artificially lowered lake volume of Jialongco. The selection of worst-case avalanche volumes will be comprehensively described, and based on a more nuanced assessment of triggering factors given in Table 3, particularly relating to glacial and geological conditions.

In conclusion, we would summarise the novelty and contribution of the revised manuscript as being **a first study to demonstrate the effect of future lake development on downstream flood magnitudes, with implications for DRR planning.** In addition, we will now be able to **demonstrate the effect of lake lowering on downstream flood magnitudes**.

Given the valid concerns of the reviewer, we also propose to modify the title to more accurately capture the scope and rationale of the paper:

"Glacial lake outburst flood magnitudes under current and future conditions: implications for disaster risk reduction in a transboundary Himalayan basin"

Detailed comments:

-L6: 'far reaching': in terms of run-out? Social consequences? Media coverage?
Revised to *"where societal impacts can occur far downstream"*

-L10: 'well-known dangerous': please consider avoiding subjective terms.
Revised to *"previously identified dangerous lakes"*. This is in line with the numerous studies cited in the text that assess these lakes as being dangerous.

- L26-27: 'Based on … capacity building programs': Hardly mentioned in the text, consider revising or deleting.
Revised to *"education and awareness raising programs"*. We feel it's an important point to make, even if not a central component of this study, because authorities (and funders) often overlook such important social dimensions of DRR.

- L39: 'rapidly in both size and number': how much? Consider using recently multi-temporal glacier lake inventories[14,15].
The more recent studies have been cited, and we report, by way of example, the increase of 27 km^2 in glacial lake area for the Central Himalaya reported over the past decade (Chen et al. 2021).

- L40: What is the frequency of GLOFs in the Himalaya? If I am not mistaken, most researchers deem GLOFs a low rate of occurrence.
We now report the GLOF frequency of 1.3 per year for the Himalaya since the 1980s (after Vey et al 2019).

- L47f: 'Such lakes can have volumes >100 m^3', according to ref. [16]
Sentence revised to *"volumes exceeding 100m^3"* and the new reference added.

- L69: 'within the eastern Himalayan region': why there?
Have removed this part of the sentence – it was stating the obvious as the largest border area between Nepal and China is in the eastern Himalayan region.

- L75: 'are lacking': suggest to tone this a bit down, as it undermines previous appraisals

We don't intend to undermine the previous studies which have considered future lakes – many of which we were involved in or led – but these are studies aiming to identify district or regional-scale changes in GLOF risk. Other than a general indication of how hazard or risk could change, they don't provide information for specific lakes, that is required for DRR planning.

In view of the concern of the reviewer, wording of this sentence will be revised to *"yet practical examples on how to account for future lake development in local GLOF risk assessment and risk management have been rarely demonstrated"*.

- L79-82: Is this statement valid for all glaciated high mountains? Or only the Himalayas?
To our knowledge the statement is valid globally. We know of no cases where local authorities have begun to consider future lakes in the their hazard and risk assessment. Hence, we leave as a general, globally applicable statement.

- L89-91: The last sentence could be deleted, if shortening is needed. The introduction ends well with a strong presentation of the research goals.
Agree, sentence removed.

- L93: 'considering potential GLOF impacts'
Agree, wording revised

- L97: 'mean annual air temperature'
Wording revised

- L99: are these precipitation rates (I think these are monthly?) the maximum or the average recorded in a given period?
Thanks for pointing this out. There are average monthly rainfall totals (not rates). Text will be corrected.

- L105: 'GLOF danger': replace with 'GLOF hazard'?
Disagree – "danger" was the term deliberately used in Allen et al. 2019 as the study went beyond hazard to consider also exposure of infrastructure (but fell short of a full risk study).

- L107: 'losses of up to US$4 million': is this in today's currency?
These were values of 2015 – we will clarify this in the text.

- L111-112: 'a large and persistent threat, considered as one of the most dangerous lakes in Tibet': suggest to tone this down again. Given that previous appraisals used different criteria and thresholds, we may wish to avoid confusion what is now the 'most dangerous lake' in a given region.
The point we want to make here is that irrespective of different methods and criteria used, multiple studies point to his lake as being one of the most dangerous/hazardous in the region. We will add a couple of further papers to support this. Also we've removed the term "large" as this is subjective. Text revised to *"identified by multiple studies as being one of the most critical lakes in Tibet"*

- Figure 1: Please highlight the border between Nepal and China.
Border will be added.

- L139: So estimating lake depth comes with zero uncertainty?
Over the past 2 field seasons we have been able to measure bathymetry of both Jialongco and Galongco lakes. We will now compare the measured volumes to the estimated volumes from several empirical relationships to come up with a range of uncertainty. For GLOF modeling, the measured volumes will now be used for Jialongco and Galongco, while for the future lake, upper and lower volumes will be simulated, based on the uncertainty range established above.

- L150: To my knowledge GlabTop models the glacier thickness in a bedrock topography. Did the authors add the moraine on top of this bedrock depression? How high / wide is this moraine? If not, how susceptible is a bedrock depression to overtopping? Did the authors consider sediment input from supraglacial debris into the lake?

Reviewer is correct in their interpretation. In keeping with a philosophy of a worst-case simulation from the future lake, we do not consider additional height (and freeboard) from a moraine dam, the height of which can only be guessed. We take the bedrock topography as simulated from Glabtop. Likewise sediment deposition into the lake, thereby reducing the potential lake volume is not considered. These aspects/limitations will be noted in the methodology.

- L155: Please consistently use Fig. / Fig / Figure (or any other option) throughout the manuscript. Sometimes the abbreviation is written with a dot, sometimes not.

Corrected.

- L154-164: the PFV approach may underestimate the amount of water that is generated from mass flows entering the lake and causing a splash wave. Given that the authors consider ice avalanches and landslides entering the lakes, how useful is it to use the PFV approach?

The revised modelling approach using r.avaflow will generate the flood volume based on the actual simulated avalanche scenario into the lake.

- L156: The PFV concept further seems to confuse mean and maximum depth. Why should the drain empty completely if the depth of the breach is equal to the mean depth of the lake? Again, the assumption of a fixed breach depth might simplify (or even underestimate) the worst-case scenario of GLOF volumes.

This was a typo, and should have referred to maximum depth. In any case, the PFV approach will be replaced, and flood volumes will be dynamically generated with r.avaflow.

- L162-163: 'In comparison, … (Xu, 1988)': what is this comparison good for?

The comparison was provided to indicate that the estimated breach depth of 40 m is not completely unrealistic. This paragraph will be revised now based on the new breach depth simulated using r.avaflow.

- L165: 'the DEM accuracy is unknown': what is the accuracy of the 1-m DEM then?

No other high-resolution DEMs of Poiqu are publicly available to assess the vertical and horizontal accuracy in the Pleiades DEM. However, Berthier et al. (2014) computed vertical accuracy of Pleiades DEMs over the Agua Negra study site and reported mean vertical biases ranging from 0.99 to 1.33 m without GCPs. Similar accuracy level (0.3 m) was also reported by Zhou et al. (2015) from the comparison of a Pleiades-1 DEM with an airborne LiDAR DEM. Similarly, without ground control points (GCPs), the horizontal location accuracy of the images was estimated as 8.5 m (CE90, Circular Error at a confidence level of 90 %) for Pléiades-1A and 4.5 m for Pléiades-1B (Lebègue et al., 2013; Oh and Lee, 2014).

References

Berthier, E., Vincent, C., Magnusson, E. et al. (2014). Glacier topography and elevation changes derived from Pleiades sub-meter stereo images. The Cryosphere, 8, 2275-2291.

Zhou, Y., Parsons, B., Elliott, J.R., Barisin, I and Walker, R.T. (2015), Assessing the ability of Pleiades stereo imagery to determine height changes in earthquakes: A case study for the El Mayor-Cucapah epicentral area. Journal of Geophysical Research- Solid Earth, 120, 8793–8808.

Oh, J. and Lee. C. (2014), Automated bias-compensation of rational polynomial coefficients of high-resolution satellite imagery based on topographic maps. ISPRS Journal of Photogrammetry and Remote Sens., 100, 12–22.

Lebègue, L., Greslou, D., Blanchet, G., De Lussy, F., Fourest, S., Martin, V., Latry, C., Kubik, P., Delvit, J.-M., Dechoz, C., and Amberg, V. (2013). PLEIADES satellites image quality commissioning, Proc. SPIE 8866, Earth Observing Systems XVIII, 88660Z (23 September 2013), doi:10.1117/12.2023288, 2013.

- L171-172: Again no uncertainties in these equations? Propagating estimates through Equations 1-4 might have already generated a substantial amount error that remains not completely untouched in the remainder of the manuscript. Also, why did the authors not choose a physical dam break model such as BASEMENT?

The Froehlich equations will not be used in the revised manuscript. The simulation of the entire process chain will be undertaken using r.avaflow. Testing of different parameters in the simulations, and related uncertainties, will be reported in supplementary material.

- L180: What is the mechanism that causes overtopping? A splash wave? Or overflow by a (gradually) growing lake volume?

In the revised manuscript, the overtopping mechanism will be simulated using r.avaflow. Given that ice-rock avalanching is identified as the most feasible GLOF triggering mechanism for all 3 lakes, we expect a splash wave to be the primary mechanism. Gradual collapse of a lateral moraine wall could feasibly lead to gradual overflow in the case of Jialongco.

- L189: 'Uniformity of land cover and lack of vegetation': strong statements - please show this. Source for the chosen value of Manning's n?

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 added.

- L201-2014: I found it somehow confusing to see first the paragraph on flood modelling, followed by a paragraph that describes potential causes for this flood. Again, it may sound a bit harsh, but considering these triggers decoupled from the flood routing models seems to represent not the state of the art in GLOF modelling. Determining the likelihood (again without a probability) for a rock or ice avalanche from a fixed angle of reach might be suitable for a large-scale assessment of GLOF susceptibility that pursue a rapid screening for sites that need more attention. Here, the authors have identified such sites, so there is (from my perspective) no need to rely still on the more traditional side of GLOF susceptibility appraisals. In other words, if there is a potential for rock to detach, what could be its size? How rapidly may this rock avalanche enter a lake? What would the displacement wave look like? It also remains unclear why the authors had chosen exactly those susceptibility factors in Table 3 (or 2). Suggest to add references to this table that showed if these susceptibility factors in deed had positive (+) relevance for triggering a GLOF.

Thanks for these comments. We agree, the current structure is not logical or in keeping with best practice. In the revised manuscript, the methodological description on lake susceptibility will come before the section on GLOF modelling (and likewise in the results). The link between triggering processes and outburst process will be described, and used as justification for the choice of modelling approach (r.avaflow). Use of topographic potential (angle of reach) for the rock/ice avalanche likelihood will be removed. This will be replaced with a more comprehensive assessment of local geological and glacial conditions to estimate possible upper limits to an initial avalanche volume. Characteristics of the avalanche flow and its interaction with the lake will then be simulated with r.avaflow. The susceptibility factors in Table 3 are taken from the GAPHAZ international guidance document. As suggested, we will add references (from the region) to support the relevance of these factors in this particular study.

- L205: There seems to be a referencing issue: The item Table 3 must be Table 2.

Thanks – will be corrected.

- L222: 'cannot': 'may not'?

Will be revised to "may not"

- L217-231: It's good to know about historic changes in glacier velocity and elevation, but these insights seem to be decoupled from the future lake development. Please try to make this link clearer or consider deleting.

As suggested above, we will improve on the text in section 3.3 to ensure that the relation between glacier dynamics and geometry, and their impact on future lake development, are more clearly set out in this section. Contrary to the reviewers statement here, historic changes in glacier surface velocity and elevation are key to supraglacial meltwater ponding (e.g. Quincey et al., 2007; King et al., 2018) and initial lake development, so we will ensure this is clear to the reader.

- L241: Please elaborate more on what you mean with 'replicate'.
Will be revised to *"seen again"*

- L237-243: Do these scenarios also consider accelerated lake growth and glacier retreat, once the lake has formed?
They do not, which we are careful to acknowledge in the discussion of the results on lines L484-489. We will add to this text that lake expansion rates will likely be much higher when processes such as ice front calving begin to occur in response to lake expansion. Unfortunately, inclusion of ablative processes such as calving are beyond the scope of the approaches we employ in this work.

- L246: 'probability': again, there is no probability involved in this study. 'exposure of buildings' has not been mentioned in the Methods. 'A full hazard … assessment … is beyond the scope of this study': why then calling this paper then a hazard assessment in the title?
We accept the reviewers point. The opening sentence will be revised to *"We focus below on results relating to susceptibility of the 3 lakes, and the potential magnitude of downstream impacts, as simulated under worst-case outburst scenarios"*.
Furthermore, the results section currently titled "GLOF likelihood" will be revised to "lake susceptibility" (consistent with the methodology section). This section will come first, before the section on downstream impacts.
Please note also the suggested revision to the paper title: *"Glacial lake outburst flood magnitudes under current and future conditions: implications for disaster risk reduction in a transboundary Himalayan basin"*.
The approach and data used to assess exposure of buildings, will be noted in the methods.

- L250: Why did the simulations stop the border? Isn't it one of the goals of this paper to show the transboundary hazard from GLOFs?
We chose to only simulate until the border as that was the extent of the high resolution DEM we created, and further extension would have been at significant financial cost. This is noted later in the manuscript, but will be moved earlier. By simulating the arrival time and magnitude of the flood at the border, we believe this provides the intended basis for discussing transboundary hazard. Of course, detailed EWS planning will require modelling further downstream.

- L266: Why are the flow depths of 25m more realistic? Calls for some sensitivity analysis.
The problems with the lower resolution DEMs were clear and could be seen with simple profiles taken along the stream (stream path flowing uphill for example with obvious steps/blockages in the topography). So it was very clear that flow pooling and huge depths was artificial and not realistic. However, we agree that some sensitivity analyses around the parameters used in the modelling need to be included, and will do so in supplementary material.

- L274-276: Again, very simplified assumption on sediment entrainment and aggradation.
Using r.avaflow for the revised modelling will now allow us to make more quantitative statements around sediment entrainment and possible flow transformation.

- L285: 'significant': how much?
In the revised manuscript we will estimate this overtopping volume into Jialongco (assuming this overtopping still occurs in the new model results with r.avaflow).

- L287: 'high-impact low-probability': How high is this impact and how low is this probability?
As we are dealing with worst-case scenarios, we are assuming low (or very low) probabilities. As the reviewer no doubt knows, it's extremely difficult in the field of glacial hazards to assign quantitative

probabilities, unless dealing with reoccurring events (ice dammed lakes for example). However, we understand it would be important in our study to determine if such overflow into Jialongco might be possible with lower volume triggering events. We will therefore perform some sensitivity analyses on this, using initial volumes 75% and 50% less than the worst-case scenario to determine if runnup and overflow into Jialongco still occurs. The multiple simulations will not be continued downstream as this goes beyond the scope of the study. Results will be included in supplementary material.

- L288: 'requires more sophisticated modeling': unclear why this has not been considered in this specific case study that offers the data to do this sort of modeling.
In the revised manuscript we will attempt to simulate this chain reaction event using r.avaflow. This could be quite an innovative addition, given we are unaware of a model being used to simulate such a domino effect from one lake into the next.

- L301: 'likelihood or probability': replace with 'possibility'?
Here and throughout, we will speak only of likelihood, which can be qualitative (eg. high, med, low).

- L310: 'low': how low?
We don't believe given the scope of this study it is feasible to go beyond a qualitative probability level.

- L311: 'can be effectively discounted': why? And how efficient?
As above, we find it unfeasible to be more quantitative with such statements. As pointed out be other reviewers, the huge width of the dam make catastrophic erosion of the dam very unlikely. How the dam behaves in response to the avalanche triggered overflow in the r.avaflow simulations will give us some further basis for this statement.

- L316: 'large slope instabilities': In-SAR might help to detect those?
Noted, but outside the scope of this study. We will add to the discussion a point on the potential for In-SAR to be included in a monitoring/EWS strategy.

- L326-347: Most of this section could be redundant or offer much more potential for discussion, if the authors considered a suite of lake impacts in this study.
As noted in the response to the general comment, we prefer to keep the scope of this study on single worst-case scenarios, and believe there is justification for this. Using r.avaflow will now allow us to determine the outburst volume and downstream flood magnitudes linked to an assessed worst-case avalanche volume, and the section will be rewritten accordingly. We understand the challenges involved in estimating a worst-case avalanche volume, and will carefully discuss this aspect.

- L343: 'making this a high magnitude, but very low likelihood process chain': how high is this magnitude and how low is the likelihood?
Section will be rewritten based on new simulations, which will directly link the worst-case avalanche volume with the outburst volume. The estimation of likelihoods will remain an evidence-based qualitative estimate.

- L344-345: 'active instabilities are clearly evident': so, why not show these?
We will add a photo from the field showing this.

- L349-355: Similar to the comments above, it remains unclear, how the authors define the likelihood (or magnitude) of a GLOF.
Section will be rewritten based on new simulations, which will directly link the worst-case avalanche volume with the outburst volume. The estimation of likelihoods will remain an evidence-based qualitative estimate.

- L360: ‘Con.’, ‘Trig.’, ‘Mag.’: please write out fully. ‘Catchment drainage density’: why is this important. ‘TP = 3280’: Unclear what this means, please explain. ‘Steep moraine slopes >500 m high’: really? A 500 m high moraine dam?
Further explanation will be provided in the table, in line with the reviewer comments. The moraine slope >500m refers to the lateral moraine walls.

- L372: How complete is OSM in this region?
As noted in the text, both OSM and latest google earth imagery were used to ensure all exposed buildings were identified.

- L387: ‘While we did not simulate beyond the border owing to the limited coverage … ‘: needs to come earlier.
Noted, and will be moved earlier.

- L435: ‘applied these approaches for the first time’: really?
To our knowledge, yes, this is the first time a study has undertaken any sort of GLOF modelling for an overdeepening/future lake to give downstream flow heights, velocities etc. Previous studies have identified overdeepenings and downstream infrastructure that are within a GIS routed flow trajectory. However, to avoid a discussion around what is or is not considered GLOF modelling, we will remove the term “first time”.

- L436: What is ‘complex’ in this topography?
Will be revised to *“steep, mountain topography”*

- L444-447, and again L455ff: The artificial drainage of Jialongco somehow undermines parts of this paper, given that the simulations use the lake volume before the drainage, right? It would have been good to show how simulated flood magnitudes (flow depths, extent of the inundation, etc.) change because of the reduced flood volume, and if this in turn changes GLOF risk. In essence, if the probability of a given flood magnitude decreases (= hazard), then risk must also decrease, assuming constant values of exposure and vulnerability. It thus remains unclear why this insight has been held out from the introduction and all subsequent analysis.
We agree, and learned only of the artificial drainage late in the process of working on this paper (highlighting the disconnect between local authorities and scientists in this region). For the revised manuscript, we will now include a new scenario with the reduced volume.

- L458: ‘had only a minimal effect on the overall lake size’: Unclear conclusion with regards to Fig. 9.
Now that we have measured bathymetry for this lake (pre volume reduction) we’ll include an estimation of how much the volume has been reduced.

- L463: ‘would reduce the threat to these buildings’: contradicts to what is written some sentences above?
The sentence will be revised according to the results from the new scenario with the reduced lake volume.

- L470-483: largely repetitive from the Results, consider substantial shortening or deleting.
Repetition to be removed.

- L495-499: Content of these sentences unclear, even if formulated ‘in other words’. Please rephrase or shorten.
The sentence will be revised for clarity and in line with the revised modelling results.

- L501: When talking about early warning, why not using the calculated flood arrival times to provide a solid basis for discussion? What do the authors suggest to implement in such an early warning system? How can people response to the warning?

This part of the discussion will be expanded as suggested, based on the flood arrival times modelled with r.avaflow. We'll also refer to the UNDP guidelines on the essential components of an EWS, including how people can respond, highlighting that a functional EWS goes far beyond the technical components.

- L503-504: 'under the philosophy of preparing for the worst, while hoping for the best': please avoid jargon.

Agree, rather more language for use when presenting to authorities and decision-makers and will be removed here.

- L504: 'complex transboundary regions': what is complex here and how can this study help to understand this complexity?

Wording of "complex" will be removed. The sentence is intended to simply highlight why warning times are so critical in a transboundary region, because we know from past experiences that communication between national authorities can lead to delays in alerting communities.

- L523-525: Not sure whether the increase in GLOF risk has been quantified in this study?

Fair point. We will revise to *"increased GLOF exposure"* which is justified based on Figure 6.

- L528-529: 'Hard engineering strategies that address only the hazard source are a socially and environmentally less desirable option': Not sure whether I can agree with this statement: If Hazard = 0

We want to say here that hazard will never = 0 in such a context, unless you drain every lake completely, and do so for every new lake that develops. In addition, even if you arguably could reduce GLOF hazard to zero, the community remains vulnerable and exposed to other geohazards. Hence, a more comprehensive approach, involving also EWS and related social interventions, are far more desirable in almost all contexts in our view. This does not mean that remedial measures do not have a place within such a comprehensive approach. In view of the reviewers concern, we will revise the wording to: *"Hard engineering strategies, in isolation, do little to address underlying risk drivers of exposure and vulnerability to GLOFS and other geohazards, and are likely unsustainable in the face of ongoing environmental changes and lake growth"*