Reviewer 2

General Comments:

1) The overall framework and structure – in the current version of this study, the authors first do the 'worst case' modelling and then search for possible GLOF triggers to justify modelled results (which is actually done not very convincingly when admitting that modelled GLOFs would need very unlikely occurrence of high magnitude (X0 Mm3) ice-rock avalanche into the lake as a trigger); logical framework would start with: (i) search for possible / likely GLOF triggers for existing lakes, (ii) feeding them into definition of outburst parameters and scenarios, and (iii) leading to GLOF modelling + (iv) future lake and GLOF. I suggest to consider re-structuring the manuscript accordingly

We thank the reviewer for his careful and comprehensive comments, and apologise for the time taken to respond. We agree that the logic of the paper structure was not ideal or in line with best practice. Both the methods and results will be restructured to first address lake susceptibility (incl. triggering), before proceeding to GLOF modelling, and finally consideration of the future lake. The fundamental methodological change we will be making, based on comments from all reviewers, will be the use of r.avaflow to simulate the GLOF process chain, thereby directly linking the triggering events (large ice-rock avalanche) to the outburst event.

2) Uncertainties in input data: as the future is uncertain, I'm quite reluctant to using any single value 'worst case scenario' concept and I call for using a range of values (and scenarios) instead. Below I comment on (some of the) major sources of uncertainties which are cumulating throughout the process and are not properly treated:

We understand the reviewers concern, and ourselves are involved in several studies where the goal necessitates a full range of scenarios are modelled. However, in this paper we are aiming to address what we see as 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). Likewise, we believe these questions can be answered without going to the level of detailed scenario-based hazard modelling. 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 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. We therefore will maintain the focus on worst-case scenarios, but will revise the way in which these scenarios are developed. The revised approach will focus on defining an upper limit to an expected ice/rock avalanche starting volume, and use r.avaflow to simulate the resulting GLOF cascade. For the case of the GLOF from the new lake potentially overflowing into Jialongco and causing a simultaneous outburst from that lake, additional sensitivity analyses will be undertaken to determine if this potentially critical process chain could also occur under smaller initial scenarios.

a) the essential value at the very beginning is the estimation of breach depth (in this study referred as breach height hb). The authors provide neither details on how this value is estimated nor what the uncertainty of this estimation is; another issue is whether flat (<7° (rough Google-Earth-based measurement)) and pretty wide (> 450 m) moraine dam (e.g. Galong co) could ever be breached; and if it is breached, the crucial question is how deep (longitudinal profile of the breach is typically far from flat – I mean, if you have a vertical difference between the lake level and the toe of moraine dam 40 m (this is how you define breach depth, right?), lake level decrease in case of breach will be less than that (it is not going to be breached to 0° slope), depending on longitudinal width of dam body; this is actually seen in Fig. 2b: if you define breach depth in this way, you should not use the same value in calculating released volume, because it differs to the lake level decrease (and in turn it leads to substantial overestimation of released volume))
We agree that these parameters and assumptions on breach depth were not well substantiated. The revised modelling approach using r.avaflow will reduce the need for these assumptions around the dam breaching and/or overtopping mechanisms. The erodible area of the dam will still need to be defined, and will be done based on careful consideration of the dam characteristics including any possible ground ice (relevant for Galongco only). In addition, we've since obtained bathymetry measurements from both Jialongco and Galongco that will be used for the modelling.

b) in the next step, the authors use this pretty uncertain value to estimate released volume (which is not correct in my opinion, see above) and breach parameters, using Froehlich (1995) empirical relationships; but it is important to realize that: (i) Froehlich (1995) is based on compiled information of man-made earthen dam failures, not natural dams; (ii) failure mechanism of most of these cases in the database was piping, not overtopping; (iii) released volumes in the dataset was mainly <1Mm3; and >100 Mm3 in only two cases (Oros, Teton); with expected released volumes 25, 70 and 262 Mm3, you are extrapolating far beyond observed data of Froehlich (1995) and the uncertainty is unknown (Froehlich, 2004 should be checked).
Based on this, and other comments, we will no longer use the Froehlich approach. The GLOF process chains will be simulated using r.avaflow, with breach parameters dynamically calculated. Important model parameters (e.g. 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) Timing - Using Eq. 4, calculated time for breach formation of Galong co is 153 min, but you expect peak discharge in Nyalam in 82 min -> please explain what times are you referring to (82 min from breach initiation, from peak discharge at the dam (when from breach initiation?) or from development of the breach?); being as clear as possible is especially important when talking about EWSs, presenting hydrographs at the dam would be beneficial.
A new figure will be included giving the hydrographs at the dam – thank you for this suggestion! For clarity, all timing will be revised to be relative to the impact of the initial mass movement into the lake.

4) GLOF likelihood – this section gives some largely general statements and qualitative RSbased observations and looks more like a discussion rather than result to me; Tab. 3 summarises first order GLOF susceptibility factors, but this study is not a first order

assessment – it is a detailed study of two existing and one potential future lake; what is shown in Tab. 3 is perhaps true for most of the lakes in the region (warming climate, steep slopes and crevassed glaciers upstream, …) and leaves the question of GLOF likelihood open; the use of >30° threshold for initiation of mass movements seems too simplifying and not really helpful for the scale you are working on
In line with other reviewer comments, this section will be renamed to "lake susceptibility" recognising that it is outside the scope of this study, and probably not feasible, to provide quantitative likelihoods linked to specific scenarios. Table 3 will be revised, and where possible, quantitative details will be provided. Please note however, many observations (e.g. permafrost characteristics) will remain RS based and to some extent qualitative. The slope threshold-based avalanche calculations will be removed, and replaced with a more detailed assessment of local geological and glacial conditions. It's true that some statements are general and could be applied to other lakes in the region, e.g, "No permafrost in dam. Degrading permafrost in surrounding slopes", but there is no basis for a more detailed quantified statement as there are no high resolution permafrost models for this area. Nonetheless, we prefer to keep the table in full, even if some entries are general, because this is the first study to directly utilise the comprehensive "check-list" table of susceptibility factors coming out of the GAPHAZ international guidance document, so an important opportunity to demonstrate both the applicability and limitations of the GAPHAZ approach.

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 scenario-based 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 is rather limited here 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".*

5) Practical implications – the authors mention the importance of such studies for local authorities, which is in principal true and also a rationale of many similar studies. My experience is that practical utilization, however, often lacks behind. As documented by the authors, local authorities meanwhile started remedial works by themselves, meaning that they have some kind of GLOF hazard assessments and management procedures in hands. I expect these documents may not be publicly available, but attempting getting in touch with authorities in charge of these measures would be highly appreciated (and could also help to bridge the gap between what scientists and authorities are doing).
This is a great comment on the practical implications of this project. We have struggled to make progress here throughout the 3 years of the project, and despite several efforts in the field, we've been unable to enter into exchange with the local authorities and learn details from them about what is planned in terms of DRR measures. In Tibet, and close to a military controlled border region, exchange with authorities is even more challenging than in other regions we have worked. It seems almost everything is explained as being "for military purposes". Hence, the onset of remedial measures, has come as a surprise, and there is no evidence that these measures are underpinned by GLOF modelling. A desire to bridge this gap between scientists and authorities is exactly why we focussed this study on the methodological space between first-order assessment approaches, and detailed scenario-based hazard assessment. In our view, it is within this space that we can most reasonably demonstrate the importance of considering future lakes as part of DRR planning. Going to the level of comprehensive scenario-based GLOF modelling and hazard mapping only makes sense in our view once there is buy-in from local authorities, particularly with regards to the threat of a future lake. Otherwise it remains an academic exercise only.

Specific Comments:

L19-20: please comment on what can be done to reach this ambitious aim (not a part of the study)

We agree that our study rather does not feed directly into "decision making" and we will remove reference to this in the abstract. Nonetheless, as described in the responses above, we do believe our study provides fundamental insights for future-proofing DRR planning. For example, demonstrating that land zoning or EWS planning based on the threat from Jialongco and Galongco will possibly be insufficient to deal with the threat from a large new lake forming some decades later in the same basin. Anecdotally we know there are plans from international donors to fund the design and implementation of a transboundary EWS in this basin, so such messages are important of feed into the scoping of such projects.

L40: high magnitude

As per comments from other reviewers, we will avoid subjective wording here, and rather refer to the reported GLOF frequency of 1.3 per year for the Himalaya since the 1980s (after Vey et al 2019).

L52: I would not call 17 GLOFs overt the Tibet since 1935 'particularly common'

Agree – sloppy wording. We'll revise factually to state "At least 17 GLOF disasters (causing loss of life or infrastructure) have been documented in Tibet since 1935, mostly….."

L68: these numbers are confusing; you mentioned 3-fold increase, does it mean that future doubling in border areas of China – Nepal is thus below average?

Agree – taken out of context of the original paper these numbers are confusing and cannot be directly compared. To avoid confusion we'll remove the reference to a 3-fold increase in risk, and focus just on the doubly of transboundary lakes, which is the most relevant information for the present study.

L82-85: not sure this is met

We agree that our study rather does not feed directly into "decision making" and we will remove reference to this. Nonetheless, as described in the responses above, we do believe our study provides fundamental insights for future-proofing disaster response planning. For example, demonstrating that land zoning or EWS planning based on the threat from Jialongco and Galongco will possibly be insufficient to deal with the threat from a large new lake forming some decades later in the same basin..

L92: please consider adding description of 2(3) studied lakes in this section

Thanks for this excellent suggestion. We'll add a physical description of the 3 lakes, including key physical parameters and for the two current lakes, details on assessed hazard/danger levels provided in previous studies. This will also help better justify the focus on these lakes.

Fig. 1: please consider adding topography info; there are many dangerous lakes in the region – the authors are asked to justify why they focus on these two existing and one potential future lake (while there are other lakes forming currently)

We propose to add a series of inset photos showing details on the three lakes. Linked to the physical description added to the text (previous comment), the images and text will justify the focus on these 3 lakes.

L164: methodology of obtaining hb is not clear
Under the new modelling approach, hb will not be estimated in this way, but will be dynamically calculated during the simulation.

L178: what breach scenarios?
Apologies, this should have referred simply to the 3 worst-case scenarios. Text will be revised.

L189: this value needs justification
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.

L210-214: this approach seems too rough for detailed case study like this one
Agree – the approach to assessing ice/rock avalanche susceptibility will be revised, and will be based on detailed consideration of local geological and glaciological conditions.

L247: considering uncertainties behind a single-number result, I found a range of values highly desirable
See response to general comment (2)

L252 ms-1 when talking about velocities (please check throughout the manuscript)
Thank you – will be checked throughout.

L274-297: this is contradicting; on the one hand you expect 48 m deep breach of very flat moraine dam and on the other hand you find erosion unlikely?
Wording will be clarified. The lack of erosion potential is referring to flow path downstream of the lake, where channel slopes are relatively gentle. However, you are right that the 48 m deep breach is also rather unlikely given the very flat moraine dam – and this is mentioned later in the manuscript. Under the new approach to modelling with r.avaflow, the dam breach might be considerably less, and wording/values will be revised accordingly.

Table 1: please specify timings (see my general comments); Jialongco – are these values of the lake before or after the remediation?
Timings will be revised based on the new simulations, and reported consistently relative to the time of avalanche impact into the lake. For Jialongco, we'll now include an additional simulation for the reduced lake area/volume and altered dam geometry. This will allow us to assess the effect of the recent remedial measures on downstream flood magnitudes.

L300: consider moving to discussion (see my general comments)
See response to general comment (4). We will add quantitative detail here to the extent possible. This goes far beyond discussion in our view, even if some details will remain qualitative.

L319: this is not very well-argued (most of the glacial lakes are surrounded by glacierized slopes with >30°)
The slope is not really the main factor here, but rather the smooth, ramp-like topography of the glacier tongue and likely temperate bed. We will revise the text to place more emphasis on these characteristics, including evidence of past instabilities of this tongue.

L326-343: yes, large volume ice-rock avalanches are rare and in the seismically active regions, you can't rule out the possibility of hitting the lake – you can say this about most of the lakes in the region (and most of the high mountain lakes globally); I'm wondering

whether is there any site-specific implication for GLOF likelihood?
One key factor here is the size of the mountain headwalls immediately surrounding the lake (i.e., 3000 metre high slope of Shishapangma). Another factor is the potential peak ground acceleration linked to seismic activity (we will update the table to include this information), which varies across the Himalaya and is enhanced in such steep topography. We will also revise the text to include more detail on the structural geology of the surrounding slopes and glacial conditions (derived from the high resolution DEM). Of course, we will not get to a quantified likelihood level, but will be able to make a stronger site-specific qualitative assessment.

L346: Klimes et al. actually showed that landslides in moraines are not capable of producing any large GLOF from Lake Palcacocha
Apologies – this citation will be removed.

Tab. 4: what is freeboard to height ratio? Both existing studied lakes seem to have surface outflow (freeboard = 0m); catchment stream density / order seem odd for evaluating GLOF likelihood; you also report no evidence of historical instabilities, further questioning the likelihood of such events for triggering GLOFs
Reviewer refers to table 3 we believe. As has been done is many other studies, we considered the remaining freeboard (i.e., a hypothetical line across the crest of the dam), irrespective of the fact there is surface outflow. The logic being that the remaining freeboard area still offers some protection in the case of an overtopping wave. We will add a footnote to the table with this detail. Stream density/order is related to the potential for rainfall/snowmelt triggering of a GLOF (after Allen et al. 2016 paper on Kedarnath). All factors come from the GAPHAZ guidance document. We Agree that details on historical instabilities are needed, and we will add this evidence (and references) to a revised version of the table.

Tab. 4: again, estimating possible ice avalanche starting zones with precision to 1 m2 is not appropriate considering apparent uncertainties; better use a range of values
Agree the precision is unjustified. This table and section will be revised to focus on the largest scenarios affecting each lake, and will include an estimated ratio if ice to rock (again for a worst-case), as required for input to r.avaflow.

Fig. 6: if intensities are based on flow depth only, why not to use flow depth directly?
Fair comment – the thinking was that intensities are more generic, and allow authorities to compare with other approaches and estimates, including for other hazard processes.

L418-418: please comment on a difference between values estimated here and size of the future lake considered in Tab. 1?
This comparison between estimated lake size at 2100, and the maximum anticipated lakes size will be added here. We note that the estimate at 2100 does not account for ice calving feedbacks, so is expected to underestimate actual size, as commented in the discussion section.

L433: there is not much about management planning in the study
As noted in previous responses, we believe the results of this study have important implications for DRR planning, and are relevant for local authorities, even if the results cannot directly on their own be taken as a basis for hazard mapping or EWS design.

L435-436: the authors published several studies on GLOF from potential future lakes previously
Yes that is true, but 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”.

L446: the greatest immediate threat from 2 existing studied lakes
Sentence will be revised (assuming it remains true with the new simulations).

L450-454: this is not suggesting any lower limit, this is estimated potential loss for given scenario; please re-word this sentence
Sentence will be removed as we will no longer use the estimated maximum potential flood volume as input to the simulations.

L456-458: maybe the remediation is still in progress?
Yes, we agree, and hope this is true, but as indicated in the response to the general comment, it has not been possible to get information on this from authorities. We’ll add a line that remediation may well be an ongoing process.

L465: EWS can help to save lives, but not the immovable property (which may already be there); if the value of potentially affected immovable property is >> than the cost of remedial works, then it makes sense also to remediate the lake(s)
Fair point and we were too dismissive of remedial measures in the initial draft. Both here and in the conclusions, we will revise the wording to make it clear that a comprehensive solution is recommended, which includes (but is not limited to) remedial works at the lake.

L508: no clear conclusion on GLOF likelihood is given
While we do not provide a quantitative assessment of likelihood, we provide clear conclusions on the relative likelihood of an outburst from the lakes, concluding that Jailongco (based on consideration of all susceptibility factors) has the highest likelihood of generating a large outburst event. In the revised manuscript, based on a more detailed assessment of local geology and glaciological conditions, we’ll strengthen this aspect to the extent that is possible.

L515: details about the project (planned final stage) should be presented (maybe the plan is to drain the lake much more?)
As written in previous responses, information from local authorities working in this sensitive military-controlled area, has not been shared. We’ll add a line to the conclusions that remediation may well be an ongoing process.

L519-519: this is general qualitative statement which is true for many lakes in the region (not very helpful for DRR authorities I guess)
This conclusion will be revised based on the new large-volume ice/rock avalanche scenario to be simulated. Reminding authorities that such very large/high impact events need to be considered in DRR planning is important in our view, and worth the line that is included here.

L529: why are they socially less desirable? And why environmentally less desirable (GLOF is a major disturbance to the valley ecosystem)?
In view of concerns raised by multiple reviewers we will remove the reference to social and environmental sustainability as this may be subjective (depending on how local people view glacial lakes). The wording will be revised 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”