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Dr. Tobias Bolch
The Cryosphere

16th April, 2021

Revision of manuscript "Controls of outbursts of moraine-dammed lakes in the greater Himalayan region"

Dear Dr. Bolch,

thank you for the quick and transparent review process. Please find our point-by-point responses to the referees below. Changes to our manuscript are cited with line numbers which are based on the revised manuscript file without track changes.

We also thank you for your remark concerning potential data sources of glacier-mass balances prior to 2000. Although we decided to not include these in our analysis at this point we will gladly consider them for future studies.

Best regards,

Melanie Fischer
On behalf of all co-authors

Reply to Referee #1 (RC1)

General comment #1:

> *The authors present interesting results, some of which are novel in a sense that contradict assumptions of previous GLOF hazard assessment studies (e.g. the assumption that fast-growing lakes are more susceptible to GLOF), but this is only one part of the story (so far pretty much model-oriented) in my opinion. If the overall aim is enhanced identification of potential future GLOF sites or so, stronger linkages of investigated GLOF indicators to physical processes behind as well as (at least brief) characterization of documented GLOFs (in terms of triggers, mechanisms) are missing. For instance, how (process-wise) is the EDW, glacier-mass balance or lake (catchment) area linked to documented GLOF? What are triggers of historic GLOFs considered in this study? In fact, I'd expect this to be taken into consideration in the very first step – selection and justification of GLOF indicators.*

We appreciate the reviewer's comment and elaborated in detail the choice of our predictors in a new table (now Table 2). We rewrote large parts of Section 2.1 to make clear how we selected each predictor. For example, we note that:

- "Larger and growing lakes offer more area for impacts from mass flows such as avalanches, rockfalls, and landslides originating from adjacent valley slopes (Haeberli et al., 2017)." (L123-124);
- "A larger upstream catchment area has been associated with an increased susceptibility to GLOFs as more runoff from intense precipitation, together with glacier and snow melt, can lead to sudden increases in lake volume (Allen et al., 2019; GAPHAZ, 2017)." (L126-128);
- "These readily available data on regional glacier-mass balances are proxies for other, less accessible, physical controls on GLOF susceptibility such as glacial meltwater input, either directly from the parent glacier or from glaciers upstream, as well as permafrost decay in slopes fringing the lake." (L133-136);
- "Meteorological drivers entered previous qualitative GLOF hazard appraisals mostly as (the probability of) extreme monsoonal precipitation events: the Kedarnath GLOF disaster, for example, was triggered by intense surface runoff (Huggel et al., 2004; Prakash and Nagarajan, 2017). Heavy rainfall may also trigger landslides or debris flows from adjacent hillslopes followed by displacement waves that overtop moraine dams (Huggel et al., 2004; Prakash and Nagarajan, 2017). Elevated lake levels during the monsoon season also raise the hydrostatic pressure acting onto moraine dams (Richardson and Reynolds, 2000). Furthermore, different precipitation regimes and climatic preconditions may also influence moraine dam failure mechanics (Wang et al., 2012)." (L137-143)

Background information on triggers is scant or conjectural for most GLOFs in our study region. We now offer a more thorough discussion on possible triggers: "The triggering mechanism of these studied GLOFs is reported in only seven cases, four of which are attributed to ice avalanches entering the lake (e.g. Tam Pokhari, Nepal or Kongyangmi La Tsho, India; Ives et al., 2010; Nie et al., 2018). Other triggers of the GLOFs studied here include piping (Yindapu Co, China; Nie et al., 2018) and the collapse of an ice-cored moraine (Luggye Tsho, Bhutan; Fujita et al., 2008)." (L157-160). Please also see our response to general comment #2 in this regard.

Contrary to the reviewer's notion, the goal of our study is not to *"identify potential future GLOF sites or so"*. Our goal is to explore possible predictors of **historic** GLOFs. We had stressed this issue in the original Abstract: "We use a comprehensive inventory of 3,390 moraine-dammed lakes and their documented outburst history in the past four decades to test whether elevation, lake area and its rate of change, glacier-mass balance, and monsoonality are useful inputs to a probabilistic classification model” (L13-15 of orig. manuscript); and: “We find that mostly larger lakes have been more prone to GLOFs in the past four decades, regardless of elevation band in which they occurred“(L17-18 of orig. manuscript), and in many other locations in the manuscript.

General comment #2:

> *It would be interesting at least discuss how many of documented GLOFs were actually triggered by processes associated with investigated GLOF indicators? This is briefly touched in the introduction (L36-39) or study area section (L108), but I’m convinced that bit deeper and more comprehensive elaboration (e.g. a separate discussion section) would be beneficial for readers. Another example - on L244-245 it is mentioned that ‘greater lakes are more likely to having had a GLOF ...’. I wonder what do primary data say about this – what proportion of these 31 GLOF-producing lakes would be classified as large at the time of GLOF and what this proportion is in the population of 3,390 moraine-dammed lakes? And in the other way around - can a specific combination of values of GLOF indicators infer about possible (likely) GLOF trigger and mechanism (if not known)?*

The reviewer may acknowledge that the link between mechanistic processes, choice of statistical predictors, and the occurrence of GLOFs invites some interpretation. If we had sufficient data to run adequately parameterised process-based, numerical models on past GLOFs, we could go beyond the outputs of statistical models of outburst susceptibility. Our motivation, however, is to choose predictors as **proxies** instead of physical parameters in a deterministic model. Each of these proxies subsumes various physical processes that might be relevant to producing GLOFs (now further stressed in our new Table 2 in L147 and in the additions we made to Section 2.1 in L123-143). How well these proxies can describe the presence or absence of a GLOF is what our probabilistic models predict. These models learn from the data directly and inform us about the suitability of our predictors to hindcast historic GLOFs. By design, the outputs are probabilities and less so physical triggers or mechanisms. This probabilistic approach forms a cornerstone in modern hazard and risk analyses.
In this context, we are unsure what the reviewer means by “*primary data*”. Our model learns from all the data as stated in the Methods section. We also note that the role of lake area in our model is that of a **continuous** predictor and not that of a response variable, as the reviewer seems to suspect. The model summarises how the susceptibility to GLOFs changes with lake area rather than vice versa.

General comment #3:

> *Let me also critically comment on some of the selected GLOF susceptibility indicators (in general, I’m convinced it would be useful presenting these indicators in a separate table with more detailed and comprehensive description than stated in the overview Tab. 1, and in places of the text):*

We appreciate this suggestion and split Table 1 into two new tables: Table 2 (L147) now provides more detail on, and motivation for, our selected predictors.

> *Lake area change: I’m aware this indicator is always tricky to define and employ; according to what is written on L134-135, two intervals are used for lake are change (1990-2005 and 2005-2018); considering GLOFs occurring throughout the period 1981-2017, it means than these intervals may be pre-GLOF, post-*

> *GLOF or the GLOF occurred somewhen during one of these intervals – please comment on how this inconsistency was treated and whether it can explain that no link was observed between lake area change and the occurrence of GLOF.*

The data on lake-area changes are not yet resolved on an annual basis for our study area, so that we had to resort to changes averaged over longer periods. However, we used our forecasting model to test whether changes in lake size between two observation periods had a credible effect on P_{GLOF}. Here we explored the weight of relative changes in lake area between 1990 and 2005 to estimate the probability of observing GLOFs that happened in the subsequent period 2005-2018. In other words, we trained the model on GLOF data that **pre-date the testing data**, and thus offer a realistic and rigorous prediction and validation scenario. We reported in our original manuscript, that "The weight of relative lake-area change in the 15 years before is ambiguous ($\beta_{A^*} = -0.04^{+0.76}/_{-0.67}$) […]" (L245-246 of orig. manuscript) and that "In the forecasting model, however, the influence of lake-area change remains negligible even for <50% HDIs." (L350 of orig. manuscript). This indicates with 95% probability that relative lake-area change **before** the outburst is an inconclusive predictor. To further stress this result, we added the following sentences in the Discussion: "However, in the forecasting model, in which we tested whether differing data observation periods have any credible effects, the influence of lake-area change remains negligible even for <50% HDIs. We thus conclude that relative lake-area change before outburst is an inconclusive predictor. This result contradicts the assumptions made in many previous studies that argued that rapidly growing lakes are the most prone to sudden outburst (Aggarwal et al., 2016; Bolch et al., 2011; Ives et al., 2010; Mergili and Schneider, 2011; Prakash and Nagarajan, 2017; Rounce et al., 2016; Wang et al., 2012)." (L382-387).

> *Glacier mass balance: similarly to my comment on lake area change - how can 2000-2016 glacier mass balance be used to explain GLOFs occurring throughout the period 1981-2017? These characteristics (mass balance as well as lake area change) are dynamic in nature and I'm wondering how can a static information from available datasets possibly blur a GLOF signal, especially for pre-2000 GLOFs?*

The problem of limited data for lake-area changes is even more pronounced for glacier-mass balances in our study area. Again, the data averaged from 2000 to 2016 are among the few regionally consistent data sets that we considered as input for our model. The underlying assumption is that the regional regime of prevalent glacier melting from 2000 to 2016 largely follows a trend dating back to the late 1980s. This is in line with the review of Bolch et al. (2019) who summarized that *"glaciers* [in High Mountain Asia] *have thinned, retreated, and lost mass since the 1970s, except for parts of the Karakoram, eastern Pamir, and western Kunlun"* (p. 211).

To answer the reviewer's question of "[…] *how can a static information from available datasets possibly blur a GLOF signal, especially for pre-2000 GLOFs?"*, we curtailed our glacier-mass balance model only to those lakes that had documented outbursts **after 2000,** and hence that overlap with the study period of Brun et al. (2017). Table R1 shows the output from this model:

Table R1: Posterior model parameter estimates for the glacier-mass balance model trained on a post-2000 GLOF subset.

Model parameter	Estimate	Estimation error	Lower 95% CI boundary	Upper 95% CI boundary
$\alpha_{z,r}$	-7.32	1.36	-10.40	-5.02
$\beta_{A^{*}b}$ (2005 to 2018)	-0.62	0.29	-1.19	-0.06
β_C	0.88	0.22	0.44	1.32
γ_r	-2.87	3.00	-9.64	2.13

σ_z	0.48	0.42	0.02	1.53
σ_r	1.11	0.72	0.08	2.88

We find that:

- The parameter estimates at the population level changed only minutely: the weight of catchment area (β_C) remains credibly positive and that of lake-area change from 2005 to 2018 (β_{A^*b}) remains credibly negative.
- At the group level, the standard deviation of intercepts of our grouping variable elevation (σ_z) is also similar to our previous results.
- Posterior estimates of σ_r, the standard deviation of group level intercepts of glacier-mass balance regions, increase from $0.81^{+1.60}/_{-0.78}$ to $1.11^{+1.77}/_{-1.03}$, **though with much overlap**. This further underlines our finding that the glacier-mass balance in a given region credibly affects P_{GLOF}.

We now highlight these findings in our results: "On the basis of higher standard deviations, we learn that effects of glaciological regions vary more than those of elevation bands ($\sigma_r = 0.81^{+1.60}/_{-0.78}$ and $\sigma_z = 0.48^{+1.19}/_{-0.47}$). When training this model on a subset of glacial lakes with documented GLOFs that happened after 2000 (i.e. including only those in the interval covered by glacier-mass balance data), posterior estimates of σ_r increase to $1.11^{+1.77}/_{-1.03}$, further underlining our result that glacier-mass balance credibly affects P_{GLOF}." (L304-308).

> *Monsoonality: using climate indicators in GLOF research is promising, but proportion of summer precipitation doesn't tell you about the extremity; for instance, the proportion will be lower in areas where extreme rainfalls occur in summer, but also some precipitation in winter, but will be super-high in generally dry areas with some precipitation during the summer and no precipitation in winter. But process-wise, the first area will have much higher potential to trigger GLOF in my opinion.*

We are unsure about whether the reviewer offers an opinion here or whether their statement is supported by data. Our analysis shows that the proportion of summer precipitation is highest in areas with strong monsoonal influence (Fig. 1). We are unaware of any GLOFs that have been reported in winter. For example, ice cover on lakes and freezing moraine dams have been thought to make glacier lakes resilient against outbursts, even during strong seismic shaking (Kargel et al., 2015). Most of the heavy rainstorms are tied to the South Asian summer monsoon, and some reported GLOFs were triggered by such storms (Allen et al., 2016; Liu et al., 2014). The drier areas of our study area usually receive higher amounts of precipitation during winter via westerlies (Bolch et al., 2012; Bookhagen and Burbank, 2010), so we think that monsoonality remains a useful predictor and did not change our manuscript in this regard.

Detail comment #1:

> *L11: yes, the approach is quantitative, but selection of GLOF indicators in this study is also expert judgement-based as the authors are GLOF experts*

We rephrased this sentence to "Estimating regional susceptibility of glacial lakes has largely relied on qualitative assessments by experts, thus motivating a more systematic and quantitative appraisal." (L10-12).

Detail comment #2:

> *L34: see also Cook et al., 2018, Science*

We thank the reviewer for suggesting this useful reference, which we added to our manuscript (L35).

Detail comment #3:

> *L36-37: this needs deeper elaboration in relation to selected GLOF susceptibility indicators (see also my general comment)*

We refer the reviewer to our reply on General Comment #3.

Detail comment #4:

> *L103: I suggest to use 'GLOF susceptibility indicators' instead of 'diagnostics of GLOF potential' or 'diagnostics of GLOF hazard' (L125); similarly, 'controls' and 'predictors' are used throughout the manuscript, please define a difference or unify*

We decided to now consistently use the term "predictor", in line with the common terminology in the statistics literature. Our use of "diagnostic" is also appropriate, as the regression model has a bivariate outcome. Yet to make things more clear, we replaced "diagnostic" by "predictor" throughout the manuscript. We avoid the term "indicator", as it may be confusing in the model context. In regression models, an indicator variable is often a logical binary [0, 1] or dummy variable, whereas we mostly use continuous variables.

Detail comment #5:

> *L111-112: lake deepening increases hydrostatic pressure, not areal or volumetric growth*

This is physically more correct, though we fail to see how any change in lake area or volume could not affect hydrostatic pressure eventually. To be more clear, we rewrote our statement to: "Lake area scales with lake volume and depth (Huggel et al. 2002), and growing lake depths increase the hydrostatic pressure on moraine dams, thus raising the potential of failure (Rounce et al., 2016)." (L119-121).

Detail comment #6:

> *L115-116: the authors usually argue that larger lakes are more susceptible because large lake areas are more exposed to slope movements potentially triggering GLOFs; large area is also correlated with larger depth (and so hydrostatic pressure acting on a dam)*

We added this reasoning to the text: "Larger and growing lakes offer more area for impacts from mass flows such as avalanches, rockfalls, and landslides originating from adjacent valley slopes (Haeberli et al., 2017)." (L123-124).

Detail comment #7:

> *L130: how is different date of GLOF and input data for model treated? (how possibly different environmental conditions at the time of GLOF and at the time of datasets acquisition can influence your results?) see also my general comments*

We used the reported dates of historic GLOFs where available. We acknowledge that our predictor variables can only approximate the environmental conditions at the time of lake outburst, but this is the point of a statistical predictor in a data-driven model. Please see our replies to General Comments #1-3 in this regard.

Detail comment #8:

L136-139: I suggest to move this to L133

We moved and slightly rephrased this text passage accordingly: "These variables are correlated with elevation because of the same underlying interpolation technique so that we limited our models to those with poorly correlated predictors. This meant omitting other predictors such as mean annual temperature, annual precipitation totals and annual temperature and precipitation variability." (L163-165).

Detail comment #9:

L166: delete 's'

Deleted accordingly.

Detail comment #10:

L176: delete ','

Deleted accordingly.

Detail comment #11:

L207: what is meant by 'common susceptibility'?

For clarification, we rephrased this sentence to "In essence, this varying-intercept model acknowledges that glacial lakes in the same elevation band may have had a common baseline susceptibility to GLOFs in the past four decades." (L239-240).

Detail comment #12:

L262-263: this step is not clear to me? Please explain

To clarify our approach, we added more details on our predictor catchment area in the new Table 2 (L147). We explain our choice of this predictor and why we use it instead of the static lake area A in the glacier-mass balance and monsoonality models: "We also tested the impact of upstream **catchment area** C (m^2) on GLOF susceptibility. A larger upstream catchment area has been associated with an increased susceptibility to GLOFs as runoff from intense precipitation as well as glacier and snow melt can lead to sudden increases in lake volume (Allen et al., 2019; GAPHAZ, 2017)." (L126-128).

Detail comment #13:

L263: please provide details about this correlation

We find that catchment area C has a strong linear correlation with lake area A (Pearson's correlation coefficient of 0.446), such that we preferred C over A in two of our models, as C is invariant at the timescale of our study. We also added this to the text: "We find that catchment area C correlates with lake area A (Pearson's $\rho = 0.45$) and, thus preferred C over A in two of our models, as C is invariant at the timescale of our study." (L128-130).

Detail comment #14:

L272: what is meant by 'average lake'?

The average lake is defined by the combination of all average predictor values. We added this definition to the text: "This model has the highest values of P_{GLOF} for average lakes (i.e. all average predictor values combined) in the Nyainqentanglha Mountains and the Eastern Himalaya (Fig. 4).“ (L209-311).

Detail comment #15:

L352-354: this can be true for a specific period in long-term evolution of a mountain range (considering gradual glacier retreat and overall shift of all rapid processes including GLOFs to higher elevation zones; i.e. the general shift of morphoclimatic zones)

The point we wanted to make here was that stratifying by elevation hardly helped to inform us more about GLOF susceptibility in this context. In essence, GLOF susceptibility is aptly represented by the pooled model here. We did not change our manuscript in this regard.

Detail comment #16:

L365: so why not to consider this indicator in your model?

We ran a number of models that used the distance from the parent glacier as a predictor, though obtained no credible posterior weights. However, we found that this distance is most likely the most prone to highly dynamic changes in historic times. We thus added to the text: “However, systematically recorded time series of glacier fronts are even harder to come by when compared to systematic measurements of changes in glacial-lake areas.” (L403-404).

Detail comment #17:

L373: are ‘minute’? Please check

We clarified this: “[…] deviations from a pooled model for the HKKHN are minute when compared to the spread of posterior group-level intercepts in the other models (Fig. 4).” (L411-412).

Detail comment #18:

Tab 2: what is PDF?

The abbreviation PDF stands for probability density function. We changed the column header in Table 3 (former Table 2) accordingly (L227).

Detail comment #19:

Tab. 4: please also consider presenting false positives and false negatives

We accordingly added false positives and false negatives in a separate column to the table (now Table 5, L364).

Detail comment #20:

Fig. 2: three lake inventories are mentioned (ICIMOD, Veh et al., 2019 and Wang et al., 2020); please make clear how these were integrated; these 3,390 lakes (L131) are from which inventory?

The 3,390 lakes forming our database are a subset of the ICIMOD inventory published by Maharjan et al. (2018). We changed Figure 2 to better show this. We also clarified in the text that “Second, we identified from an independent regional GLOF inventory (Veh et al. 2019) 31 lakes that had at least one outburst between 1981 and 2017 and that are listed in the ICIMOD inventory.” (L155-157).

Detail comment #21:

Fig. 3: how about green color in Many Models part?

We modified our figure to clarify this and also changed the colour scheme (as requested by referee #2) for improved contrasts.

Detail comment #22:

Fig. 10: please consider highlighting GLOF-producing lakes; switch a-d in the panel (e)

We modified the figure accordingly.

Reply to Referee #2 (RC2)

General comment #1:

> *Hazard concept: The article is strictly focusing on GLOF susceptibility and using this term consistently throughout the manuscript. Nevertheless, I think regarding some aspects of the study, concepts and terminologies are mixed at some places. According to international standards from UNISDR, IPCC etc., hazard is a function of probability (of occurrence) and intensity (or magnitude). Susceptibility in turn 'is a relative measure of the likelihood (or probability) that a hazard will occur or initiate from a given site, based on intrinsic properties and dynamic characteristics of that site' and 'has an inverse relationship with stability' (GAPHAZ, 2017). I.e., susceptibility can be considered as probability of occurrence and is one factor of hazard. It is determined by conditioning factors (=inherent and more or less static factors) on the one hand, and triggering factors (=factors that directly initiate an outburst) on the other. The factors (predictors) analyzed in this paper, are limited (for good reasons!) to conditioning factors. In other words, the result of an analyses based on the parameters used in the present study, is mainly a lake stability assessment.*

We thank the reviewer for this observation and these definitions. However, we would have hoped for some guidance as to where exactly we might have mixed concepts and terminologies in this regard. The referee's specific comments do not pick up this issue as promised. Perhaps some of the confusion arises from both qualitative and quantitative uses of the term "hazard". We echo the reviewer's comments on hazard and susceptibility in principle. Yet we wish to stress that our model is far from a stability assessment of moraine dams. The reviewer may agree that such appraisals frequently hinge on hard classes such as "stable" or "unstable". Even if such a geotechnical or engineering geological appraisal would be feasible, it would require data on the internal structure and geometry of moraine dams, including grain-size distribution, presence and size of ice cores, the volume, width, height, and slope of moraine dams, pore water pressure in the dam, armouring of the outlet channel, presence and opening of tension cracks, rates of subsidence, and many others. Such parameters have been likely prone to change during our study period and are difficult to obtain for a single lake, and so even less feasible for the size of our regional study. To avoid an elusive feeling of stability, and hence, safety, we refrain to call our approach a dam-stability assessment.
Strictly speaking, our analysis estimates the **probability of correctly detecting historic lake outbursts from a set of predictors**. The referee may acknowledge that this probability is indeed a likelihood of GLOF outburst conditioned on reporting. In this sense one could see this metric as a "relative measure of the likelihood (or probability) that a hazard will occur or initiate from a given site" as the referee suggests. Our forecasting model in particular addresses this scenario. To further stress this point, we added the following definitions and statements to our introduction: "Our method estimates the probability of correctly detecting historic GLOFs from a set of predictors, which act as proxies subsuming various physical processes described as being relevant to GLOFs. Triggering mechanisms of these GLOFs are rarely reported, however. Thus, we discuss what we can learn more about how these historic GLOFs were linked to readily available measures of topography, monsoonality, and glaciological changes. Our model results provide a posterior probability of outburst conditioned on detection, and this may be used as a relative metric of GLOF release from a given lake. Therefore, our approach is an alternative to a formal assessment of moraine-dam stability, which is (geo)technically feasible only at selected sites and at scales much finer than our regional and decadal focus." (L75-81).

In contrast to this, most of the mentioned regional glacial lake assessment approaches with more expert based, and probably subjective, parameter weightings, follow a hazard assessment approach, rather than a stability/susceptibility assessment.

From the literature that we compiled in this and our previous work on GLOFs, we infer that very few, if any, of these so-called hazard assessments offer probabilistic metrics that satisfy the formal quantitative definition of hazard, as the referee clearly points out. During our review, we found that most of these studies deal with hazard in a qualitative or semi-quantitative way. We now acknowledge this in our manuscript: "Specifically, we tested how well some of the more widely used predictors of GLOF susceptibility and hazard fare in a multi-level logistic regression that is informed more by data rather than by expert opinion." (L72-73).

The reasons why these other studies consider factors like lake area or volume, or regional glacier mass balance, is not mainly because these factors directly influence lake stability, but because they have an impact on hazard potentials. Larger lake volumes (area is often used as a proxy for volume) and lake growth imply higher potential flood volumes, and therefore increase the hazard due to higher intensities, without affecting GLOF susceptibility. For similar reasons glacier masse balance is included in such models: Negative regional mass balances lead to glacier retreat and the formation of new and growth of existing lakes. Both processes increase the GLOF hazard potential in a region, but only have minor effects on GLOF susceptibility of individual lakes.

We agree with the referee in principle here. Yet we found it difficult to trace objectively any increases (or even changes) in hazard in the literature due a distinct lack of the necessary probabilistic metrics.

Further, unfavorable conditioning factors do not lead to a lake outburst immediately. It of course increases GLOF susceptibility, but requires still a triggering event to initiate an outburst. Clague and Evans (2000) and Emmer et al. (2020) present concepts about the timing of the causal chain of climate change, glacier retreat, glacial lake formation, and glacial lake outburst and conclude, based on empirical data from British Columbia and the Cordillera Blanca, that there is a lag between lake formation and outburst of up to several decades. The fact that a lake did not have an outburst event in the periods investigated in this study, does not automatically imply that the lake has a low GLOF susceptibility. It is indeed possible, that the lake is actually unstable (i.e. has a high susceptibility) but an outburst simply has not been triggered (yet).

We reiterate our point above and state that our method is set out to detect reported GLOFs, as now further stressed in the added introductory definitions in L75-81. The referee's comment on dam stability is important and should be considered in geotechnical assessments, but is tangential to our objectives. Nowhere did we state that we wanted to quantify or estimate the stability of moraine dams. We also do acknowledge the concept of lag times between lake formation and outburst: every lake has a life span, but the question is whether an outburst needs to end it. This concept of lag time is thought provoking, but hinges on data similar to the models that we present here. One major advantage in our models is that we can fully capture the underlying uncertainties, something that we have so far yet to see for any lag-time model.

General comment #2:

Used data and parameters: Data availability for the entire study region is of course an important criterion for the selection of predicting parameters. But in addition to the parameters investigated in this study, there are candidates for other parameters which are often and successfully applied in other regional assessment approaches cited in the study, such as the Steep Lake front Area (SLA) developed by Fujita et al. (2013) and used by Rounce et al. (2016), or the topographic potential for rock or ice avalanches (cf. Allen et al., 2019), one of the most frequent GLOF triggers in High Mountain Asia. Considering this, I suggest to include more details about the selection of the predicting parameters.

We wish to refer the reviewer to the changes that we have made following the suggestions of referee #1. We rewrote large parts of Section 2.1 to make clear how we selected each predictor. For example, we note that:
- "Larger and growing lakes offer more area for impacts from mass flows such as avalanches, rockfalls, and landslides originating from adjacent valley slopes (Haeberli et al., 2017)." (L123-124);
- "A larger upstream catchment area has been associated with an increased susceptibility to GLOFs as more runoff from intense precipitation, together with glacier and snow melt, can lead to sudden increases in lake volume (Allen et al., 2019; GAPHAZ, 2017)." (L126-128);
- "These readily available data on regional glacier-mass balances are proxies for other, less accessible, physical controls on GLOF susceptibility such as glacial meltwater input, either directly from the parent glacier or from glaciers upstream, as well as permafrost decay in slopes fringing the lake." (L133-136);
- "Meteorological drivers entered previous qualitative GLOF hazard appraisals mostly as (the probability of) extreme monsoonal precipitation events: the Kedarnath GLOF disaster, for example, was triggered by intense surface runoff (Huggel et al., 2004; Prakash and Nagarajan, 2017). Heavy rainfall may also trigger landslides or debris flows from adjacent hillslopes followed by displacement waves that overtop moraine dams (Huggel et al., 2004; Prakash and Nagarajan, 2017). Elevated lake levels during the monsoon season also raise the hydrostatic pressure acting onto moraine dams (Richardson and Reynolds, 2000). Furthermore, different precipitation regimes and climatic preconditions may also influence moraine dam failure mechanics (Wang et al., 2012)." (L137-143)

Fujita et al. (2013) used the SLA approach to derive Potential Flood Volumes (PFVs) of Himalayan lakes as a proxy for GLOF susceptibility. We assume that PFVs are largely represented by our predictor lake area, given that larger lakes should produce larger floods. A major critique of the SLA concept is that lakes can have zero PFV despite large lake volumes. This issue was observed, for example, at Imja Lake in the Mt. Everest region, Nepal, that stores 78.4×10^6 m^3 of water surrounded by steep slopes (Haritashya et al., 2018). Fujita et al. (2013) also point towards an issue that the reviewer had cautioned against above (p. 1834): *"PFVs were simply calculated from the topography surrounding the moraine-dammed lakes and thus the robustness of the dam could not be evaluated. As the existence of ice within the damming moraine may alter the dam's vulnerability, understanding the distribution and degradation of permafrost will be an important factor for the further assessment of GLOF probability".* Furthermore, SLA depends on user-defined cutoffs, for example a 1-km search radius for steep slopes around lakes (Fujita et al., 2013), or a minimum slope threshold (Rounce et al., 2016). Such thresholds introduce additional subjective bias that we wished to avoid in our appraisal. Finally, errors in digital elevation models in high mountains remain unaccounted for in these slope-based metrics, though have been acknowledged for many years (Fujita et al., 2008). For example, Mukul et al. (2017) reported that *"vertical accuracy of the data decreases with increase in slope and elevation due to presence of large outliers and voids. Therefore, studies* *using SRTM data "as is", especially in regions like the Himalaya, are not statistically meaningful".* In summary, these findings motivated us to keep the influence of potentially error-prone model inputs at a minimum.

> *Then, the influence of overlapping time periods of the different data sets used should be discussed in more detail, as also mentioned in the review of A. Emmer (Emmer, 2021). In particular the fact that the lake area change period overlaps the period which is investigated for GLOF occurrence, in my view disqualifies this parameter to be considered, as actually discussed in L340-344.*

We again wish to refer the referee to our reply to referee #1:

The data on lake-area changes are not yet resolved on an annual basis for our study area, so that we had to resort to changes averaged over longer periods. However, we used our forecasting model to test whether changes in lake size between two observation periods had a credible effect on P_{GLOF}. Here we explored the weight of relative changes in lake area between 1990 and 2005 to estimate the probability of observing GLOFs that happened in the subsequent period 2005-2018. In other words, we trained the model on GLOF data that **pre-date the testing data**, and thus offer a realistic and rigorous prediction and validation scenario. We reported in our original manuscript that "The weight of relative lake-area change in the 15 years before is ambiguous ($\beta_{A*} = -0.04^{+0.76}/_{-0.67}$) […]" (L245-246 of orig. manuscript) and that "In the forecasting model, however, the influence of lake-area change remains negligible even for <50% HDIs." (L350 of orig. manuscript). This indicates with 95% probability that relative lake-area change **before** the outburst is an inconclusive predictor.

To further stress this result, we added the following sentences in the Discussion: "However, in the forecasting model, in which we tested whether differing data observation periods have any credible effects, the influence of lake-area change remains negligible even for <50% HDIs. We thus conclude that relative lake-area change before outburst is an inconclusive predictor. This result contradicts the assumptions made in many previous studies that argued that rapidly growing lakes are the most prone to sudden outburst (Aggarwal et al., 2016; Bolch et al., 2011; Ives et al., 2010; Mergili and Schneider, 2011; Prakash and Nagarajan, 2017; Rounce et al., 2016; Wang et al., 2012)." (L382-387).

General comment #3:

> *Statistical significance: Bayesian approaches are certainly most suitable for this type of research question where a large number of lakes (3,390) had relatively few (31) GLOF events. But still this is a very limited data basis, in particular since for the Forecasting, the Glacier-mass balance, and the Monsoonality models, only 11 GLOF events were recorded in the relevant 2005 to 2018 period. Even more, these 11 events are split over four to seven groups, depending on elevation, region, or monsoonality. Over the western half of the study region, only 3 GLOFs are found. This leads to very few (often only 1 or 2) or even zero GLOF events per subgroup (cf. boxes for Hindu Kush, Karakoram and Western Himalayas in Fig. 7). I wonder, how any predictor weights can be found in these cases.*

The problem of highly imbalanced data (few GLOF reports out of several thousand lakes) was a major motivation for us to use Bayesian models. The low prior probability of detecting a reported GLOF can be compared directly with the posterior probability, a strategy that we showed in our original Fig. 9. Classical rare-events logistic regression penalises the model likelihood, and this step is done naturally via the prior distributions in the Bayesian setting. The low number of data points in some groups is even less of an issue in a hierarchical model, as this always draws strength across each group and the pooled model of all data taken together. The high posterior uncertainties tied to some model groups clearly underline the effect of fewer data points. To further emphasise these points, we added that: “The small number of reported GLOFs introduces strong imbalance to our data, given that some regions, and hence levels, had few or no reported GLOFs. Although this would be problematic in most other modelling approaches, Bayesian multi-level models are well suited for this kind of imbalanced training data (Gelman and Hill, 2007; Shor et al., 2007; Stegmueller, 2013).“ (L208-211) and in the Discussion that: “In the forecasting model, however, the contributions of lake elevation to P_{GLOF} are devoid of any systematic pattern and likely reflect several, potentially combined, drivers (Fig. 4). This model was trained on fewer GLOFs and the imbalance in the data introduces more uncertainties in terms of broad 95% HDIs.” (L391-393).

> *A very recent study from Zheng et al. (2021) on a slightly larger study region found evidence for a total of 215 GLOF events that presumable have happened since 1900, 176 thereof so far unreported. This does not contradict any of the data used here, but offers at least a potential alternative of a database with much more GLOF evidences (in turn posing challenges on the predictor data of course).*

We checked the study by Zheng et al. (2021) but found mostly GLOFs without timestamps that are difficult to align with our predictors, some of which are averaged over specified time periods. We, thus, decided not to add this source to our study. Please also see our reply to referee #1 with respect to the validity of predictors that change over time.

Detail comment #1:

> *L11: I suggest to include something like ‘regional-scale’ (hazard estimations), because at the level of individual lakes, there are many quantitative assessments available, including numerical modeling, geophysical measurements etc.*

We rephrased this sentence accordingly to: “Estimating regional susceptibility of glacial lakes […].” (L11-13).

Detail comment #2:

> *L21: Maybe change ‘with respect to’ to ‘compared to’?*

We changed the phrasing as requested (L21).

Detail comment #3:

> *L81: Indicate the version number of the RGI*

We added the version number 6.0 (L89).

Detail comment #4:

> *L112: Hydrostatic pressure acting on the dam depends mainly on lake depth, not area.*

We acknowledge that this is physically more correct, though we fail to see how any change in lake area or volume could not affect hydrostatic pressure eventually. We accordingly rephrased our statement to: “Lake area scales with lake volume and depth (Huggel et al., 2002), and growing lake depths increase the hydrostatic pressure acting on moraine dams, thus raising the potential of failure (Rounce et al., 2016)." (L119-121).

Detail comment #5:

> *L263: The statement that upstream catchment area is well correlated with lake area is not clear to me. This needs further explanations of references. Also I do not understand why lake area is replaced by upstream catchment area in these model (Glacier-mass balance and Monsoonality), but not in others. This requires some more explanation.*

We thank the reviewer for this suggestion and added more details on our predictor catchment area. We refer to our replies to referee #1's detail comments #12 and #13:

To clarify our approach, we added more details on our predictor catchment area in the new Table 2 (L147). We explain our choice of this predictor and why we use it instead of the static lake area *A* in the glacier-mass balance and monsoonality models: "We also tested the impact of upstream **catchment area** *C* (m^2) on GLOF susceptibility. A larger upstream catchment area has been associated with an increased susceptibility to GLOFs as runoff from intense precipitation as well as glacier and snow melt can lead to sudden increases in lake volume (Allen et al., 2019; GAPHAZ, 2017)." (L126-128).

We find that catchment area *C* has a strong linear correlation with lake area *A* (Pearson's correlation coefficient of 0.446), such that we preferred *C* over *A* in two of our models, as *C* is invariant at the timescale of our study. We also added this to the text: "We find that catchment area *C* correlates with lake area *A* (Pearson's $\rho = 0.45$) and, thus preferred *C* over *A* in two of our models, as *C* is invariant at the timescale of our study." (L128-130).

Detail comment #6:

> *L285: In the Forecasting and Glacier-mass balance models, A* represents lake-area change between 2005 and 2018. Is A* here also referring to this period (and not 1990 – 2018, as written)? If so, please correct, if not, another symbol should be used (ΔA?).*

Our predictor relative lake-area change A^* (not to be confused with net lake-area change ΔA) is calculated for three different time windows: 1990 to 2005 in the forecasting model, 2005 to 2018 in the glacier-mass balance model, and 1990 to 2018 in the monsoonality model. In order to avoid confusion and correctly refer to each respective time interval of relative lake-area change, we now assigned each with its own superscript: relative lake-area change between 1990 to 2005 is A^{*a}, relative lake-area change between 2005 and 2018 is A^{*b}, and relative lake-area change between 1990 and 2018 is A^{*c}. We explain this notation in our new Table 2 and in the respective model descriptions in the Results section 3 (L269-340).

Detail comment #7:

> *L321/Fig. 9: Why are the log-odds ratios negative for the first (few) lakes? Would be interesting to describe in the text.*

The negative log-odds ratios indicate lakes for which the posterior probability of a reported GLOF is lower than the prior probability. To further clarify this, we rephrased this in the text to: "A positive log-odds ratio means that we obtain a higher posterior probability of attributing a historic GLOF to a given lake compared to a random draw. Negative log-odd ratios indicate lakes for which the posterior probability of a reported GLOF is lower than the prior probability." (L347-349).

Detail comment #8:

L323/324 (Caption Fig. 9): '...in the past four decades' applies only to the lakes in the x-axes of (a) and (c), for the other panels it's 2005-2018. I suggest to replace this with 'in the period 1981 – 2018 (a and c) and 2005 – 2018 (b and d-h). (Or change panel letters, see suggestion below).

We changed panel labelling and added information on used time periods for lake subsets on the x-axis to the figure caption accordingly (L356-361).

Detail comment #9:

Table 1: This is a pretty large table for only presenting the 6 predictor parameter selected for this study. I suggest to present the 6 parameters used here in separate table, giving some more details as well. (By the way, I think dam type could be ticked as well, at least a tick in brackets. As only moraine-dammed lakes are investigated here, this criteria is inherently considered). If the authors wish to keep having a table with other potentially relevant parameters for GLOF hazard assessment, this could be done in a more compressed format. But in this case, further geotechnical and geomorphic parameters would need to be included, such as permafrost conditions, lithology, seismicity, etc. The annex tables of the GAPHAZ guidelines (GAPHAZ, 2017) might give some indications for this.

To meet the requests from both referees, we split our former Table 1 into two separate tables with an overview table listing the predictors for HKKHN lakes described in the literature (now labelled as Table 1, L83) and a comprehensive table listing our predictor choices (now labelled as Table 2, L147). We added a number of additional parameters to Table 1 and more details (used notation and selection reasoning) to Table 2. Now the indicator "dam type" is also ticked in Table 1.

Detail comment #10:

Fig. 1: According to the caption, white triangles represent GLOFs since 1935. But as the study only deals with GLOFs that have occurred on the periods 1981-2018 and 2005-2018, respectively, only these should be shown here. Preferably with two colors, one for 1981-2005 and another for 2005-2018 to discriminate these to reference data sets. Please also indicate the spacing of the lake bubbles.

We thank the reviewer for this suggestion and changed the figure accordingly.

Detail comment #11:

Fig. 3: Blue-green combinations are hardly readable in the bubbles. I can see it in the text, slightly see it in the middle ('mulit-level') bubble, but do not see any green in the right ('many models'). Colors to be adjusted.

We modified Fig. 3 to make the principles of multi-level modelling more clear. We also chose a green-purple-yellow colour combination to improve contrast.

Detail comment #12:

Figure 4: I suggest to sort sub-groups from highest (top) to lowest (bottom) in (a) and (b), West to East (or East-West) in (c) and highest monsoonality on top to lowest monsoonality in (d). (Same as ordering in Figs. 5-8).

We appreciate this suggestion. However, the point of the figure is to highlight better the ranked deviations of the group-level coefficients from the pooled mean (at the bottom of the stack). We believe that this ranking allows a better visual assessment of which groups deviate most from the pooled means and, thus, made no changes to Figure 4.

Detail comment #13:

> *Figs. 5-8 (general): In none of the figures I can see the middle (blueish) line. I only see the purple and orange lines. Similar for the color shades, I guess I only see the purple and the orange and the overlap of the two. Is this middle line represented in the Figures? If so, please adjust coloring, if not please add (or remove from the legend). For all figures it would be very nice to also have a panel for the pooled data, similar to Fig. 4.*

The middle line is grey in the original Figs. 5-8 and lies between the orange and purple lines. We acknowledge that this may be hard to decipher and changed the colour scheme to provide more contrast. Adding a panel for pooled data is a good suggestion and we altered the figures as requested.

Detail comment #14:

> *Figs. 5 and 6: It would be helpful to indicated elevation bands in m a.s.l.*

This is a good suggestion and we modified the figures as requested.

Detail comment #15:

> *Fig. 9: To me it would make more sense to number the TP a-d and the TN e-h.*

We thank the reviewer for this suggestion and changed the figure accordingly.

Detail comment #16:

> *Fig. 10: In the legend (e) (the letter e is not needed in my view) swap ordering, that a is on top and d at the bottom, as in the main panels. Ad '%' to the numbers at the bottom. In the panels it would be helpful to include the locations with a recorded GLOF (for 1990-2018 in (a) and 2005-2018 in (b), (c) and (d)).*

We thank the reviewer for this suggestion and changed the figure accordingly.

References cited in this Author's Reply

Aggarwal, A., Jain, S. K., Lohani, A. K. and Jain, N.: Glacial lake outburst flood risk assessment using combined approaches of remote sensing, GIS and dam break modelling, Geomatics, Nat. Hazards Risk, 7(1), 18–36, doi:10.1080/19475705.2013.862573, 2016.

Allen, S. K., Rastner, P., Arora, M., Huggel, C. and Stoffel, M.: Lake outburst and debris flow disaster at Kedarnath, June 2013: hydrometeorological triggering and topographic predisposition, Landslides, 13(6), 1479–1491, doi:10.1007/s10346-015-0584-3, 2016.

Allen, S. K., Zhang, G., Wang, W., Yao, T. and Bolch, T.: Potentially dangerous glacial lakes across the Tibetan Plateau revealed using a large-scale automated assessment approach, Sci. Bull., (April), doi:10.1016/j.scib.2019.03.011, 2019.

Bolch, T., Peters, J., Yegorov, A., Pradhan, B., Buchroithner, M. and Blagoveshchensky, V.: Identification of potentially dangerous glacial lakes in the northern Tien Shan, Nat. Hazards, 59(3), 1691–1714, doi:10.1007/s11069-011-9860-2, 2011.

Bolch, T., Kulkarni, A., Kääb, A., Huggel, C., Paul, F., Cogley, J. G., Frey, H., Kargel, J. S., Fujita, K., Scheel, M., Bajracharya, S. and Stoffel, M.: The state and fate of himalayan glaciers, Science (80-.)., 336(6079), 310–314, doi:10.1126/science.1215828, 2012.

Bolch, T., Shea, J. M., Liu, S., Azam, F. M., Gao, Y., Gruber, S., Immerzeel, W. W., Kulkarni, A., Li, H., Tahir, A. A., Zhang, G. and Zhang, Y.: Status and Change of the Cryosphere in the Extended Hindu Kush Himalaya Region, in The Hindu Kush Himalaya Assessment: Mountains, Climate Change, Sustainability and People, edited by P. Wester, A. Mishra, A. Mukherji, and A. B. Shrestha, pp. 209–255, Springer International Publishing, Cham., 2019.

Bookhagen, B. and Burbank, D. W.: Toward a complete Himalayan hydrological budget: Spatiotemporal distribution of snowmelt and rainfall and their impact on river discharge, J. Geophys. Res. Earth Surf., 115(3), 1–25, doi:10.1029/2009JF001426, 2010.

Fujita, K., Suzuki, R., Nuimura, T. and Sakai, A.: Performance of ASTER and SRTM DEMs, and their potential for assessing glacial lakes in the Lunana region, Bhutan Himalaya, J. Glaciol., 54(185), 220–228, doi:10.3189/002214308784886162, 2008.

Fujita, K., Sakai, A., Takenaka, S., Nuimura, T., Surazakov, A. B., Sawagaki, T. and Yamanokuchi, T.: Potential flood volume of Himalayan glacial lakes, Nat. Hazards Earth Syst. Sci., 13(7), 1827–1839, doi:10.5194/nhess-13-1827-2013, 2013.

GAPHAZ: Assessment of Glacier and Permafrost Hazards in Mountain Regions: technical Guidance Document. Standing Group on Glacier and Permafrost Hazards in Mountains (GAPHAZ) of the International Association of Cryospheric Sciences (IACS) and the International Per, Zurich, Lima., 2017.

Gelman, A. and Hill, J.: Data Analysis using Regression and Multilevel/Hierarchical Models, Cambridge University Press, New York., 2007.

Haeberli, W., Schaub, Y. and Huggel, C.: Increasing risks related to landslides from degrading permafrost into new lakes in de-glaciating mountain ranges, Geomorphology, 293, 405–417, doi:https://doi.org/10.1016/j.geomorph.2016.02.009, 2017.

Haritashya, U. K., Kargel, J. S., Shugar, D. H., Leonard, G. J., Strattman, K., Watson, C. S., Shean, D., Harrison, S., Mandli, K. T. and Regmi, D.: Evolution and Controls of Large Glacial Lakes in the Nepal Himalaya, Remote Sens. , 10(5), doi:10.3390/rs10050798, 2018.

Huggel, C., Kääb, A., Haeberli, W., Teysseire, P. and Paul, F.: Remote sensing based assessment of hazards from glacier lake outbursts: a case study in the Swiss Alps, Can. Geotech. J., 39(2), 316–330, doi:10.1139/t01-099, 2002.

Huggel, C., Haeberli, W., Kääb, A., Bieri, D. and Richardson, S.: An assessment procedure for glacial hazards in the Swiss Alps, Can. Geotech. J., 41(6), 1068–1083, doi:10.1139/T04-053, 2004.

Ives, J. D., Shrestha, R. B. and Mool, P. K.: Formation of Glacial Lakes in the Hindu Kush-Himalayas and GLOF Risk Assessment, International Centre for Integrated Mountain Development (ICIMOD), Kathmandu., 2010.

Kargel, J. S., Leonard, G. J., Shugar, D. H., Haritashya, U. K., Bevington, A. and Fielding, E. J.: Geomorphic and geologic controls of geohazards induced by Nepal's 2015 Gorkha earthquake., Science, 3(1), 1–10, doi:10.1126/science.aac8353, 2015.

Liu, J. J., Cheng, Z. L. and Su, P. C.: The relationship between air temperature fluctuation and Glacial Lake Outburst Floods in Tibet, China, Quat. Int., 321, 78–87, doi:10.1016/j.quaint.2013.11.023, 2014.

Maharjan, S. B., Mool, P. K., Lizong, W., Xiao, G., Shrestha, F., Shrestha, R. B., Khanal, N. R., Bajracharya, S. R., Joshi, S., Shai, S. and Baral, P.: The Status of Glacial Lakes in the Hindu Kush Himalaya, International Centre for Integrated Mountain Development (ICIMOD), Kathmandu., 2018.

Mergili, M. and Schneider, J. F.: Regional-scale analysis of lake outburst hazards in the southwestern Pamir, Tajikistan, based on remote sensing and GIS, Nat. Hazards Earth Syst. Sci., 11(5), 1447–1462, doi:10.5194/nhess-11-1447-2011, 2011.

Mukul, M., Srivastava, V., Jade, S. and Mukul, M.: Uncertainties in the Shuttle Radar Topography Mission (SRTM) Heights: Insights from the Indian Himalaya and Peninsula, Sci. Rep., 7(February), 1–10, doi:10.1038/srep41672, 2017.

Nie, Y., Liu, Q., Wang, J., Zhang, Y., Sheng, Y. and Liu, S.: An inventory of historical glacial lake outburst floods in the Himalayas based on remote sensing observations and geomorphological analysis, Geomorphology, 308(December), 91–106, doi:10.1016/j.geomorph.2018.02.002, 2018.

Prakash, C. and Nagarajan, R.: Outburst susceptibility assessment of moraine-dammed lakes in Western Himalaya using an analytic hierarchy process, Earth Surf. Process. Landforms, 42(14), 2306–2321, doi:10.1002/esp.4185, 2017.

Rounce, D. R., McKinney, D. C., Lala, J. M., Byers, A. C. and Watson, C. S.: A new remote hazard and risk assessment framework for glacial lakes in the Nepal Himalaya, Hydrol. Earth Syst. Sci., 20(9), 3455–3475, doi:10.5194/hess-20-3455-2016, 2016.

Shor, B., Bafumi, J., Keele, L. and Park, D.: A Bayesian multilevel modeling approach to time-series cross-sectional data, Polit. Anal., 15(2), 165–181, doi:10.1093/pan/mpm006, 2007.

Stegmueller, D.: How many countries for multilevel modeling? A comparison of frequentist and bayesian approaches, Am. J. Pol. Sci., 57(3), 748–761, doi:10.1111/ajps.12001, 2013.

Veh, G., Korup, O., Specht, S., Roessner, S. and Walz, A.: Unchanged frequency of moraine-dammed glacial lake outburst floods in the Himalaya, Nat. Clim. Chang., 2000, 1–5, doi:10.1038/s41558-019-0437-5, 2019.

Wang, X., Liu, S., Ding, Y., Guo, W., Jiang, Z., Lin, J. and Han, Y.: An approach for estimating the breach probabilities of moraine-dammed lakes in the Chinese Himalayas using remote-sensing data, Nat. Hazards Earth Syst. Sci., 12(10), 3109–3122, doi:10.5194/nhess-12-3109-2012, 2012.

Worni, R., Stoffel, M., Huggel, C., Volz, C., Casteller, A. and Luckman, B.: Analysis and dynamic modeling of a moraine failure and glacier lake outburst flood at Ventisquero Negro, Patagonian Andes (Argentina), J. Hydrol., 444–445, 134–145, doi:10.1016/j.jhydrol.2012.04.013, 2012.

Zheng, G., Bao, A., Allen, S., Ballesteros-Cánovas, J. A., Yuan, Y., Jiapaer, G. and Stoffel, M.: Numerous unreported glacial lake outburst floods in the Third Pole revealed by high-resolution satellite data and geomorphological evidence, Sci. Bull., 2021.