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Interactive comment on "Controls of outbursts of moraine-dammed lakes in the greater Himalayan region" *by* Melanie Fischer et al.

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Please see the supplementary PDF-document for a formatted version (incl. Table R1) of this Reply Letter.

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 identiïňĄcation 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.

Reply General comment #1:

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 originating from adjacent valley slopes such as avalanches, rockfalls, and landslides (Haeberli et al., 2017)."; - "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; Worni et al., 2012). "; "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."; "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). [. . .] Elevated lake levels during the monsoon season also raise the hydrostatic pressure acting onto moraine dams (Richardson and Reynolds, 2000)." 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)." 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 an 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"; 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", 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 speciïňĄc combination of values of GLOF indicators infer about possible (likely) GLOF trigger and mechanism (if not known)?

Reply General comment #2:

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. How well these proxies can describe the presence or absence of a GLOF is what our probabilistic models. 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. Still, 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):

Reply General comment #3:

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

General comment #3 (cont.):

Lake area change: I'm aware this indicator is always tricky to deïňĄne 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.

Reply General comment #3 (cont.):

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 PGLOF. 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 (βA* = –0.04+0.76/–0.67) [. . .]" (L245-246) and that "In the forecasting model, however, the influence of lake-area change remains negligible even for <50% HDIs." (L350). 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 assumed 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)."

General comment #3 (cont.):

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?

Reply General comment #3 (cont.):

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 outbursts after 2000, and hence that overlap with the study period of Brun et al. (2017). Table R1, which is included in the PDF-version of this Reply Letter, shows the output from this model. We find that: - The parameter estimates at the population level changed only marginally: the weight of catchment area (βC) remains credibly positive and that of lake-area change from 2005 to 2018 (β_(Aˆ(*b))) remains credibly negative. - At the group level, the standard deviation of intercepts of our grouping variable elevation (σz) is also comparable to our previous results. - Posterior estimates of σr, 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 PGLOF. We now highlight these findings: "On the basis of higher standard deviations, we learn that effects of glaciological regions vary more than those of elevation bands (σr = 0.81+1.60/–0.78 and σz = 0.48+1.19/–0.47). When training this model on a subset of glacial lakes with documented GLOFs post-2000 (i.e. including only GLOFs which occurred in the time period covered by our glacier-mass balance data), posterior estimates of σr increase to 1.11+1.77/–1.03, further underlining our result that glacier-mass balance credibly affects PGLOF."

General comment #3 (cont.):

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.

Reply General comment #3 (cont.):

We are unsure about whether the reviewer offers an opinion here or whether this 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 occurred 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.

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

Reply Detail comment #1:

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."

Detail comment #2:

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

Reply Detail comment #2:

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

Detail comment #3:

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

Reply Detail comment #3:

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

Reply Detail comment #4:

We decided to consistently use the term "predictor", in line with the common terminology in multivariate statistics. 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”. 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

Reply Detail comment #5:

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).“

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)

Reply Detail comment #6:

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

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

Reply Detail comment #7:

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

Reply Detail comment #8:

We moved and slightly rephrased this text passage accordingly.

Detail comment #9:

L166: delete ‘s’

Reply Detail comment #9:

Deleted accordingly.

Detail comment #10:

L176: delete ‘,’

Reply Detail comment #10:

Deleted accordingly.

Detail comment #11:

L207: what is meant by ‘common susceptibility’?

Reply Detail comment #11:

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."

Detail comment #12:

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

Reply Detail comment #12:

To clarify our approach, we added more details on our predictor catchment area in the new Table 2. 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)."

Detail comment #13:

L263: please provide details about this correlation

Reply Detail comment #13:

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 constant at the scale of our study.

Detail comment #14:

L272: what is meant by 'average lake'?

Reply Detail comment #14:

The average lake is defined by the combination of all average predictor values.

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)

Reply Detail comment #15:

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.

Detail comment #16:

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

Reply Detail comment #16:

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 glacier-lake areas."

Detail comment #17:

L373: are 'minute'? Please check

Reply Detail comment #17:

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

Detail comment #18:

Tab 2: what is PDF?

Reply Detail comment #18:

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

Detail comment #19:

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

Reply Detail comment #19:

We accordingly added false positives and false negatives in a separate column to the table.

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?

Reply Detail comment #20:

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 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 are listed in the ICIMOD inventory."

Detail comment #21:

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

Reply Detail comment #21:

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)

Reply Detail comment #22:

We modified the figure accordingly. âĂČ

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Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2020-327/tc-2020-327-AC1-supplement.pdf

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