Response to Referee Report #2:

Minor comment #1:

> *My first general comment in the initial review was referring to the terminologies around hazard and susceptibility. Related adjustments in the new manuscript regarding this point are good and helpful, namely the consistent renaming of what was initially called "GLOF hazard parameters" or "diagnostics of GLOF potential" to now "predictors of GLOF susceptibility" is a clear improvement. The only point where I am not yet convinced is the statement in the conclusions (L481-482): "In any case, the widely adapted notion that (rapid) lake growth may be a predictor of impending outburst remains poorly supported by our model results". In my view lake growth is not mainly considered as an outburst predictor in these assessments (i.e. a proxy for a high outburst susceptibility), but rather as a predictor of increased downstream hazard potential due to increased potential flood volume and thus hazard magnitude. I might be wrong, but since this statement is also used in the abstract, I would ask the authors to more clearly elaborate from the cited publications about these assessment schemes, if any why lake area changes are really considered as predictors of lake outburst susceptibility (rather than indicators of increased potential flood intensities downstream). This could be done, for instance, around L122 where it now says "and many studies have emphasised the role of growing lakes on GLOF susceptibility (Bolch et al., 2011; Prakash and Nagarajan, 2017; Rounce et al., 2016)".*

We thank the reviewer for giving us the opportunity to elaborate more on the roles of lake area and lake area growth in previous appraisals on GLOF susceptibility. We agree that some studies included these predictors to estimate GLOF hazard and risk downstream. However, other studies emphasize that a larger lake area could make lakes more prone both to external triggers, such as mass flows entering the lake, and to internal triggers, such as an increasing hydrostatic pressure on the moraine dam. For example, GAPHAZ (2017) reported that "large lakes obviously can produce potentially greater flood magnitudes, but larger lakes also are more susceptible to impacts from rock and ice." Rounce et al. (2016) noted that "lake growth is crucial to incorporate into hazard assessments as the expansion of a glacial lake may greatly alter the lake's proximity to potential hazards and increase the volume of water likely released in a GLOF." Prakash and Nagarajan (2017) argued that "Characteristics of the lake and its mother glacier, such as the area of the lake, […] are important factors affecting the outburst probability of a glacial lake (McKillop and Clague, 2007; Bolch et al., 2011)." and that "large expanding lakes are more susceptible to outburst, as the greater surface area is exposed to potential mass movement impact". Iribarren Anacona et al. (2014) reported that "lake dimensions have been directly related to outburst volume, peak discharge and flood damage potential (Costa and Schuster, 1988). Accordingly, larger lakes are considered to be more hazardous than small lakes. Furthermore, lakes with larger areas are generally deeper (see e.g. the database by Diaz et al., 2007), and may exert higher hydrostatic pressures over the dams making them more susceptible to failure (Richardson and Reynolds, 2000b). Larger lakes also have a greater surface area potentially exposed to mass movement and ice avalanche impacts, increasing their outburst susceptibility." Finally, Mergili and Schneider (2011) directly used "lake area development" as one of their four key parameters in their qualitative rating for the susceptibility to lake outburst by internal forces (dam failure).

We agree with the referee that the citation of Bolch et al. (2011) within this context is less suitable in L122 and therefore removed it. We now rewrote this text passage as follows (LL122-132):

“Since 1990, lake areas have grown largest in the Central Himalayas (+23%), and lowest in the northwestern Himalayas (+5.0%) (Nie et al., 2017), and many studies have emphasised the role of growing lakes on GLOF susceptibility (e.g. GAPHAZ, 2017; Prakash and Nagarajan, 2017; Rounce et al., 2016). Many previous GLOF assessment schemes included lake area or lake area growth as a proxy for the volume of water that could be potentially released by an outburst and, thus, the resulting downstream hazard (e.g. Allen et al., 2019; Bolch et al., 2011). However, a number of studies also stress that lake area and its growth define the exposure to external and internal triggers of moraine dam breach: larger and growing lakes offer more area for impacts from mass flows such as avalanches, rockfalls, and landslides originating from adjacent valley slopes (GAPHAZ, 2017; Haeberli et al., 2017; Prakash and Nagarajan, 2017; Rounce et al., 2016). Some authors also link growing lake areas to an increase in hydrostatic pressure acting on its moraine dam, thus, making the letter more susceptible to sudden failure (Iribarren Anacona et al., 2014; Mergili and Schneider, 2011).”

Minor comment #2:

> *The second aspect where I am not yet 100% convinced is the point on using catchment area instead of lake area. The authors provide convincing arguments in their response letter and include this also in the revised manuscript (catchment area representing the potential of intense rainfall runoff and snow melt, as well as being invariant at the investigated time scales). But then I wonder why catchment area has not been used in all the models instead of lake area. Or why is this aspect more important for the glacier-mass balance model and the monsoonality model, an less for the elevation dependent and the forecasting model? In my view, a few more words on that would be needed, probably in the paragraph ending on L130.*

From a physical process point of view, we argue that catchment area is more suitable in our glacier-mass balance and monsoonality models than glacial lake area. We anticipate that larger catchments can collect more run-off from glacier melt or heavy monsoon precipitation. The choice of this predictor is supported by Allen et al. (2019) who noted that the “total watershed area upstream of the lake [*recognises*] the potential for runoff from heavy rainfall or glacier/snow melt to drain into and overwhelm a glacial lake [4,41].” We now address this issue in our revised manuscript (LL135-138): “We find that catchment area *C* correlates with lake area A (Pearson’s ρ = 0.45) and we, thus, preferred *C* over *A* in two of our models, as *C* is invariant at the timescale of our study and we use these two models to explicitly test whether runoff by glacier melt or monsoonal precipitation had an effect on GLOFs in our study area.” We reiterate our point that the strong correlation between the static predictors glacial lake area and catchment area in our database makes them, from a data-driven point of view, interchangeable in our models. One motivation for our study was to test and present alternative model variants, and the replacement of glacial lake area by catchment area may be useful in areas with limited data on either predictor.

Minor comment #3:

> *Then also the reasoning of selecting “lake-area change” as a predictor (Table 2) as well as the response to my detailed comment #4 (lake depth, not area is determining the hydrostatic pressure on the dam) do not convince me yet. Most glacial lakes, once they have a certain size, mainly increase their area (and thus volume) but not their depth while the grow further. It is true that there are depth and volume estimation rules based on lake area, but the uncertainties of these approaches are*

huge, and they are based on empirical relationships (of static conditions) and do not involve any dynamic considerations, not any physical processes. If I think of any larger moraine dammed lake in the Himalayas that is growing due to glacier retreat at its upstream end (and thus increasing its area and volume), I would not expect a significant increase of lake depth. L119-121 and the reasoning in Table 2 should be reconsidered in my view.

We find it difficult to comment on the reviewer's remark that a glacial lake requires a "certain size" to chiefly increase its area instead of depth. We are unsure whether this is a hypothesis or a result from a published study. The reviewer may concede, though, that reliable field data on lake-depth changes are few, hence our choice of a data-driven probabilistic model instead of a physical one. We do acknowledge that empirical relationships between lake area, depth, and volume have high noise (depending on model choice). Yet some of this noise arises from the different stages of lake development (or age) that the data are based on. Hence the dynamics of lake growth are implicit in these empirical relationships, contrary to what the reviewer suggests. In this context, we recall the purpose of a statistical predictor: we use changes in lake area because we lack detailed measurements of changes in lake depth at the scale of our study. Our objective is to offer probabilistic estimates of a GLOF history based on these predictors instead of deriving a physical-based model that ties lake-depth changes to past outbursts.

We point out that a number of studies have stressed that the relationship between lake area, lake volume, and lake depth might be assumed for a majority of glacial lakes and have subsequently used this approach (Huggel et al., 2002; Iribarren Anacona et al., 2014; Mergili et al., 2011; Prakash and Nagarajan, 2017). These studies are – like our own assessment – predominantly remote-sensing-based and are, due to the lack of bathymetric data on a regional scale, limited to the more readily available metric of lake area. To our knowledge, (measured) lake depth has not been applied in any GLOF assessment of comparable scale in the greater Himalayan region. Our objective is to test whether commonly applied predictors of GLOF susceptibility are credibly informative in this regard, and glacial lake area is indeed so. In other words, we can learn more from the data by including this metric. In reflection to this comment, we rephrased this in LL119-122 as followed: "Due to a general lack in available bathymetric data on a regional scale, a number of studies used the frequently observed phenomenon that lake area scales with lake volume and depth (Huggel et al., 2002; Iribarren Anacona et al., 2014). Growing lake depths increase the hydrostatic pressure acting on moraine dams, thus raising the potential of failure (Iribarren Anacona et al., 2014; Rounce et al., 2016)." We also now list the bullet point "increasing lake area commonly reported as scaling with increasing lake depth" in our Table 2.

Minor comment #4:

Finally two more details on Table 1:
In the new Table 1, the predictors "glacial lake area" "lake-area change" should be moved from "lake characteristics and dynamics" to the "potential triggering mechanisms" group in my view. As the authors mention in the text and the response, lake area (increase) is a proxy for (increased) probability of the lake being impacted by an upstream mass movement.

In our reply to Minor Comment #1 and in our revised manuscript, we argue that glacial lake area and its change may affect the susceptibility both to external and internal GLOF triggering mechanisms. In Table 1, the group "potential triggering mechanisms (geomorphic)" predominantly focusses on external triggering mechanisms, so that we prefer to keep these in their dedicated “lake characteristics and dynamics” group.

Minor comment #5:

> *Further, I think it is very unfortunate no reference is given for the “summer precipitation” predictor (last line in Table 1). Not that I could make a good suggestion, but having no reference here contradicts the study description (L72-73) “[…]we tested how well some of the more widely adopted predictors of GLOF susceptibility and hazard fare in a multi-level logistic regression […]”. Either some reference(s) should be added to this last line in Tab. 1, or a statement on the summer precipitation predictor should be made around this statement in the introduction.*

Liu et al. (2014) studied the relationship between air temperature and GLOFs on the Tibetan Plateau and emphasize that warm and moist conditions during the Asian summer monsoon may have played a role in historic GLOFs in the Himalayas. Wang et al., (2012) considered the “climatic predisposition” of glacial lakes as categorical value in their assessment of moraine-dam breach probabilities in the Chinese Himalayas. Using this variable, they defined a category “warm-wet”, derived from high annual precipitation and high daily average temperatures during the summer months at a given lake location. In their assessment of breach probabilities for the Longbasaba and Pida glacial lakes, Wang et al. (2008) also assessed the climatic setting of lakes. They defined the “combination of high temperature–wetness” as one of the “value(s) of most likely breaching” in their assessment. In both of these studies these “warm-wet” and “high temperature-wetness” values were used as indicators of the effects of summer monsoon. However, both studies used meteorological station data instead of CHELSA model data and we are – to our knowledge – the first to apply these data in a regional GLOF assessment for the HKKHN. As a result, the estimated summer precipitation or the precipitation of the warmest quarter (variable Bio18; Karger et al., 2017), has not yet been explored in other GLOF studies. We use this variable to estimate the annual proportion of summer precipitation and thus as a proxy of monsoonality. To acknowledge the approach by Wang et al. (2008, 2012), we changed the description of this predictor to “Summer precipitation or proxy of monsoonality”.

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