Natural Hazards and Earth System Sciences Discussions

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Comment on nhess-2021-167

Adam Emmer (Referee)

Referee comment on "Glacial lake outburst flood hazard under current and future conditions: worst-case scenarios in a transboundary Himalayan basin" by Simon K. Allen et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-167-RC2, 2021

The authors aim at modelling the worst-case GLOF scenarios from two existing lakes and one potential future lake in the transboundary Poiqu river basin, Himalayas. I'm convinced such studies are needed and might be of interest for readers of NHESS as well as DRR authorities. However, I have a couple of comments to this study:

1) The overall framework and structure – in the current version of this study, the authors first do the 'worst case' modelling and then search for possible GLOF triggers to justify modelled results (which is actually done not very convincingly when admitting that modelled GLOFs would need very unlikely occurrence of high magnitude (X0 Mm^3) ice-rock avalanche into the lake as a trigger); logical framework would start with: (i) search for possible / likely GLOF triggers for existing lakes, (ii) feeding them into definition of outburst parameters and scenarios, and (iii) leading to GLOF modelling + (iv) future lake and GLOF. I suggest to consider re-structuring the manuscript accordingly

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

- the essential value at the very beginning is the estimation of breach depth (in this study referred as breach height h_b). The authors provide neither details on how this value is estimated nor what the uncertainty of this estimation is; another issue is whether flat (<7° (rough Google-Earth-based measurement)) and pretty wide (> 450 m) moraine dam (e.g. Galong co) could ever be breached; and if it is breached, the crucial question is how deep (longitudinal profile of the breach is typically far from flat – I mean, if you have a vertical difference between the lake level and the toe of moraine dam 40 m (this is how you define breach depth, right?), lake level decrease in case of

breach will be less than that (it is not going to be breached to 0° slope), depending on longitudinal width of dam body; this is actually seen in Fig. 2b: if you define breach depth in this way, you should not use the same value in calculating released volume, because it differs to the lake level decrease (and in turn it leads to substantial overestimation of released volume))

- in the next step, the authors use this pretty uncertain value to estimate released volume (which is not correct in my opinion, see above) and breach parameters, using Froehlich (1995) empirical relationships; but it is important to realize that: (i) Froehlich (1995) is based on compiled information of man-made earthen dam failures, not natural dams; (ii) failure mechanism of most of these cases in the database was piping, not overtopping; (iii) released volumes in the dataset was mainly <1Mm^3; and >100 Mm^3 in only two cases (Oros, Teton); with expected released volumes 25, 70 and 262 Mm^3, you are extrapolating far beyond observed data of Froehlich (1995) and the uncertainty is unknown (Froehlich, 2004 should be checked)

3) Timing - Using Eq. 4, calculated time for breach formation of Galong co is 153 min, but you expect peak discharge in Nyalam in 82 min -> please explain what times are you referring to (82 min from breach initiation, from peak discharge at the dam (when from breach initiation?) or from development of the breach?); being as clear as possible is especially important when talking about EWSs, presenting hydrographs at the dam would be beneficial.

4) GLOF likelihood – this section gives some largely general statements and qualitative RS-based observations and looks more like a discussion rather than result to me; Tab. 3 summarises first order GLOF susceptibility factors, but this study is not a first order assessment – it is a detailed study of two existing and one potential future lake; what is shown in Tab. 3 is perhaps true for most of the lakes in the region (warming climate, steep slopes and crevassed glaciers upstream, …) and leaves the question of GLOF likelihood open; the use of >30° threshold for initiation of mass movements seems too simplifying and not really helpful for the scale you are working on

5) Practical implications – the authors mention the importance of such studies for local authorities, which is in principal true and also a rationale of many similar studies. My experience is that practical utilization, however, often lacks behind. As documented by the authors, local authorities meanwhile started remedial works by themselves, meaning that they have some kind of GLOF hazard assessments and management procedures in hands. I expect these documents may not be publicly available, but attempting getting in touch with authorities in charge of these measures would be highly appreciated (and could also help to bridge the gap between what scientists and authorities are doing).

Sorry for being bit critical, I understand that some of these issues / uncertainties are hard to address, but should be at least discussed in my opinion.

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Specific comments:

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

L40: high magnitude

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

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

L82-85: not sure this is met

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

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

L164: methodology of obtaining h_b is not clear

L178: what breach scenarios?

L189: this value needs justification

L210-214: this approach seems too rough for detailed case study like this one

L247: considering uncertainties behind a single-number result, I found a range of values highly desirable

L252 ms^{-1} when talking about velocities (please check throughout the manuscript)

L274-297: this is contradicting; on the one hand you expect 48 m deep breach of very flat moraine dam and on the other hand you find erosion unlikely?

Table 1: please specify timings (see my general comments); Jialongco – are these values of the lake before or after the remediation?

L300: consider moving to discussion (see my general comments)

L319: this is not very well-argued (most of the glacial lakes are surrounded by glacierized slopes with >30°)

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

L346: Klimes et al. actually showed that landslides in moraines are not capable of producing any large GLOF from Lake Palcacocha

Tab. 4: what is freeboard to height ratio? Both existing studied lakes seem to have surface outflow (freeboard = 0m); catchment stream density / order seem odd for evaluating GLOF likelihood; you also report no evidence of historical instabilities, further questioning the likelihood of such events for triggering GLOFs

Tab. 4: again, estimating possible ice avalanche starting zones with precision to 1 m^2 is not appropriate considering apparent uncertainties; better use a range of values

Fig. 6: if intensities are based on flow depth only, why not to use flow depth directly?

L418-418: please comment on a difference between values estimated here and size of the future lake considered in Tab. 1?

L433: there is not much about management planning in the study

L435-436: the authors published several studies on GLOF from potential future lakes previously

L446: the greatest immediate threat from 2 existing studied lakes

L450-454: this is not suggesting any lower limit, this is estimated potential loss for given scenario; please re-word this sentence

L456-458: maybe the remediation is still in progress?

L465: EWS can help to save lives, but not the immovable property (which may already be there); if the value of potentially affected immovable property is >> than the cost of remedial works, then it makes sense also to remediate the lake(s)

L508: no clear conclusion on GLOF likelihood is given

L515: details about the project (planned final stage) should be presented (maybe the plan is to drain the lake much more?)

L519-519: this is general qualitative statement which is true for many lakes in the region (not very helpful for DRR authorities I guess)

L529: why are they socially less desirable? And why environmentally less desirable (GLOF is a major disturbance to the valley ecosystem)?

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To sum up, I recommend major revisions of this work. I encourage the authors to do so and I'll be happy reviewing the revised version of this study.