The Cryosphere Discussions

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Comment on tc-2022-62

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Referee comment on "Lake volume and potential hazards of moraine-dammed glacial lakes – a case study of Bienong Co, southeastern Tibetan Plateau" by Hongyu Duan et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-62-RC1, 2022

This study aims at estimating lake volume and modelling potential GLOF from the Lake Bienong, SE Tibet Plateau. In general, I'm convinced that such studies are needed and are logical step following region-wide GLOF susceptibility assessments. The outcomes might be of interest for research community as well as DRR practitioners.

The authors of this study employ broadly used data (Landsat images, ALSO PALSAR DEM, …) and methods (bathymetric surveying, empirical equations for deriving breach parameters, MIKE11 modelling tool) in new geographical context. As such, this study brings only limited novelty in terms of methodological development and to some extent only replicates the workflows of previous GLOF modelling studies of different lakes. I would maybe expect more novelty in leading journals such as the Cryosphere. Below I provide my comments to individual parts of this study:

The section about lake evolution actually presents no evolution and can be omitted or summarized in one sentence in the Introduction or Study area section in my opinion.

The authors defined 4 moraine dam breach (GLOF) scenarios which are then modelled in the MIKE11 and MIKE21 software. However, the moraine dam breach is not the beginning of the process chain, but a consequence of certain triggering event. Considering that this is detailed case study of only one lake, I would expect the authors to analyze the whole process chain in as much detail as possible, i.e. starting with detailed quantitative analysis of potential GLOF triggers which would help to define and justify dam breach scenarios.

However, this is not met in the current version of the manuscript. My major concern is that the breach scenarios are defined subjectively and are not linked to possible GLOF triggers identified in Section 5.1. What is described in Section 5.1. gives mainly qualitative overview of potential GLOF triggers. This is perhaps true for many lakes in the region, but there is no link to considered breach depth scenarios. And this is the major drawback of

this study in my opinion.

For instance, I wonder what would need to be the magnitude of triggering slope movement to initiate 72 m deep breach? Is there any evidence that such slope movement could occur in the lake's surrounding? Landslide zones identified in Figure 11 don't seem to be releasing large mass volumes into the lake. Ideally, the initial slope movement, displacement wave propagation in the lake as well as dam breaching would be modelled, not only the GLOF. Critical questions regarding potential GLOF triggers are: Is there any evidence of mass movements entering the lake in the past? Have you observed any evidence from your analysis of remote sensing images and DEMs? Is there any evidence of potential future mass movements (displaced blocks, surface ruptures, opening crevasses, etc.)? Any evidence from your field work? Did any strong earthquake hit the region in the past? Did the lake experience any precipitation / temperature extremes? Do you expect them to change in the future? Considering the scope of the study (case study of one lake), individual triggers should be identified, quantified and treated more in depth in my opinion, feeding the definition of dam breach (GLOF) scenarios.

For the modelling part (Section 4.3, Table 3, Figures 7 to 10), flow velocities and peak discharge drop in Bada to 0.26 m/s (2,260 m3/s) in the most extreme scenario, after which it again speeds up to 18.47 m/s (22,992 m3/s) in Zongri is contra-intuitive and should be discussed / explained. In Figure 10, you even have negative peak discharge in Bada (?). Also the flow velocities of Scenario-1 (S1 to Jiawu; 44 to 65 m/s) seem unrealistically high considering it is supposed to originate from moraine dam breach.

Finally, the authors invested a lot of effort in comparing lake volumes (Section 5.2) and lake volume estimates (Section 5.3), but the implications of these comparisons are nebulous to me and the statistical treatment is incorrect. Considering the bathymetry done by the authors, it is clear that: (i) they have the best possible lake volume estimate for their study; (ii) their one bathymetry can hardly be used to validate or evaluate existing area-volume relationships. Strikingly, bathymetric data used to develop the new area-volume relationship (Table 5, Figure 12) are then used for the performance assessment (Tabel 7, Figure 14), which is statistically not correct. Moreover, some of the existing equations (e.g. Fujita's equation developed specifically for Himalayan lakes) are not considered in this comparison. Further, the number of lakes listed in Table 5 is too low to generate any meaningful conclusions about a difference between lakes associated with continental and maritime glaciers. In addition, it seems that simply larger lakes are deeper as you only have one lake > 1 km2 associated with continental glacier in your dataset while all lakes associated with maritime glaciers are > 1km2. This makes the Discussion section overall weak.

Table 4: if the lake has a surface outflow, dam freeboard = 0m.

Mean breach (Eq. 3) is used as max. breach in Figure 6; please check

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I think that especially GLOF triggers need to be addressed in more detailed and quantitative way first, resulting in re-definition and justification of dam breach scenarios. Also the Discussion section should be re-worked substantially in my opinion. To sum up, I recommend major revisions of this manuscript.