Review of the revision of the manuscript:

Anticipation the Hazard of Glacial Lake Outburst Floods in Karakoram by N.A. Bazai et al.

The authors of the manuscript “Anticipation the Hazard of Glacial Lake Outburst Floods in Karakoram” took great care to respond to the reviewers’ comments. The article now reads better than before, the reading flow of the individual chapters is much more structured and many ambiguities have been resolved. However, one main problem remains: The focus of the paper and the objective are still not entirely clear. The title is too vaguely formulated and can be interpreted as pointing in the wrong direction. A “GLOF hazard anticipation” for the Karakoram is not the topic of the paper, it is much more focused on the relationship between surge speed / critical lake depth and GLOF events.

I have summarized the comments of the three reviewers and discuss how I think the authors have implemented the suggestions. If the remaining minor revisions are incorporated and the title and aim of the paper are coherent, I recommend publication, as the topics covered are quite interesting and of value, especially if the parameters for water pressure and critical depth are incorporated into a GLOF warning system.

General comments:

1) *The title and the aim of the manuscript*
   The authors have replaced "predicting the risk" with "anticipating the hazard", but a true hazard analysis would need to consider more parameters than just lake volume and surge velocity (e.g., the stability of the ice or moraine dam, the likelihood of triggering events, or the potential paths of the flood). Therefore, the title is too broad and does not reflect the content of the manuscript. Also, mentioning the Karakoram region in the title suggests that the results are somehow specific to this region (apart from the location of the lakes, which is not relevant in the manuscript) – which, as the authors admit in their response, is not the case. I would therefore suggest that the title be changed to better reflect the scope and aim of the manuscript. A title along the lines of "Refining Lake Volume Estimation and Critical Depth Identification for Enhanced GLOF Event Anticipation" would better reflect the authors’ statements at the end of the introduction and in the conclusion.

2) *The topic of surge velocity*
   Following the reviewers’ comments on the relationship between surge velocity and GLOF event, the authors have made some changes. However, it is still not clear enough to the reader why this topic is part of this manuscript. The introduction states that the "primary objective of this study is to improve the predictive capability of GLOF event timing by refining empirical lake volume estimates and identifying critical depths for future hazard and risk reduction". Surge velocity is not mentioned – which is not a problem in itself, as other papers have investigated this at the same lakes (e.g., Bazai et al. 2022, Li et al. 2023). The authors themselves warn that this part of their results should be treated tentatively and with caution. In my opinion, the more valuable results are the surprisingly accurate geometric estimates of lake volumes and the investigation of critical lake depths. I would therefore agree with the other reviewers in suggesting that the part on surge velocity be omitted.

Specific comments:

1) Chapter 2, Study Area, is a welcome addition to the manuscript and serves its purpose well. However, it lacks a review of previous research for the selected glaciers and their lakes. There has been some research focused on these three glaciers and their lakes, some even focusing on the relationship between surge velocity and GLOF occurrence. For example, Round et al. (2017) state that surges are the main factor in lake formation at Kyagar Glacier, while Li et al. (2023) found that the volume of the lake there reaches its maximum three years after a surge period. Furthermore, Bazai et al. (2023) conclude that surge velocities have a significant impact on lake formation at Khurdopin. I would therefore encourage the authors to add some paragraphs detailing previous research in order to avoid duplication in later parts of the manuscript.
2) Regarding Figure 1, I have some minor annotations, mainly for the overview panel in the top right: The fonts are too small to read – and not really necessary, as the other regions of HMA are not part of this study. Also, the lon-lat grid is too small, and also not necessary for this kind of map. Thirdly, I understand that the map depicts a hillshade of the region, but the color map is unusual and would need to be explained. I suggest using a surface reflectance with low opacity and an underlying hillshade would be easier to understand, but that my only be my opinion. In the three small panels, the authors indicate a "Dam site". Maybe I'm misunderstanding something, but isn't the location of the ice dam at the very other end of the lake where the lake connects to the glacier? The arrows seem to point to the inlet of the lake.
   At last (and again, this is just personal preference): Please make the outline of the three glaciers stand out from the outlines of all other glaciers. If you choose to include the Indus, please use a shapefile with a higher resolution. And to be *very* nitpicky: Please adjust the scale bar lengths to 100 km (panel a) and 1000km (overview panel).
3) In Chapter 3.3 (L197f), the authors claim that the inlet of the subglacial conduit is always located at the deepest position in the ice dam. This may well be true for most cases, but the possibility of higher drainage channels in the ice should be mentioned, as this would have a large influence on the potential GLOF volume. I would also advise the authors to rephrase the start of this paragraph (L195) because, contrary to their statement, there are records of higher drainage channels at Kyagar Glacier (Li et al. 2023).
4) As mentioned in the general comments: Chapter 4.1 is very long, although the reviewers have criticized the presentation of the surge-GLOF relationship. Although the authors have partially addressed this, the chapter still adds little to the scientific discourse. The results presented are tentative at best, but there are other studies that clearly define the relationship between surge events and lake development as an increase in lake volume in the months/years after surges, i.e., a negative relationship between velocity and volume. The focus of this paper should be on estimating of lake volume and critical water pressure/lake depth, not on the relationship between surge velocity and lake formation. I agree with the other reviewers in suggesting that the paper be restructured and the surge aspect de-emphasized in order to properly focus on the more original content.
5) The conclusion is indeed very short, as previously mentioned by the reviewers. Furthermore, the first five lines are not really part of the conclusion but more of a secondary introduction to the topic. To my mind, the conclusion should comprise the summary of the findings (a little bit more detailed than in its current state) and give indications on how to use them (which are currently absent). The ability to apparently determine the lake volume quite accurately based on the geometric estimate is an important point and should be the focus of the paper, together with the critical depth parameter. In the conclusion, the authors should describe in more detail how and when these values can be applied meaningfully.

Technical corrections:

L 165: “Khurdophin” → “Khurdopin”

L 206: The layout of Table 1 seems very crowded. If the need arises, the column “Type of drainage” could be omitted, because it mirrors the values in “Vol after”.

L 261: The assumptions are not addressed in the Discussion, but in the Results.

L 264: “ice dam volume” → “ice-dammed lake volume”

L 281: For the sake of completeness some comments on Figure 4, despite my concerns with the whole surge analysis in general and chapter 4.1 more specifically: My main point is that panel d does not correctly summarize the other three panels. The bars representing the surge periods do not correspond to the position of the GLOF events. One example: The first four GLOF events at Khurdopin appear during the surge period in panel a, but in panel d they all appear after it. The same applies for multiple other occasions. Also, the labeling of the X-Axes is still confusing, as the labels area sometimes five and sometimes six years apart – in the same panel. Why is the X-Axis in panel d formatted differently when the style of the other panels could be used here as well? Why doesn’t the X-Axis in Panel c start at 0 as well?

L 301: This figure is a response to a reviewer’s comment, but since it takes up a lot of space, I would suggest moving it to the supplementary material.

L 318: In this chapter and the next, the authors assume the lake surface to be a triangle. This is only the case for the lake at Shishper Glacier. I would encourage the authors to address the question how their method can be applied to more unregular surfaces, like the lakes at Khurdopin and Kyagar.

L 335: In the corresponding figure, this coefficient is given as 0.93. Which is correct?

L 347: “of the lakes was determined…” → “of the lakes determined…”

L 349: The authors need to provide references for their statement that the lake depth is the main driver for tunnel drainage. According to other studies, low water pressure inside or under the glacier can also trigger a drainage.

L 355: The authors should choose their words more carefully when describing the applicability of their n’-value. It could play a valuable role in determining GLOF probability, but while they do not say so explicitly, readers could gain the impression that with a lake level of n’=0.6 there is the danger of a GLOF occurring imminently. It should be made clearer that a GLOF warning would be based on more components than lake depth.

L 363: Please be more specific here: Low values of n’ do not necessarily indicate shallow lakes, as it is a relative value. A large lake with a maximum depth of 300m could still contain a significant amount of water with a value of n’=0.5.

L 363: As one reviewer mentioned before: The reasoning behind the 500kPa is not clear and may confuse the reader. You have three glaciers with three significantly different water pressures – why choose the Kyagar? Do you mean to suggest that lakes shallower than 50m (~500kPa) are not capable of producing significant GLOFs?

L 364: I agree with the other reviewers: The regional aspect of the values for depth and water pressure is not clear. Why is the region mentioned here?

L 391: Again – why are your results specific to the Karakoram region? Even the three glaciers selected for this study are very different in terms of the shape, area, depth, and volume of their lakes – which of these characteristics are typical for this region?

L 419: The value of 0.6 appears solid in this context, but the second part of this sentence should be rephrased: the lake depth would be part of the hazard determination, and in itself not sufficient to issue a warning.

L 422-424: This should be mentioned before and not be part of the discussion, as it does not discuss results.

L 451: A small point: n'=0.6 does not automatically mean 510kPa, as n' depends on the water depth, while 510kPa corresponds to a fixed depth of approximately 50m. If n' is only valid from a water depth of 50m, this should be mentioned in the paper.

References:

Bazai, N. A. et al. (2022) Glacier surging controls glacier lake formation and outburst floods: The example of the Khurdopin Glacier, Karakoram, Glob. Planet. Change, 208. 10.1016/j.gloplacha.2021.103710.

Li, G. et al. (2023) Characterizing the surge behaviour and associated ice-dammed lake evolution of the Kyagar Glacier in the Karakoram, The Cryosphere 17(7), 10.5194/tc-17-2891-2023.

Round, V. et al. (2017) Surge dynamics and lake outbursts of Kyagar Glacier, Karakoram, The Cryosphere 11(2), 10.5194/tc-11-723-2017.