Third Referee Comments

Thank you for the opportunity to review the manuscript "Predicting the Risk of Glacial Lake Outburst Floods in Karakorum" by Bazai and co-authors.

In their study, Bazai et al. ask the important question of when an ice-dammed lake reaches a critical depth, requiring further investigation and possibly warning of impending flooding. This is a timely question, as many ice-dammed lakes form and drain each year in this and other regions. However, the text lacks structural clarity and motivation as to why we need both geometric models and DEMs to better understand the volumes and depths of ice-dammed lakes.

Thank you for noting that the subject matter is timely. The text structure has been comprehensively overhauled to address the referee's concerns regarding clarity and motivation.

Parts of the results should be better placed in the methods to motivate the two geometric approximations developed to estimate volumes and depths of ice-dammed lakes.

Agreed, as noted above, restructuring has addressed this issue.

The referee noted that the text at the original line 69 “needs to be rephrased to something like 'can lead to...' or equivalent. In most of the regions mentioned, most contemporary ice-dammed lakes form because a tributary becomes ice-free, and the main glacier trunk then impounds part of the meltwater from the tributary catchment. How many glaciers in Norway, the European Alps or Patagonia have been reported to surge and form a lake in past decades?”

The sentence has been rephrased to indicate that glacier surges can result in ice-dammed lakes in these regions. As the focus of the manuscript is on surging glaciers, there is no need to note other means by which ice-dammed lakes can form. We have acknowledged that there are other means of forming lakes.

At original line 76 the reviewer states “ please be more specific what knowledge gaps remain. Otherwise, this statement would downplay decades of research on ice-dammed lake formation and drainage in Iceland, Norway or Switzerland.”

We noted the range of topics for which information is lacking earlier within this paragraph. It is not our intention to review the details of where knowledge is deficient within this submission. Rather, we had indicated the focus of the study within our hypotheses. We have not downplayed the significant contribution of previously published research within this area.

The idea that surge velocity controls lake depth is interesting, but the underlying analysis falls a bit short, using mostly visually guided drawing of exponential curves.

We agree that the relationship between surge velocity and lake depth is not clear-cut. However, these data are hard-won and worth reporting, as we wish to stimulate further focus on the potential for a conclusive relation to be shown in due course. We have used suitably cautious language in reporting and discussing these observations.

Another good point is the comparison of ice-dammed lakes and their depths at failure in other regions, but there is no information on how this data was obtained and processed.

The details of how these data were obtained were included in the original Method section. However, we have edited the Method section to ensure this information cannot be overlooked by readers, as it was by the reviewer.

The discussion is largely focused on a single paper (Carrivick et al., 2020), but could benefit from a more thorough reflection on previous work on the geometry and processes involved in the drainage of ice-dammed lakes.

Thank you for your feedback. We appreciate the opportunity to improve our manuscript. In response to the reviewer's suggestion, we have expanded our discussion to incorporate a broader reflection on previous work concerning the geometry and processes involved in the drainage of ice-dammed lakes. Below is an overview of the additional references we have included and their contributions to the topic: Zhang et al., 2023, Walder, J. S., & Costa, J. E. (1996), Ng, F. S. L., Liu, S., Mavlyudov, B., & Wang, Y. (2007), (Zhao et al., 2017), Richardson, S. D., & Reynolds, J. M. (2000), and Quincey and Luckman, 2014.

Please see the attached PDF for more specific comments.

The marked manuscript comments by the referee were copy-protected, so we cannot copy them verbatim on this document. We have addressed all the comments made by the referee on the marked manuscript s/he provided, adding references as required. Here, we reply to the more substantive comments made by the referee flagged by the original line references in the manuscript. If there is no reply to a specific comment, it is because it was minor and we have made a suitable alteration.

Hazard v. Risk. We have modified the title as suggested and checked the manuscript for the correct use of these two terms.

Abstract: Minor comments made by the referee have been dealt with within the min text as there is no space to amplify the points within an abstract.

Introduction: We have emphasized the focus on ice-dammed lakes rather than moraine-dammed lakes. We have rewritten the text to make it clear that most glaciers in the HMA are now in recession, and we have added the references the referee suggested.

The reviewer indicates that the purpose and goal need to be outlined better. The objective of the study is defined in the final sentence of the Introduction.

The reviewer suggests that the study sites need to be introduced better, perhaps with a small chapter on its own. We have added a separate section after the Introduction and before the Method sections.

please phrase clearly. Reads as if 23 different lakes formed, but I think, it's three lakes that formed and drained repeatedly (23 times) in a given period.

We understand the reviewer's point. However, we cannot argue that only three lakes drained repeatedly. The glaciers are surging, and lakes form and drain repeatedly. Whether they represent the same local conditions, such that they could be regarded as the ‘same’ lake each time, is a moot point.

The reviewer asks the questions: “so, the inlet is where the bed elevation is lowest and where the glacier is thinnest? could that be a consequence of a collapsed tunnel?”. We cannot speculate on this matter, and, indeed, the text referred to is within the Method, so it would not be appropriate to make further statements in that section.

In our statement regarding the distortion of UAV images, the referee asks, “What does elevation have to do with image distortion?” There are many image issues related to the altitude at which UAV imagery is obtained. The best-known is parallax, but other than noting that we minimized distortion, there is no need to go into the technical details here.

The reviewer notes that there is “no info about the (digital) processing chain to generate DEMs and orthophotos?” We supplied references herein for succinctness. The DEM procedure is detailed.
Bazai et al. (2021) and Gardelle et al. (2013).

The reviewer states “if you have a DEM that shows the lake empty - why do you need a geometric approximation of the lake depth and volume then? couldn't you just fill the DEM until you reach the elevation of the manually mapped lake outline?”

Thank you for noting this issue. We have revised the text to clarify that the procedure is developed for situations wherein the lake is not drained.

The reviewer asks for more information on the characteristic lengths of lake basins and the procedure for determining lake volumes. This section has been fully revised to take these points into account.

The reviewer states “I read the assumptions about the lake geometry several times. It is important emphasize and justify these assumptions better, maybe using a table that show relations between lake width and depth from measurements that you extracted from the DEM.”

Our assumption of lake shape was based on visual assessment of UAV and satellite images; we have revised the text to make it clear that, to ensure an objective approach, we estimate the lake volumes assuming geometric considerations and only then compare the volume with those measured from DEMs.

The reviewer notes that the message within the following sentence is unclear. “In passing, it can be noted that the volumes of the irregular tetrahedrons in most cases were not dissimilar to the volumes of regular tetrahedrons (*i.e.*, triangular-based pyramids), the equation for which is simple, in contrast to Equation 1.”

We agree and have deleted this sentence.

In the method statement regarding assuming geometric shapes for lakes, the reviewer asks “why we do not introducing a constant that scales h to the 'true' depth of the lakes?”

We are applying the method to lakes that have not drained and for which there is no depth information. The revisions to this section have clarified the method.

At the end of the Method section the reviewer states: “n the abstract, you write about n' and the pressure in the lake necessary to generate an outburst flood. why is there no emphasis in the methods how to obtain these quantities?”

The significance of the value ‘n’ is only realized within the Results and is considered fully within the Discussion. It would be confusing to introduce n in the Method as it is not part of the method but an outcome of the results. Water pressure is a direct function of water depth, so it would be trivial to explain how depth and pressure are related.

In line 248, the reviewer asks, “Does the repeat rate of Landsat images allow for obtaining daily glacier flow velocity? I think this is probably again an average?” Yes, the velocity obtained from Landsat images represents an average velocity. This is calculated by measuring the glacier's displacement between two available images and dividing it by the number of days between those images. Consequently, the derived velocity is an average over the period between the two images.

At line 256, the reviewer asks if we extrapolated the valley walls downwards to determine whether the valley is V-shaped or U-shaped. We state that we did, and so that is a fact. We did not provide any analysis of this procedure, as the decision of V-shaped versus U-shaped is inevitably subjective. Rather, the statement here is to remind the reader of the concept of viewing the geometry of a lake as an irregular tetrahedron or isosceles triangle.

With reference to the shape chosen to represent the lakes the reviewer asks “Why not semicircular? And the lake being represented as a half cone?”.

It is possible that other geometric shapes might be chosen to represent the shape of lakes. We did not explore further shapes as the chosen shapes give a good comparison with the volume of lakes obtained using DEMs, as is shown within the Results.

I understand that the exponential relationship suggests both, that depth reduces as surge speed increases, or that depth increases as surge speed reduces. However, I am not sure whether your statement is generally valid, because throught most of your time series, you have zero depth (because there is no lake), but low glacier flow velocities.

The point made is valid if we were considering the full period of investigation. However, the velocities used to produce Fig. are the average velocities for each surge period, so these values do relate to the period of lake formation.

With reference to the exponential functions the referee notes “this sounds tempting, but given the more visual fits of the exponential function, I would refrain from this statement.”

Thank you for this cautionary note. We have revised the analysis and interpretation of Fig. 5.

At former line 298, the referee asks how much depth must be reduced to ensure that the lake volume is defined by an irregular trihedron. This value will vary lake by lake, so it is not useful to define any such value. Rather, we have indicated that regression functions between the assumed geometric lake volume and the measured lake volumes (from DEM) are the appropriate comparators.

The referee states "I still have a hard time to understand why you fit the geometry model, if you can infer the lake volume directly from the DEM.

We have reworded the text to clarify that the geometric procedure was developed to determine whether the lake's volume can be approximated accurately. The volume from the DEM is required to complete the test.

The referee asks why the relationship between the "roughly half" tetrahedral volume and the DEM volume is not illustrated. The reason is that this information is not required graphically, as the "roughly half" relationship indicates that a quadrilateral approximation might be better. This explanation was provided in the sentence following the referee's query.

again, this equation and reasons for assuming either lake geometry should be part of the methods. It's a lot going back and forth between theoretical considerations and the results.

Agreed. This equation and the explanation for its use are now found in the Method section.

The referee asks, "How did you arrive at estimating the (hydrostatic) pressure?". According to physics, the water pressure is directly related to the water depth.

The referee notes that the "discussion is not really rich of literature. The idea that lakes drain at a certain critical depth has been developed by Thorarinsson in 1939. What is the novelty compared to his findings?"

We were aware of the Thorarinsson paper but did not cite it for reasons explained here. However, we have now acknowledged his contribution in introducing a concept of critical depth by citing the relevant paper. Thorarinsson introduced the concept of a "critical zone" (k) which he designated (in his example) as having only one elevation (500 m), which is not a zone. He refers the reader to his Fig. 3 for the definition of k but k is missing in Fig. 3. He also assumed that a correction, for crevassed ice, can be applied to lower the ice barrier height and finally he calculated the height of the ice barrier relative to the lake level that would allow the ice barrier to lift (*i.e*, float). An ice barrier cannot 'lift' unless the pressure is equalized on both sides (*i.e.*, there has to be water on both sides to provide lift for the whole ice barrier. Given the uncertainty of Thorarinsson's assumptions, we can see no value in discussing our definition of *n'* with Thorarinsson's k as we are unable to come to any meaningful outcome.

CONCLUSIONS. The referee asks for a reference to be added to the Conclusions to support the sentence "Year after year, there is a rise in human casualties and losses to residences, infrastructure, the energy sector, and local and international trade.". It is not usual to include references within the Conclusions and suitable references were included within the Introductory section.

FIGURES CONSIDERED BELOW HAVE THE NUMBERS UPDATES

Fig. 1 The reviewer suggests adding the ownership of the images herein. However, the authors are the owners so there is no need to provide a credit.

Fig. 2. The reviewer suggests that we "could be good to use capital letter for coordinates, and lower-case letters for edges and lines."

The problem with this suggestion is that we only have two coordinates (A, B), and it is logical to use this notation. Using lowercase 'a' in the main text could lead to confusion.

Fig. 3 With reference to the new Fig. 3a figure, the reviewer asks, "how does Fig 5 show that the edges of the triangular lake have the same length?"

We are unsure why the reviewer asks this question as the figure does not show a lake with equal side lengths.
Fig. 4a the difference between the red and blue velocities is explained in the revised figure caption.

Fig. 4d. The reviewer states it "would be good to emphasize the timing of the GLOFs more. I am not sure whether I see a very systematic pattern in Figure 03a-c." S/he also queries what the bars show and what the horizontal axis represents.

We are unsure how to address the point of emphasis (within the main text) without laboriously comparing surge timing with each individual GLOF occurrence. The basic data are in Table 1, and Fig. 4d shows the timings in a relative sense, which should suffice to make the point. The bars were defined in the caption, and the horizontal axis has been revised to make its relationship to time evident.

In the main text, we suggest that lake volumes would increase when the ice speed is low. The referee questions this statement: "In a number of cases in Fig 4, the GLOF seems to occur around the peak glacier flow velocity."

The referee suggests that the hypothesis that "surge speed should control lake depth, volume, and potential GLOF volumes, and states "isn't this hypothesis very much dependent on the exact position of the lake? At the end of the surge phase, redistribution of the ice mass may cause a lower ice thickness in the middle part of the glacier, but a thickening at the glacier terminus. This is what I see when looking at Fig. 10 in Round et al. (2017)."

We acknowledge that there may be confounding factors influencing this hypothesis, yet the purpose of Fig. 5 is to explore the statement. We anticipated scatter in the data due to factors such as the referee indicates and, in the Introduction, we had already noted the complexity of the multiple factors which might pay a role. Thus, our proposition is a general one based on Fig. 4d. We have already acknowledged that there can be exceptions, and the issue of the controls on ice-dammed lakes is complex. At this point, we are introducing the possibility that surge speed might be one of the multiple controls on lake formation.

Fig. 5. The referee queries the exclusion of outliers in the regression analysis and the fitting procedure for the curves. The outliers were excluded using statistical procedures. However, we have revised the text and included all data points in the regression analysis. The fitting of curves is now explained more clearly. There is no upper limit to the exponential function.

With reference to Fig. 5, the reviewer asks, "does it really make sense to throw lakes of different (ABSOLUTE) sizes into this model?

Fig. 5 shows the data for all three glaciers visually. The data are not lumped into one analysis. The regression function is fit only to the Kyager data as this is the only 'large 'data set.

Considering a small lake, possibly because the thickness of the ice dam does not allow the lake to grow more: what does the surge speed might tell you then compared to really thick surging glacier?
Wouldn't it make sense to talk about RELATIVE lake sizes in terms of the lake volume relative to the maximum storage capacity of the lake?"

These are useful questions, but there is currently not enough data to examine them with any confidence. Developing an analysis based on relative lake sizes is further problematic as the maximum storage capacity is unknown.

Again, with reference to Fig. 5, the reviewer asks, "Could it make sense to plot the volume against the duration since peak glacier flow velocity? I guess it takes some time for a lake to form as soon as ice motion decreases.

We agree there are alternative approaches to relate glacier behaviour to lake volume (if indeed there is one). We believe that there may be a relationship such as the one shown in Fig. 5 and have presented the data in this format to encourage others to explore the possibility of such relationships, which might, as the reviewer suggests, include lag effects.

Fig. 6. The referee asks what DEM was used to produce the long profiles.

The DEMs for Shishper and Khurdopin are derived from UAV-generated DEMs, while the corrected SRTM DEM and the ALOS PALSAR DEM were used for Kyager Glacier.

Fig. 6. The referee asks which panel is for the Kyager glacier.

The caption indicates that Kyager is panel c.

Fig. 7. The referee notes, "this is a really interesting figure; however, you did not introduce the reader to the underlying data and how you obtained them."

This is a summary figure and is based on i) the data obtained during this investigation or ii) extracted from publications. The method used to obtain data for (i) is provided within the Method section, and the Results are summarized in Table 1. Data for (ii) are extracted directly from publications.

Fig. 7. The referee asks "why is the elevation above sea level important? isn't the elevation relative to the lake level more important?"

This is a good question. The absolute elevation is used as a convenience as it neatly separates the bar graphs to avoid overlap. Relative elevation is more appropriate for analysis and is subsumed in the parameter *n*'.

Fig. 7. The referee suggests that we "Use an informative title for the figure, please."

The figure caption has been rewritten.

The editor has asked for a third opinion on this manuscript. Admittedly, I can only add a few points to the comments of the other two reviewers, who have done a very good job in suggesting ways to improve this manuscript. If their comments are fully taken into account, I expect that this manuscript can be a suitable one for the community focusing on ice-dammed lake outburst floods.

Thank you for noting that if the revisions are thorough the work should be publishable.