Reviewer 2: Author response

Thank you for the constructive comments on our manuscript.

First, we agree that additional quantitative methods exist to assess glacier retreat, lake evolution, and rock avalanche/GLOF hazard. However, given the limited data availability, uncertainty of many processes, and complexity of the lake bathymetry, glacial bed, and geological setting at Fjallsjökull, our conclusion is that retreat rates resulting from numerical ice flow and calving models with uncertain boundary conditions will not add significantly to the results or change the main conclusions of this study. Thus, we prefer to take the simple approach presented here and to focus on the impact and broad time scale rather than a detailed, but widely uncertain, timing of events. The results of this study also contribute newly collected field data of proglacial lake bathymetry, which provide the foundation for several additional studies along the lines suggested by the reviewer, but which are beyond the scope of our project. We address the separate points of the review in more detail in specific and technical comments below.

We also note that our study is designed as a preliminary assessment of this emerging hazard in Iceland rather than a comprehensive case study of Fjallsjökull. This is the first study that investigates mass movement-triggered GLOF hazard in Iceland (with the exception of a 1967 paper that described the geomorphologic impacts of the only documented event in Iceland). Our method is intended to identify sites that should be top priorities for future studies and/or monitoring, which is especially important in areas like Fjallsjökull with rapid increases of visitors and infrastructure development, but where detailed site studies are time-consuming and costly; in other words, where the growth of potential risk is outpacing the speed of research. We will emphasize this broader application and objective in the Introduction, Conclusion, and end of Section 5.4 (lines 454-466), and also list examples with relevant citations of additional sites that should be prioritized for future study due to increasing tourism and mass movement-triggered GLOF potential in Iceland (i.e. Svínafellsjökull and Sólheimajökull) and other global proglacial areas (i.e. Alaska).

Specific comments:

Reviewer comment	Author response
To estimate a reasonable glacier retreat, the 2000-2021 average retreat rate was taken (l. 139-141 and 243) and kept constant over the next century. However, based on projected temperature scenarios, other studies showed that a linear glacier retreat is somewhat optimistic (e.g., Bosson et al., 2023). Similarly,	We agree that a linear retreat rate may not represent the future of Fjallsjökull, but this is a first order approximation given uncertainties with modeling future retreat. Glacier advance and retreat rates depend on numerous factors that we describe in lines 308-317. Estimating the future retreat rate can be done with an ice flow model coupled with a mass balance model at the outlet glacier scale. At Fjallsjökull, however, there is large uncertainty

performing a model run for a more pessimistic temperature curve (like SSP5-8.5) would be interesting, where the glacier retreat would keep accelerating in the coming decades. Then, it would be possible to give a range of glacier retreat dates for the different locations studied instead of a single one (and ranges in Table 1, too).	in the climate projections, subglacial bedrock topography, and glacier dynamics, as we explain in lines 320-322. Therefore, our assessment is that applying a numerical model of Fjallsjökull's future retreat rate will not provide more accurate results than extrapolating the current rate. Furthermore, though global scale models have made projections for surface mass balance under different climate scenarios, these have large bias corrections, come with many assumptions, and are 2D flow line models that do not capture the detailed terminus geometry that we focus on in this study. What we can use in this study with relative certainty based on observations is that the glacier will continue to retreat under a future warming climate, and we use the retreat rate of recent decades to project this into the future. We selected a simple linear rate as a first order approximation based on a recent period of warming (2000-2021), which is the observed retreat rate that most closely matches climate projections (described in lines 139-143). We also explain limitations and likely differences in retreat rates due to lake–glacier interactions in lines 323-339 (Discussion) and 481-484 (Conclusion). However, to clarify these points, we will make these changes in the manuscript: 1. Add a few sentences to Section 5.2 explicitly stating how future glacier retreat rate could differ from the 2000-2021 rate—for example, by discussing studies that project temperature changes in Iceland (i.e. Bosson et al. (2023); Noel et al. (2022)). 2. Revise terminology throughout the manuscript to emphasize that retreat rate projections should be interpreted at an order of magnitude scale, i.e. decades, rather than an exact year. 3. Revise the manuscript title to "Proglacial lake **development**…" instead of "evolution" to emphasize that these are potential scenarios rather than future projections. 4. Add two additional scenarios to the study for estimated future retreat rates to assess result

	sensitivity: 1) A rate based on projected climate change in Iceland from Noel et al. (2022), where glacier retreat rate slows until ~2040 and then increases; and 2) a faster retreat rate (doubling compared to the 2000-2021 rate) after the terminus enters the northern (deeper) overdeepening in the lake. Though these rates will still be linear, taken together, they will represent a range of three relative "paces" to reflect the influence of overdeepening depth and future climate warming variability.
While the effect of calving is mentioned at l.39 and 323-332, it does not influence the projection of glacier retreat in Figure 5, despite the strong change in bed depth for the N part of the glacier tongue. A faster melt in the N due to the over-deepened basin (similar to what happened in the S in recent years) would have consequences in the timing of deglaciation for two of the three identified potential rockfall source areas and could, therefore, be significant. I suggest considering this when estimating the glacier's future extent.	Though we do not have accurate input data to quantify the effect of calving on glacier retreat rate, we will add a retreat rate scenario for when the terminus enters the northern overdeepening (double the 2000-2021 rate—see point #4 in above comment). Based on this, a new timeline for when the glacier will retreat from the lake basin is added to the study. While equations to quantify terminus–lake interactions exist, we decided not to apply them because some crucial input parameters are unknown at Fjallsjökull without knowing future glacier surface mass balance. To clarify this, we will explicitly state what these parameters are (namely the new bathymetry presented in this study, and future ice thickness and surface mass balance, which is unknown), expanding on our discussion in lines 323-332.
At l. 173-175, the H/L ratio is defined as connecting "the highest zone point and the lowest deposit point along estimated flow path". Accordingly, the Fahrböschung angle is defined classically. However, later in the manuscript and Figure 4, the lowest deposit point is replaced by the lake shore. Then, this ratio and angle change over time as the lake expands. In this case, it represents the maximum angle to reach the lake, not the Fahrböschung angle. I suggest	Thank you for this observation. We agree with the suggestion and will correct this terminology in the manuscript.

modifying the terminology accordingly.	
A more detailed description of the geology and structures at the identified source zones is necessary.	We agree that geological and structural mapping is necessary to delineate potential slope failure planes and thus calculate landslide volumes, as we explain in lines 341-343. However, geological mapping in necessary detail has not been done for the Fjallsjökull area; regional geology is too complex to interpret based solely on remote sensing data such as drone or satellite imagery or InSAR; and this mapping is a significant project in itself and beyond the scope of this study. However, we will expand this discussion after line 343 to list methods (i.e. InSAR) that could be used to geologically map this area and specify which information they could yield (i.e. likely failure planes; landslide volume; areas of ongoing deformation; existing fractures).

Technical comments:

Reviewer comment	**Author response**
The sentence at l. 38-42 could be split in two.	Accepted—will split this sentence in two.
l. 86: up to... -> a maximum depth of ... (same clarification needed at l. 190 and l. 192)	Accepted—we will edit this wording.
Figure 1: remove the black outline around figures for all figures. In 1C, a capital N is probably to be removed. The figures could be the same width as the text. I would consider having a map instead of the orthophoto in 1C. Could Figure 2 be slightly extended to replace 1C?	Accepted—we will change figure size and remove the black outlines. We will also consider combining Fig. 2 with Fig. 1C and replacing the orthophoto with a map. What exactly is meant by removing the N in 1C? We would like to keep the North arrow in all three figures for consistency.
l. 113: Vertical and horizontal direction.	Accepted—will add this detail.
l. 114: Are the multibeam readings corrected with the surface temperature or temperature profiles? The temperature data could be nice to have in the supplementary material.	It is not certain if this information is available, but we will check with the company who conducted the survey for us to see if it can be included.
l. 119: The vertical uncertainty for the multibeam sonar survey could be added.	Accepted—we will add this.

Figure 2: the colours of the successive glacier extents should follow a continuous colour scale for better readability (some exist for colour-blinded, too). The sea should be blue to avoid being confused with a light grey glacier. The coordinate system used should be in the caption.	Accepted—we will make these adjustments.
l. 148-158: the assumption that sedimentation in the lake was negligible over the last 70 y should be made clear here already. The assumption is then discussed at l. 296-300 as a potential source of error.	Accepted—we will introduce this point here (but leave the discussion in lines 296-300).
l. 150: "Manually" can be omitted here if other datasets have been digitised manually, too.	Accepted—yes, these datasets were all digitized manually, so we will delete "manually" here.
Figure 3: Continuous colour scale for the lake extent would help readability.	Accepted—like the suggestion for Figure 2, we agree that a continuous color scale would better illustrate these changes.
l. 174-175: The lowest point of the mass movement deposit can be at the bottom of the lake, not necessarily at the lake shore. Could you reformulate, for example, writing that H is taken from the lake surface instead of the lowest deposit?	Accepted—we will clarify this.
Figure 4: The bottom of the figure could be extended to show the contact of the bedrock below the lake and bedrock-glacier as well as glacier-lake contacts. The figure could be modified so the glacier does not stop the rockfall before it reaches the lake.	Accepted—we will edit the figure accordingly.
Figure 5 should be improved following the specific comment above.	To clarify, does this refer to the comments for Fig. 2 and 3 to change glacier terminus outlines to a continuous color scheme and colorblind scale to improve readability? If so, we will make these improvements.
l. 241: Is it possible to have a lake level 15 m below sea level? Or should the calculation start at 0 m instead?	This is related to how we calculated lake volume uncertainty. We will add this clarification to the text: "Uncertainties were estimated by shifting the glacier bed vertically by ±20 m and lake

	bathymetry vertically by ±1 m (matching the vertical uncertainties of the radio-echo sounding and bathymetric datasets, respectively), and then calculating the corresponding uncertainty in the lake area and volume relative to the current lake level (5 m a.s.l.).”
l. 250-252: can we make an assumption based on the orthophoto regarding the geology and main structural features? They are, in general, essential to understand the landslide hazard. If possible, the geology at the source zones should be better described.	Building off the geology comment in “specific comments” above, given the complexity of Icelandic geology, we cannot accurately identify structural features or potential slope failure planes from remote sensing or orthophotos without comprehensive mapping of the regional geology, which is beyond the scope of this study. We will add two references demonstrating this at two other outlet glaciers of Öræfajökull: • Helgason, J., Duncan, R.A., 2001. Glacial-interglacial history of the Skaftafell region, Southeast Iceland, 0-5 Ma. Geology 29, 179–182. https://doi.org/10.1130/0091-7613(2001)029<0179:GIHOTS>2.0.CO;2 • Ben-Yehoshua, D., Sæmundsson, Þ., Helgason, J.K., Hermanns, R.L., Magnússon, E., Ófeigsson, B.G., Belart, J.M.C., Hjartardóttir, Á.R., Geirsson, H., Gu, S., Hannesdóttir, H., 2023. The destabilization of a large mountain slope controlled by thinning of Svínafellsjökull glacier, SE Iceland. Jökull 73, 1–33. https://doi.org/10.33799/jokull2023.73.001
Discussion: Reworking the discussion so that the main outputs and hazard scenarios for rock avalanches in the lake appear before the study’s main sources of uncertainty could strengthen the discussion.	Accepted—we will reorganize the Discussion to emphasize outputs first, then uncertainties.
l. 305-306: It would be interesting to discuss the possible evolution of the lake in case a GLOF happens in the coming years. Would a significant erosional event mean a lower lake level and intrusion of warmer seawater at each tide?	Accepted. We have also considered this scenario, and while a discussion would be speculative without knowledge of moraine dam breach dynamics, outlet incision, or downstream geomorphologic impact, we agree that it is worth briefly exploring. We will add a short discussion after line 306.
In Figure 8, a travel distance of 0 m is estimated in 2120 between	Accepted. The zones with high topographic potential for slope failures at Miðaftanstindur and

Miðaftanstindur and the lake. However, the polygon is not directly touching the lake. Similarly, for Eyðnatindur, could the rockfall/rock avalanche area extend below the current glacier surface?	Eyðnatindur do extend beneath the 2021 glacier surface, so we will revise the polygons accordingly for the 2120 schematic.