We would like to express our thanks to Koji Fujita (community comment) and the two Anonymous Referees for providing constructive comments and suggestions to our study. We provide our point-by-point replies and detail a revision plan for our study below (in blue color).

RC2: 'Comment on egusphere-2025-4136', Anonymous Referee #2, 25 Feb 2026

This communication addresses a critical inefficiency in current GLOF modeling: the tendency to assume a "100% drainage" scenario as the default Worst-Case Scenario (WCS). From a hydrological modeling perspective, I appreciate the authors' attempt to constrain boundary conditions with empirical reality rather than theoretical absolutes. The assumption that deep, overdeepened basins will empty completely is physically counter-intuitive and leads to "inflationary" hazard assessments that may misallocate risk management resources. This paper serves as a valuable "correction mechanism" for the GLOF hazard community, moving us away from unrealistic catastrophe modeling. It provides a pragmatic, defensible way to reduce the dimensionality of the WCS problem. However, I would recommend the author to consider the following thinking.

- thank you for thorough review of our study, we appreciate the suggestion and reflect on the points raised below

One problem with this research is that it lacks the process-based rigor required for high-fidelity hydrodynamic modeling. The reliance on a single scalar parameter (α) derived from a small dataset (N=24) simplifies complex geomorphic feedbacks into a geometric rule of thumb.

- the purpose of our method actually is to provide simplified estimate of maximum breach depth and so potential flood volume for cases where geotechnical / geophysical data are not available (majority of glacial lakes in remote high mountain areas)

- where such data are available and allow for modeling of complex geomorphic feedbacks, our method is oversimplified, as pointed out

The authors propose using α to limit breach depth. They report a median slope of 5.0° and a mean of 5.5°. However, for a true WCS, we should not be looking at central tendencies (means/medians), but rather at the tail of the distribution—specifically, the flattest possible channel slope which maximizes breach depth. The paper notes a minimum observed slope of 2.3°. If the goal is to define a safety margin for WCS, the methodology should explicitly standardize the use of this lower bound (or a specific percentile, e.g., 5th percentile), rather than discussing mean values which might lead practitioners to underestimate the potential discharge in outlier events.

- the 3° threshold that we used corresponds to 25th percentile in the dataset (and it is important that analyzed GLOFs are already a selection of real world WCSs)

- anyway, we agree with the suggestion of the reviewer that the tail of the distribution should be considered

-therefore, we'll use 3° (25th percentile) and 2° (slightly less than a minimum observed value) thresholds in the paper

A related problem is the "black box" nature of α unsatisfying. The paper argues that incision stops when the channel flattens to this angle, but it does not explain why. Is this angle a function of the internal friction angle of the moraine material? Is it determined by the armoring of the channel bed by coarse lag deposits? Is it a hydraulic control issue where shear stress drops below the critical threshold for sediment entrainment?

- we discuss the physics behind in section 4.1, however, as Reviewer #1 also requests more details, we'll extend out discussion of physical controls of breach depth in the revised version of the study

- We agree with the reviewer's assessment regarding the physical controls of the breach. The stabilization of the channel at angle α is fundamentally driven by the moraine material, density of buried large clasts and the reduction in hydraulic head. When the head is sufficiently high, it generates large discharges that create driving forces exceeding the resistance of the moraine material. During this phase, the flow entrains fine sediment, leaving behind only the coarse clasts which form an armoring layer. This armor significantly increases the channel's roughness and its resistance to further erosion.

By ignoring the physical drivers (sediment transport, dam heterogeneity, buried ice), the method assumes stationarity—that future breaches will behave exactly like this specific set of 24 past events. In a changing climate or in different lithological settings, this empirical relationship might break down.

- our approach is based on empirical evidence, but we acknowledge that future conditions may differ, therefore, we'll add this point in the discussion section of the revised paper

The dataset comprises only 24 events. While I understand the difficulty in obtaining high-quality post-GLOF DEMs, relying on such a small sample size to derive a global parameter is statistically risky.

- we set and stated four criteria to filter out events relevant for the study (section 2)

- additional few events from meanwhile published data (such as Izagirre et al. 2025) will be added and the analyzed dataset will be extended accordingly

Furthermore, there is a potential survivorship bias. Are we only analyzing dams that partially breached because they were the ones that left distinct channel geometries we can measure? If a dam were to fail catastrophically and wash out completely (eroding below the toe), would it fit this geometric model? The authors note the South Lhonak exception where erosion continued below the toe, which suggests the "toe limit" rule is not absolute.

- we focus on moraine breaches in general (not only partial breaches) in the study, although most are partial breaches

- complete wash out of moraine dam could possibly erase the information about the slope of breached channel, such case, however, has not been described in literature, to our knowledge

- South Lhonak is a specific case because the outflow channel did not leave the moraine at its lowest point (because it has lateral outflow channel, not corresponding to the axis of the main valley); the breach below the moraine toe at the outflow, however, did not erode below the moraine toe in line with the axis of the main valley

- the rule is, therefore, not absolute, as pointed by the reviewer, and as discussed in section 4.2 (outflow location matter)

Another problem is about predictive modeling associate with breach depth. The authors correctly identify that breach depth controls the hydrograph shape. In my experience with hydraulic analysis of megafloods, the sensitivity of downstream routing to the input hydrograph is massive. While this method provides a better "upper bound" for total volume than the "full drainage" assumption, it does not necessarily help with the rate of release. A 50% volume reduction is significant, but if that volume is released in half the time due to a rapid breach, the peak discharge could still be catastrophic. The paper acknowledges this limitation but does not resolve it.

- we totally agree, however, breach development time / peak discharge estimates are another huge topics which are beyond the scope of this study and we won’t be able to address them with the data and resources we have