Referee Comment - The Cryosphere (EGUsphere)

Manuscript: Thermal regime and geometry of a hanging glacier and its interaction with permafrost: the Pointes du Mourti, Swiss Alps (egusphere-2026-2455)
Authors: B. Robson, J. Irving, F. Magnin, L. Ravanel, C. Mollaret, L. Baron, A. Revil, J. Richard, J. Gentizon, C. Lambiel
Recommendation: Major revisions (substantial but achievable, mostly presentation, long manuscript & some pending discussion points).

1. Summary and overall assessment

This paper presents a rare, multi-method dataset (UAV photogrammetry, GPR, ERT, thermistors in ice and rock + ground temperature modelling) acquired over several years on a small, very steep hanging glacier. The interplay between mountain permafrost and the glacier thermal structure is genuinely under-studied and even more so in the case of steep hanging-glaciers. The dataset is hard-won (exceptionally dangerous terrain) and one has to acknowledge the mountaineering skills required. The study provides a valuable dataset and a welcome contribution to the scientific community that is well-suited for publication in TC.

My main reservations are on the following aspects: (i) the motivation could be sharpened - after a well-written introduction the rationale reduces largely to “little is known,” which risks the study reading as descriptive, when in fact the analyses offer real potential to advance process understanding; relatedly, the glacier–permafrost interaction, though central to the title and nicely developed in the main part, is not set up as a motivation in the introduction; (ii) the role of snow/firn as a thermal driver is discussed with respect to permafrost conditions but never systematically discussed in relation to the glacier thermal structure; (iii) the manuscript is quite long and I’d recommend moving some methodological details to the supplementary material; (iv) several figures and tables are hard to read, which obscures otherwise interesting results; and (v) the hazard implications deserve a separate discussion section that integrates all results (thermal regime, geometry, permafrost, etc.) with respect to glacial hazards. Currently this is baked into the thermal-regime section (S5.2).

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2. General (major) comments

G1 - Snow/firn cover as a thermal driver

Across the manuscript the thermal interpretation leans on ice thickness reduction & increasingly efficient meltwater pathways. The ice thickness theory was never really elaborated on while at the same time other important thermal drivers are largely undiscussed (snow & firn cover distribution, increased meltwater availability). This is somewhat perplexing as snow cover was discussed as an important thermal driver in the context of permafrost conditions.

G2 - Length: move methodological detail to supplement

The manuscript is long. The GPR processing steps (Table 1), the Laboratory analysis (S3.6.2, Fig.5) as well as Fig.3 (the figure just shows the warming trend across Switzerland) can be moved to the supplement. At the same time the main text can be tightened a bit from my perspective. Think about which figures are really necessary for the story line of the paper.

G3 - Figure and table readability throughout.

A large share of figures is hard to read: small fonts on scalebars/legends/axis labels, narrow widths, dense background grid on Fig. 9/10 (visual overload). I'd recommend switching wide figures to the TC two-column landscape format + increasing font sizes and width. Also, in some cases better contrasting colors could be used (e.g. Fig. 4). Specific instances are listed below.

G4 - Consolidate hazard/stability into its own discussion section

Although the hazard implications take up quite some room in the conclusions, they were only ones discussed as part of S3.5 "Thermal regime of the hanging glacier". I'd recommend discussing this separately and integrating all results (geometry, thermal structure, permafrost, etc.) with respect to hazard implications.

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3. Specific (minor) comments

Terminology (PMP)

- **L36:** Technically cold-based means below pressure-melting-point (PMP). Negative temperatures at the base could exceed the PMP.
- **L68:** Again, if you want to be correct here, I'd use the pressure melting point.

Methods clarity / missing descriptions

- **L170:** SwissALTI3D is not a LiDAR DEM but is based on aerial photogrammetry. The authors might also reference the respective swisstopo publication.
- **L174:** Was there any old or fresh snow coverage at the time of the drone flights? That could potentially strongly affect the determined surface elevation changes. Just a short is needed.
- **L197:** State Geprecision thermistor accuracy
- **L216:** Although less important, I'd also state the accuracy here.

- **L244:** Was liquid water content (not just porosity) represented in the model? Porosity only isn't driving latent heat release.
- **L540:** I can't recall that these measurements were introduced in the methods section. How did you measure the positions of the boreholes?

Interpretation / wording

- **L62:** Specify depth of those measurements
- **L66:** Specify why HGs are more sensitive to thermal changes than other glaciers. Surface meltwater percolation and refreezing also happen on any other glacier. Maybe I am misunderstanding the sentence though.
- **L629:** I'd be careful with the formulation here. The sensor itself was positioned in the active layer and showed long periods above 0°C (see Fig. S11 panel d) that is technically not permafrost. Permafrost conditions were derived from modelling and ERT as far as I understood.
- **L657:** I understand that the zero-curtain period is used to infer snow cover but isn't there a more direct way (e.g. ortho photo imagery) to back up this interpretation. Again, since snow cover plays an important role regarding both the ice and rock temperatures, I would try to get snow cover estimates for the site.
- **L678:** What exactly are the thermal and structural evolution patterns? I know this is somewhat described in the paragraph above, but maybe it could be brought a bit more to the point.

Data presentation

- **Fig 3:** In my opinion, this figure is not needed but can be accessed / referenced. In general, it would be better to use an external figure, especially as the article is already very long.
- **Fig 4:** The orthophoto dates from 6.9.2023 according to swisstopo, and not from 2024. It would be good to be precise when using external data.
- **Fig 7:** I'd project the GPR profiles onto those figures or at least on one of them to show where the interpolation is better constrained. In the end there are only 5 profiles that the interpolation is based on, meaning that the interpolation loses confidence towards the margins.
- **Fig 6:** In the caption the authors explicitly state that the bedrock is “clearly identifiable”. To me, this is not super-clear but I’m happy to follow the authors’ interpretation. However, a direct validation against the borehole thicknesses and a short discussion would be welcome.
- **Fig 6:** Also here I'd recommend increasing font size of annotations and axis labels. Particularly the legend and labels in panel a are hard to read. From my point of view you could omit panel a which would leave more space for the radargrams. The location of the GPR profiles and ice temperature boreholes are already introduced in Fig.4 (just annotate the profile names there) and enlarge the figure such that the surface features (e.g. crevasses, Bergschrund) become more visible.

- **Fig 8:** Does it make sense to represent surface elevation change normal to the slope? What would be the argument to do this, and how would it help visualization or integration to compute total volume change? An explanation of the author's rationale would be useful. Furthermore, surface elevation changes should be represented as negative values if ice thickness loss is mapped.
- **L444:** I'd follow the order of the table when describing the data. Starting with PM_E2 and ending with PM_SE1
- **Tab 2:** Expressing the results as "ice melt" is wrong. DEM differencing observes surface elevation changes that are a combination of accumulation, melting and ice flow dynamics. Values of elevation losses should be represented in negative terms. Furthermore, merging elevation changes (up to 2024) with local ablation rates at the cables is not possible, and the value for 2021-2025 cannot be represented. A surface elevation change can occur without any melting. It is also unclear here, whether surface elevation changes are also normal to the surface or vertical.
- **Tab 3:** I find a table view here not ideal. The results could be much easier understood in a plot. Why don't you plot repeated temperature vs. depth profiles per borehole and hydrological year?
- **Tab 4:** Personally, I find it hard to absorb the information from a table. This data could easily be plotted which would make trends easier to identify. Similarly with the data in Tab. 3.
- **Fig 9:** I'd provide a legend for + and - air temperature shading (which you did in Fig. 10). This way you can leave that out in the caption. And the figure becomes faster to interpret. Also does it need such a dense grid. Reducing the amount of grid lines would make the actual data in the figure more readable.
- **Fig 10:** Same comment as for Fig 9. I'd reduce the density of the background grid (at least on the x axis). Also, the meaning of the red and blue trend lines should be explained in the caption. Otherwise cool figure. I like how you can show the increase in temperature amplitude.

Citations

- **L52 / L53:** I'd recommend a GLAMOS citation here instead of the press release. E.g. the Swiss Glacier Bulletin 2024. https://doi.glamos.ch/pubs/glbulletin/glbulletin_2024.pdf

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4. Technical corrections

- **L93:** I'd reword to "Repeat photography between 1942 and 2020 illustrate the substantial changes that have occured at PdM during the last 80 years."
- **L658:** "reduce" → "reduced".
- **L697:** "…with most likely to disappear" → "…with most of them likely to disappear".
- Summit elevation L16 (Abstract) / L79: The first one likely a typo both should be 3563 m a.s.l.