Polythermal conditions in small glaciers in the Swiss Alps

Response to Referee #1 (Marcus Gastaldello)

27 August 2026

Dear Dr. Caroline Clason and referee 1,

We thank the reviewer for your careful and constructive assessment of our manuscript. We greatly appreciate the time and effort you invested. Below we respond to each of your comments individually. Our replies are shown in blue, and the corresponding changes in the revised manuscript are quoted in green.

The main changes we made in response to your comments are:

1. We reworked the three principal profile figures (Figs. 6–8): the depth of zero annual amplitude is now marked in both the cross-section and profile panels, borehole identity is separated from the temperature colour scale, and the line styles and axes have been clarified.
2. We identified and corrected two errors in the firn-cover model that had caused the apparent inconsistency between figures, and added an independent validation of the reconstructed firn extent against digitised orthophotos (new Supplement Sect. 3.1).
3. We added a dedicated discussion of the contrasting L1 and L2 profiles at Hohsaas.
4. We made numerous smaller clarifications, added supporting references, and corrected minor errors throughout.

We have also increased the font size in Figure 5 (now Figure 4) to improve its readability. We believe these revisions have substantially strengthened the manuscript.

Note on figure numbering: because the deployment-timeline figure was moved to the Supplement in this revision, the subsequent main-text figures shifted by one. The profile figures referred to in the comments as Figures 6–8 are now Figures 5–7 in the revised manuscript, the firn-loss map (Figure 9) is now Figure 8, and the seasonal-temperature and historical-comparison figures (Figures 4 and 5) are now Figures 3 and 4; several supplementary figures were renumbered accordingly. To match the comments, the responses below use the original numbering, and each revised figure reproduced at the end also gives its new number in the revised manuscript.

R1.C1 (Summary)

The manuscript is generally well-written exploring a rather novel subject area in glaciology of emerging polythermal conditions on small Alpine glaciers. Comprehensive and well-planned fieldwork campaigns have been carried out across several sites in Switzerland providing invaluable data on englacial temperature and their overall thermal regime. The presentation and subsequent analysis of these results could perhaps be improved in the three principal figures (Figs. 6 – 8) by better demarcating the boundary between the zone of seasonally variable temperatures from those more representative of long-term climatic changes (comment # 25). The modelled firn coverage, used to reinforce key discussion points about its role in the changing thermal regime, also lacks validation and doesn't seem to fully correlate with additional figures in the supplement (Figs. S9 – S10) (comment # 27). Finally, there is quite an interesting case where two assessed profiles in close proximity to each other on the Hohsaas glacier have very different thermal regimes, but there is very limited content on this matter – it warrants further analysis and discussion (comment # 26). Aside from these major comments, there are a few suggestions and minor corrections.

Response: We thank the reviewer for the careful and constructive assessment, and for the positive appraisal of the fieldwork and the dataset. We are grateful in particular for the three major points raised (comments #25, #27, and #26), which have led to substantial improvements, and we address these together with all minor comments individually below.

R1.C2 (Line 5)

Perhaps also specify the names of the three confirmed polythermal glaciers in the previous sentence.

Response: Agreed; this has been revised as suggested.

Changes in the manuscript: Abstract: “We confirm polythermal conditions in three glaciers — Alphubel South, Chessjengletscher, and Hohsaasgletscher — where the cold-temperate transition surface lies 17–38 m below the surface and ice temperatures range from temperate to −2.1 °C.”

R1.C3 (Line 17)

It perhaps should also be specified that we are referring to beneath the depth of seasonal invariance / zero annual amplitude. Near surface ice temperatures will fluctuate throughout the year.

Response: We appreciate the reviewer's point, but do not think a depth-based qualifier is necessary here. The classification into cold and temperate ice follows directly fom ice temperature relative to the pressure-melting point (PMP), independent of depth: ice remaining below the PMP is cold, whether or not it exhibits seasonal temperature fluctuations, and only ice reaching the PMP is temperate. In our shallow seasonally-influenced boreholes, temperatures do not approach the PMP (Sect. 4.1) and we consider this as cold ice. We therefore prefer to retain the definition as stated.

Changes in the manuscript: No manuscript change.

R1.C4 (Line 23)

'High-altitude' is a bit of a vague term - especially in the context of your work and the coming introduction to your chosen study areas. Perhaps it is better to specify these measurements are 4,000 + m a.s.l. It may also be better to mention more spatially varied examples from Fiescherhorn, Mont Blanc etc. rather than just Monte Rosa.

Response: We agree and have revised this sentence to specify an elevation threshold and to broaden the cited examples beyond Monte Rosa. Suter and Hoelzle (2002) already covers the Mont Blanc area alongside Monte Rosa, as does Vincent et al. (2020, Dôme du Goûter) in the following sentence, and we have additionally cited Pavlova et al. (2014), who report borehole temperatures of −6 to −2 °C at Fiescherhorn (Bernese Alps), to broaden the geographic spread.

Changes in the manuscript: Introduction (Line 23): “In the Alps, existing observations are biased toward accumulation areas above ~3800 m a.s.l., including sites in the Monte Rosa, Mont Blanc, and Bernese Alps massifs (Haeberli and Alean, 1985; Haeberli and Funk, 1991; Suter and Hoelzle, 2002; Hoelzle et al., 2011; Pavlova et al., 2014) (Supplementary Fig. S1), where limited energy availability has historically resulted in predominantly cold thermal structures.”

R1.C5 (Line 24)

There are also all of the recent measurements on Monte Rosa in the GLAMOS yearbooks should you wish to include them.

Response: Agreed; this has been revised as suggested.

Changes in the manuscript: Added the Swiss Glacier Bulletin 2025 (Huss et al., 2026) among the recent-measurement citations (Line 24).

R1.C6 (Line 26)

Probably better to reference the 2021 paper rather than the MSc thesis - https://doi.org/10.5194/tc-15-3181-2021 - "Firn changes at Colle Gnifetti revealed with a high-resolution process-based physical model approach"

Response: Agreed; this has been revised as suggested.

Changes in the manuscript: Switched the Colle Gnifetti firn citation to Mattea et al. (2021, https://doi.org/10.5194/tc-15-3181-2021) in place of the MSc thesis (Line 26).

R1.C7 (Line 33)

Yes, however, your upcoming results will illustrate that these phenomena are not necessarily confined to elevations below 3,000 m a.s.l. which you are implying here.

Response: We agree the phrasing here was unclear. The '3000 m a.s.l.' figure was meant to describe only the specific 1970s large valley glacier measurements (Haeberli, 1976), not the broader set of lower-elevation ablation-zone sites discussed afterward, several of which, as the reviewer notes, lie well above 3000 m. To remove this confusion, we deleted the '3000 m a.s.l.' figure from that earlier sentence, since it was not essential there, and added an explicit threshold that more broadly suits our dataset as well as the data gap discovered in the glenglat database.

Changes in the manuscript: Line 33: "At these lower elevations (< 3800 m a.s.l.), glaciers may respond to atmospheric warming differently from higher-elevation glaciers that still retain firn-covered accumulation areas, because firn loss alters how latent heat from meltwater is retained." The "< 3000 m a.s.l." figure was removed from the earlier 1970s valley-glacier sentence (now: "large valley glaciers, primarily dating back to the 1970s").

R1.C8 (Line 35)

I think this could be rephrased to be a bit clearer as it is (in my opinion) a critical point. The point is that meltwater can percolate into firn and refreeze at depth enabling energy release in an area less thermally coupled to the surface. Without firn, meltwater either simply runs-off the impermeable ice or refreezes on the surface so the latent energy is not retained.

Response: We agree this is a critical point that deserved clearer explanation, and we have adopted the reviewer's suggested framing closely.

Changes in the manuscript: Line 35: "Where firn is present, meltwater percolates to depth and refreezes in ice that is less thermally coupled to the surface, retaining the released latent heat; where firn is absent, meltwater instead runs off over the impermeable ice or refreezes at the surface, where the heat is quickly lost to the atmosphere."

R1.C9 (Line 39)

You will later mention it in your selection criteria, but I would assume the absence of significant crevassing is also an important factor to state here, as it provides a means for deep meltwater infiltration.

Response: Agreed; this has been revised as suggested.

Changes in the manuscript: Line 39: "…may be particularly important for small glaciers, where limited ice flow and internal deformation (Huss and Fischer, 2016) reduce strain heating and sparse crevassing limits deep meltwater infiltration."

R1.C10 (Line 43)

If a cold-based hanging glacier became temperate and collapsed would that not be the thermal state of the glacier directly causing an instability?

Response: Thermal conditions are best framed as a precondition rather than a direct cause of instability. This is consistent with the literature we cite: even where a cold-based glacier becomes temperate and collapses, the mechanism identified is that warming weakens basal support or creates the conditions for basal sliding, for example the 1895 Altels breakoff, rather than the thermal state itself constituting the instability. We now cite Faillettaz et al. (2015) at this point to support this framing.

Changes in the manuscript: Line 43: added a supporting citation: "While thermal conditions alone do not directly cause instability (Faillettaz et al., 2015), they can promote preconditions for ice break-offs, avalanches…".

R1.C11 (Line 49)

There is also the 1892 Tête-Rousse event if you want a more historic example.

Response: Agreed; this has been revised as suggested.

Changes in the manuscript: Added Vincent et al. (2010), the re-analysis of the 1892 Tête Rousse outburst flood, at Line 49.

R1.C12 (Line 54)

You haven't really specified this though and again if this is referring to below 3,000 m a.s.l., several of your fieldwork sites are much higher than this elevation.

Response: We agree, this is the same underlying issue as R1.C7. The claim is grounded in Supplementary Fig. S1, which was not cited at this point in the text, and we have revised the sentence to cite it directly and to state the actual elevation range.

Changes in the manuscript: Line 54: "…the lack of observations currently precludes any predictions about which glaciers might fall into this category, especially in the poorly sampled ~2700–3800 m a.s.l. range (Supplementary Fig. S1)."

R1.C13 (Table 1)

It is potentially more representative to average values over the area where you are investigating. For example, at Tortin, your study area is perhaps significantly less steep than the glacier average.

Response: Agreed; this has been revised as suggested.

Changes in the manuscript: Recomputed the Table 1 slope column over the study-area footprint and clarified the caption ("Mean slope was averaged over the study area (the GPR survey / borehole footprint)"). Study-sites text (Sect. 2): "…mean surface slopes between 9° and 29° within the study areas."

R1.C14 (Figure 2)

CV1TT's location could be better indicated on this Figure.

Response: We agree and have adjusted the arrow in panel f to point precisely to CV1TT's location, which sits in a shallow depression behind a small ridge dividing the glacier into two ice bodies. Unfortunately, this makes the measurement point not directly visible on the image. For reference, the exact position can be viewed in 3D (https://maps.app.goo.gl/Y8kyuDEY45Bor26T7).

Changes in the manuscript: Adjusted the arrow (and its colour) in panel (f) and added to the Fig. 2 caption: "CV1TT lies in a small depression and is therefore not directly visible; the green arrow indicates its approximate location."

R1.C15 (Line 105)

The hot water drill might cause quite a significant thermal perturbation (particularly compared to the Heucke) that could conceivably influence the thermal state of the glacier long-term and affect your results. Has this been considered? Perhaps an estimation of the energy input provided by both the sensible and latent energy of the drilling water and the time for this to dissipate? Even if the temperature equilibrates to its surroundings, perhaps this new equilibrium is still not fully undisturbed from pre-drilling.

Response: We thank the reviewer for raising this. Hot-water drilling does introduce a thermal disturbance, but it is spatially confined to the immediate borehole surroundings and short-lived: the drilling water refreezes and the local perturbation decays over timescales of days to weeks, depending on borehole diameter and ice temperature (Humphrey and Echelmeyer, 1990; Cuffey and Paterson, 2010). The energy introduced is small relative to the surrounding ice, so once refreezing is complete and conduction has re-equilibrated the near-borehole ice, the sensors sample the undisturbed far-field temperature rather than a drilling-warmed one. Our measurement protocol accounts for this in two ways: we exclude an initial 14-day equilibration window from all analyses, and we verify directly that sensor temperatures have stabilised before use (Supplementary Fig. S14-16), where the deeper sensors show little to no drift after this period. We agree that no equilibrium is ever perfectly undisturbed, but consider any residual offset to be well within our stated sensor accuracy and far smaller than the temperature contrasts on which our interpretations rest.

Changes in the manuscript: Added to Sect. 3.3: “The thermal disturbance introduced by hot-water drilling is spatially confined to the immediate borehole surroundings and decays over timescales of days to weeks, depending on borehole diameter and ice temperature (Humphrey and Echelmeyer, 1990; Cuffey and Paterson, 2010); our 14-day exclusion window is therefore sufficient for the deep boreholes to converge on stable, undisturbed ice temperatures, as confirmed directly by the equilibrated sensor records (Supplementary Fig. S14–16).”

R1.C16 (Figure 3)

Perhaps this figure could be placed in the supplement. It is useful information but maybe not necessary in the main body?

Response: We agree; the timeline figure is useful information but is more appropriately placed in the Supplement, and we have moved it there.

Changes in the manuscript: Moved Figure 3 (deployment/GPR/mass-balance timeline) to the Supplement (now Supplementary Fig. S2); in-text references updated, e.g. “A timeline of all borehole instrumentation and GPR surveys … is given in Supplementary Fig. S2.” (Data and Methods).

R1.C17 (Line 117)

This is quite a low accuracy for this application if the intended objective is to accurately differentiate between temperate and cold ice in the 0 - 3.5 °C.

Response: We agree that ±0.2°C is a relatively low accuracy in absolute terms. It is the manufacturer's nominal accuracy of the Tinytag PB-5010 thermistors used in the shallow boreholes. We would note, however, that the temperature contrasts underlying our interpretations are generally substantially larger than this, for example the mean warming with depth of 0.6–1.5°C at Chessjen and Hohsaas, and the multi-degree spatial contrasts between temperate cores and cold termini. The deepest, bed-reaching measurements are additionally constrained by the Geoprecision TNode HD chains at ±0.05°C, at the high-accuracy end of what is achievable for englacial point thermometry, and all sensors were calibrated prior to deployment to reduce sensor-specific offsets below the nominal specification (Sect. 3.1.2). Finally, in the few cases where a value falls within sensor accuracy of the melting point (e.g. SR2TT at −0.18°C, HS1TT at −0.1°C), we already refrain from a definitive classification and state only that temperate conditions are 'likely' (Sect. 4.1). We have added a sentence to the measurements and calibration subsection making this reasoning explicit.

Changes in the manuscript: Added to Sect. 3.1.2: “The temperature contrasts underlying our interpretations are generally substantially larger than these sensor accuracies; where individual values fall within sensor accuracy of the melting point, we interpret them accordingly.” The “(manufacturer specification)” qualifier was also added to both accuracies.

R1.C18 (Line 181)

This is potentially out of scope for this paper, but I think a good direction for future research/ work would be to use a more physically based means of calculating the spatial variation of temperature in these profiles. At present, the spatial patterns displayed in subplot (a) of Figs. 6-8 are subject to a high degree of uncertainty.

Response: We agree and thank the reviewer for this suggestion. A more physically-based approach (e.g. thermal modelling) to characterising the spatial temperature field was originally considered, but set aside as it would have gone beyond the scope of this paper, which centres on the extensive field dataset collected. We acknowledge that the interpolated temperature field carries a non-negligible degree of uncertainty, particularly away from borehole locations; we mitigate this by cross-checking the interpolated fields against GPR reflecitvity patterns (Sect. 3.3.1, L172-173) and explicitly cautioning against over-interpretation of areas without direct measurements. We agree this is a valuable direction for future work.

Changes in the manuscript: No manuscript change.

R1.C19 (Line 189)

Acronym previously defined.

Response: Agreed; this has been revised as suggested.

Changes in the manuscript: Removed the redundant spelled-out term at Line 189; only the acronym now remains.

R1.C20 (Line 197)

I think it would be beneficial to briefly elaborate on how this model works. Is it for example a simple temperature index or surface energy balance model etc. It isn't too clear from the description below and thus how valid this estimation of firn thickness / cover is.

Response: We agree the model description was too brief. We have expanded Sect. 3.4 to specify that the model of Huss et al. (2021) is a distributed accumulation and temperature-index melt model with a potential-solar-radiation term (Hock, 1999), rather than a surface energy balance model, and to describe how accumulation is computed, spatially distributed, and calibrated against the seasonal field measurements. Since the firn-cover reconstruction is most sensitive to the accumulation component, we now note that Alphubel, Chessjen, and Hohsaas are constrained by fewer years of snow-probing data than the long-monitored sites, and we have added an independent validation of the reconstructed firn extent against manually digitized SWISSIMAGE orthophotos (new Supplementary Sect. S3.1; see also R1.C27). We point readers to Huss et al. (2021) for the full model formulation.

Changes in the manuscript: Expanded Sect. 3.4: "The model combines a distributed accumulation model with a temperature-index melt model that includes a potential-solar-radiation term (Hock, 1999), rather than a full surface energy balance… accumulation is derived from precipitation, scaled by a correction factor and distributed using observed snow-accumulation patterns… Model parameters are calibrated against the seasonal stake and snow-probing measurements at each site." Added the limited-constraint note for AH/CJ/HS and a pointer to the new validation (Supplementary Sect. 3.1).

R1.C21 (Line 212)

This is perhaps a matter of author preference, but generally papers from 'The Cryosphere' seem to more commonly separate results and discussion into different chapters. I am unsure if there is a convention for this with this journal.

Response: We thank the reviewer for raising this. The other reviewer made the same suggestion, and we have adopted it: the former combined 'Results and discussion' section has been split into a separate Results section (Sect. 4: seasonal temperature variations, decadal changes at Sex Rouge and Corvatsch, and the polythermal-site thermal structure) and a Discussion section (Sect. 5: firn cover loss as the primary control, implications for Alpine thermal regimes, and hazard implications). We found that the split improves the readability of the manuscript by separating the presentation of the observations from their interpretation.

Changes in the manuscript: Split "Results and discussion" into Results (Sect. 4: 4.1–4.3) and Discussion (Sect. 5: 5.1–5.3); sections renumbered and cross-references updated.

R1.C22 (Line 273)

From this point forward, the paper focus primarily on Alphubel, Chessjen and Hohsaas. I think it would be good to explicitly state why the other 3 sites are not subject to the same level of analysis. With Corvatsch it is presumably because the glacier is not sufficiently deep, however it is not so clear with Tortin and Sex Rouge. Given that the latter two glaciers have been lacking firn for several decades it would also be a very good *comparison with the other 3 locations. In order to make the manuscript more concise, one could even consider only focusing on the polythermal glaciers and not including the others at all.*

Response: We thank the reviewer for raising this. We would like to clarify that the paper does not, in fact, set the other three sites aside after this point: Sex Rouge, Tortin, and Corvatsch are treated throughout the seasonal analysis (Sect. 4.1), the decadal comparison against historical data (Sect. 4.2), and the firn-history analysis (Sect. 5.1). What narrows to Alphubel, Chessjen, and Hohsaas is specifically the full vertical thermal-structure characterisation in Sect. 4.3, and for a concrete reason that was stated in the Methods but not recalled here: only these three sites received deep, bed-reaching boreholes, whereas the other three have only shallow (≤15 m) boreholes that sample only the seasonally influenced near-surface layer. We have added an opening passage to Sect. 4.3 making this explicit. Regarding the reviewer's alternative suggestion to drop the shallow-drilled sites entirely for concision, we would prefer to retain all six. The shallow sites are not merely supplementary: they each provide a full annual cycle of temperature data, Sex Rouge and Corvatsch enable a rare decadal comparison against historical borehole measurements (Sect. 4.2), and, having been firn-free for more than three decades, they anchor the firn-free-duration gradient that underpins our central argument linking firn loss to thermal structure (Sect. 5.1). We therefore believe that removing them would weaken the manuscript's central argument rather than streamline it.

Changes in the manuscript: Added at the start of Sect. 4.3: "Because deep boreholes reaching the glacier bed were drilled only at Alphubel, Chessjen, and Hohsaas (Sect. 3.1.1), the following analysis of the full vertical thermal structure is limited to these three sites. The shallow (≤ 15 m) boreholes at Corvatsch, Sex Rouge, and Tortin sample only the seasonally influenced near-surface layer."

R1.C23 (Figure 5)

I appreciate that it is quite difficult to compare historic measurement in the ablation area with major changes in glacier depth, however I am not certain about the validity of this method. Englacial temperatures in the seasonally variable zone are heavily influenced by the weather conditions of that particular year and I'm not too sure it can be used as a means to assess climatic changes even if you are comparing exactly the same day of the year.

Response: We agree that englacial temperatures within the seasonally affected zone are influenced by the weather of the individual measurement year, and that this complicates their use as a direct measure of climatic change. We would note, however, that the manuscript does not interpret these comparisons as direct climatic trends: we already emphasise that the observed signal reflects downward cold-wave penetration and ablation-driven removal of cold ice 'rather than atmospheric forcing alone' (Sect. 4.2), and we account explicitly for the reference-frame shift caused by surface lowering. To address the reviewer's specific concern, we have added a sentence clarifying that matching the day of year controls for seasonal phase but not interannual weather variability, and that we therefore compare the historical single-day measurements against the full modern annual envelope (Fig. 5) and interpret differences structurally rather than as precise trend magnitudes. We would emphasise that the load-bearing findings here are qualitative regime changes robust to weather noise, for example Corvatsch having thinned from a site documented as cold-based at 13 m depth (1999/2000) to one where the entire remaining 9 m column is seasonally fluctuating, rather than marginal temperature differences. Given how rarely decadal repeat measurements exist in Alpine ablation zones, we believe that presenting this comparison, appropriately caveated, is a valuable contribution rather than an overinterpretation, and we prefer to retain it.

Changes in the manuscript: Added to Sect. 4.2: "Matching the day of year controls for the seasonal phase of the signal but not for inter-annual weather variability… We therefore compare the historical single-day measurements against the full modern annual range at each depth (min–max envelope in Fig. 5), and interpret differences within the seasonal zone in terms of the broader thermal structure and its evolution rather than as precise climatic-trend magnitudes."

R1.C24 (Line 323)

'Advection of temperate ice from upstream can likewise be ruled out, as temperate conditions are predominantly observed in the upper glacier where no upstream source exists' - This could perhaps be phrased slightly better.

Response: Agreed; this has been revised as suggested.

Changes in the manuscript: Lines 323–324 now read: "Since the temperate ice identified here lies in the uppermost reaches of each glacier, where no upstream ice input exists, advection from upstream can also be ruled out as a means of supplying temperate ice."

R1.C25 (Figures 6-8 (MAJOR))

An estimated ZAA depth on subplot (a) of Figs. 6, 7 & 8 would be useful to help understand at which depths temperatures on this figure are seasonally variable. This is very important in my opinion as at present the subfigure shows a mean temperature and it is not clear to what extent these temperatures seasonally fluctuate. Ideally it would also be better to avoid colour coding the different boreholes with the same colour map used to demarcate temperature in the adjacent subplot. Eg. AH1G is coloured dark brown which signifies to the reader it is temperate, but it in fact merely shows which profile is which. Finally, it would be probably best to simply retain the same colour map for measured borehole temperatures in subplots (a) and (c) for direct comparability.

Response: We thank the reviewer for these suggestions, which we have implemented in full across all three figures (Figs. 6 to 8). (1) We have added the estimated ZAA depth in two places: in panel (a) as a horizontal tick at each borehole, and in panel (b) as a horizontal line marking the boundary of the seasonally variable zone, so that the depth range over which temperatures are seasonally variable, as opposed to representative of the longer-term thermal structure, is now clearly indicated in both the spatial and the profile views. (2) We have recoloured the borehole profiles in panel (b) using a distinct qualitative colour scheme (ColorBrewer; Harrower and Brewer, 2003), chosen for maximal categorical distinctness, so that the borehole identifiers no longer share the temperature colour map; AH1G, for example, is no longer coloured in a way that suggests temperate ice. (3) We now use the same temperature colour map for the measured borehole temperatures in panels (a) and (c), allowing direct comparison between them.

Changes in the manuscript: Revised captions of Figs. 6–8, e.g.: "Black ticks along each borehole mark the depth of zero annual temperature amplitude (ZAA), below which seasonal temperature variations become negligible. (b) … the grey-shaded area marks temperatures above 0 °C, and the solid black line marks the ZAA depth." Panel (b) recoloured with a qualitative palette; panels (a) and (c) share one temperature colour map.

R1.C26 (Figure 8 (MAJOR))

Consider only displaying profile L2 or separating this into two separate figures for each cross section. I find it very interesting how profiles L1 and L2 apparently have completely different thermal structures despite being roughly parallel at a distance of around 20-30 m apart. It is perhaps quite significantly influenced by the difference in glacier thickness, but regardless I think it warrants more discussion / attention in the manuscript.

Response: We thank the reviewer for highlighting this contrast, which we agree merits discussion, and we have added a paragraph on the L1/L2 divergence in the Discussion (Sect. 5.2; see our response to comment #28 below). Regarding the suggestion to display only L2 or to split the figure into two separate cross-sections, we prefer to retain both profiles within a single figure: it is precisely this side-by-side positioning that makes the L1/L2 contrast visible to the reader, the same contrast the reviewer highlights as interesting, and splitting them into separate figures would make that comparison harder to appreciate rather than easier.

Changes in the manuscript: See R1.C28 for the new L1/L2 paragraph. Fig. 8 retains both profiles side by side.

R1.C27 (Figure 9 (MAJOR))

It's unclear to me how valid these modeled results are. SR, GT & CV are said to be supported by 10 years of monitoring. However, the polythermal glaciers AH, CJ and HS seem to lack much validation which is especially important as this concept of recent firn loss being a key driver of their complex thermal regime is a main conclusion of the paper. The firn extent on Figure 9 also does not appear to correlate with supplement Figures S9 and S10. For example, Alphubel glacier appears to have scarcely 10% firn cover in Fig. S9 from 2019 onwards.

Response: We thank the reviewer and address this in two parts. First, on the apparent inconsistency between the main-text and supplementary firn figures: while investigating it we identified two genuine errors in the firn-cover model output, the final year (2026) was inadvertently used as the reference year for computing time since firn loss, and firn layers exceeding the 20-year threshold were not correctly converted to ice. Both have been corrected and all firn-related figures regenerated; they are now mutually consistent (see also R1.C35/R1.C36). Second, on validation of the less-constrained sites (AH, CJ, HS): we have added an independent validation of the modelled firn extent against firn/perennial-snow boundaries manually digitized from high-resolution SWISSIMAGE orthophotos, across all six glaciers and multiple years (new Supplementary Sect. S3.1, Figs. S12–S13). The comparison shows no systematic bias (+1.5 pct pts; n = 31, R^2 = 0.63) and comparable agreement at the polythermal sites relative to the long-monitored ones, indicating the reconstruction is reliable also where in-situ data are limited. We acknowledge the digitization itself carries uncertainty, as the firn line can be difficult to distinguish from residual snow; this is stated in the Supplement.

Changes in the manuscript: Added to Sect. 3.4: "…we therefore assessed the reconstructed firn extent independently against firn/perennial-snow boundaries manually digitized from high-resolution SWISSIMAGE orthophotos; the comparison shows no systematic bias and comparable agreement across all six glaciers, including the less-constrained polythermal sites (Supplementary Sect. 3.1)." Firn-model errors corrected (R1.C35) and all firn figures regenerated; affected firn-free durations in Sect. 5.1 updated.

R1.C28 (Line 378)

'The thermal response to firn loss is neither instantaneous nor spatially uniform…' - This would perhaps be an excellent place to discuss the dissimilarities between profiles L1 & L2 on Hohsaas.

Response: We agree and thank the reviewer for the suggestion. We have added a paragraph at this point in the Discussion (Sect. 5.2) discussing the divergence between profiles L1 and L2 at Hohsaas, attributing it to the difference in ice thickness relative to the seasonally-influenced layer.

Changes in the manuscript: Added to the Discussion (Sect. 5.2): "This is illustrated by the contrast between profiles L1 and L2 at Hohsaas, which are only 20–30 m apart but show markedly different thermal structures (Fig. 8a). At L1, ice thickness (15–17 m) is comparable to the seasonally-influenced layer, leaving insufficient ice thickness for a temperate core to persist even where firn-loss history is similar to L2; at L2, the greater ice thickness (> 30 m) provides enough insulated depth for temperate ice to remain despite ongoing cooling. This contrast highlights how ice thickness modulates the pace and completeness of thermal response to firn loss, independent of the surface forcing itself."

R1.C29 (Line 413)

'Mid- to lower-elevation sites' - Particularly with the inclusion of the Alphubel site, arguably you have high elevation ablation areas too.

Response: We agree and have revised this sentence to avoid elevation-band labels altogether, letting the actual range speak for itself.

Changes in the manuscript: Conclusions: "The observations span an elevation range of 2700–3800 m a.s.l., directly addressing a key gap in the existing ablation-zone record."

R1.C30 (Table A1)

Missing units [m]

Response: Agreed; this has been revised as suggested.

Changes in the manuscript: Added the missing [m] units to the X and Y columns of Table A1.

R1.C31 (Line 550)

'Mattea, E…' - This is an MSc not a PhD thesis.

Response: Agreed; this has been revised as suggested.

Changes in the manuscript: Corrected the Mattea reference to the 2021 paper (Mattea et al., 2021, https://doi.org/10.5194/tc-15-3181-2021).

R1.C32 (Line 101 (S))

'Imagine and time…' - Typo? Image?

Response: Agreed; this has been revised as suggested.

Changes in the manuscript: Corrected the typo in Supplement Line 101.

R1.C33 (Line 102 (S))

Missing reference?

Response: Agreed; this has been revised as suggested.

Changes in the manuscript: Added the missing reference at Supplement Line 102.

R1.C34 (Line 107 (S))

Again, missing reference?

Response: Agreed; this has been revised as suggested.

Changes in the manuscript: Added the missing reference at Supplement Line 107.

R1.C35 (Figure S9)

Should this not be 2025? – the final year of the simulated firn cover.

Response: The reviewer is correct. While revising this figure we identified two errors in the firn-cover model output: the final year (2026) was used as the reference year for computing time since firn loss, and firn layers exceeding the 20-year threshold were not correctly converted to ice. Both have been fixed, and all firn-related figures (Fig. 8, S10, S11) have been regenerated accordingly. We thank the reviewer for prompting this check.

Changes in the manuscript: Corrected the reference-year and 20-year firn → ice conversion errors in the firn-cover model; regenerated Fig. 8 and Supplementary Figs. S10, S11; updated affected firn-free durations in Sect. 5.1 (now 15–20 years at the uppermost boreholes to 25–30 years towards the terminus).

R1.C36 (Figure S9)

The spatial firn coverage in Fig. S9 does not seem to correlate with the data in Fig. S10 – nor to Figure 9 in the main body of the report. Also, generally white is very difficult to differentiate from a greyscale relief map.

Response: The apparent lack of correlation between the supplementary firn figures and the main-text figure was caused by the two model-output errors described in R1.C35, which have now been corrected; the firn extents in the regenerated figures are mutually consistent. Regarding legibility, we agree the white firn cover was hard to distinguish from the greyscale relief, and have adjusted the shaded-relief greyscale so that the firn cover stands out more clearly against the background.

Changes in the manuscript: Resolved the inconsistency via the firn-model corrections (R1.C35); adjusted the shaded-relief greyscale so the firn cover stands out more clearly.

R1.C37 (Figure S11)

AH1G (a) and AH3G (b) appear to be not so well equilibrated even a month after drilling - particularly with regard to the lowermost thermistor of the latter. This is also, despite these being very deep in the glacier. Going back to the prior comment about the potential influence of hot water drilling. I would have concerns that AH1G (a) could be interpreted to be heavily impacted. I'm also curious as to why there are heavy temperature fluctuations at such depths.

Response: We appreciate the reviewer's close reading of these profiles. We interpret the residual variability at AH1G and AH3G not as incomplete recovery from drilling, but as a genuine glaciological signal: a drilling disturbance would produce a monotonic decay toward equilibrium, whereas the behaviour seen here is episodic and persistent, consistent with latent-heat release from meltwater percolating and refreezing near the sensors. This is the same process we document elsewhere at Alphubel, where AH1G records a transient meltwater-driven warming during the melt season (Sect. 4.1). The deeper fluctuations are therefore, in our view, evidence of active englacial water movement rather than a drilling artefact, and are physically consistent with the temperate or near-temperate conditions inferred at these depths. By comparison, the boreholes drilled with the same system (Supplementary Fig. S15-16) are well equilibrated, which further argues against a systematic drilling-related bias. The general point that the drilling disturbance is local and short-lived, with sensors converging on stable temperatures, is now supported by text and references added to Sect. 3.3.1 (see also R1.C15).

Changes in the manuscript: No manuscript change specific to this figure; the drilling-disturbance clarification and citations were added to Sect. 3.3 (see R1.C15).

R1.C38 (Figure S12)

These by comparison (except the shallow thermistors) are much better equilibrated.

Response: We thank the reviewer for this observation, with which we agree. The good equilibration of these boreholes supports our response to comments R1.C15 and R1.C37: it demonstrates that the hot-water drilling disturbance dissipates within the excluded equilibration window, and that the residual variability seen at AH1G and AH3G (Fig. S11) reflects meltwater-driven processes rather than incomplete recovery from drilling, since these boreholes were drilled with the same system. We have retained this contrast, and it is now referenced in the discussion of drilling disturbance in Sect. 3.3.1 (see R1.C15).

Changes in the manuscript: No manuscript change; referenced in the drilling-disturbance discussion added to Sect. 3.3 (see R1.C15).

Sincerely,
Janosch Beer, on behalf of all co-authors

Revised and new figures

For reference, we reproduce below the revised and new figures referred to in our responses above; each caption also gives the figure's new number in the revised manuscript.

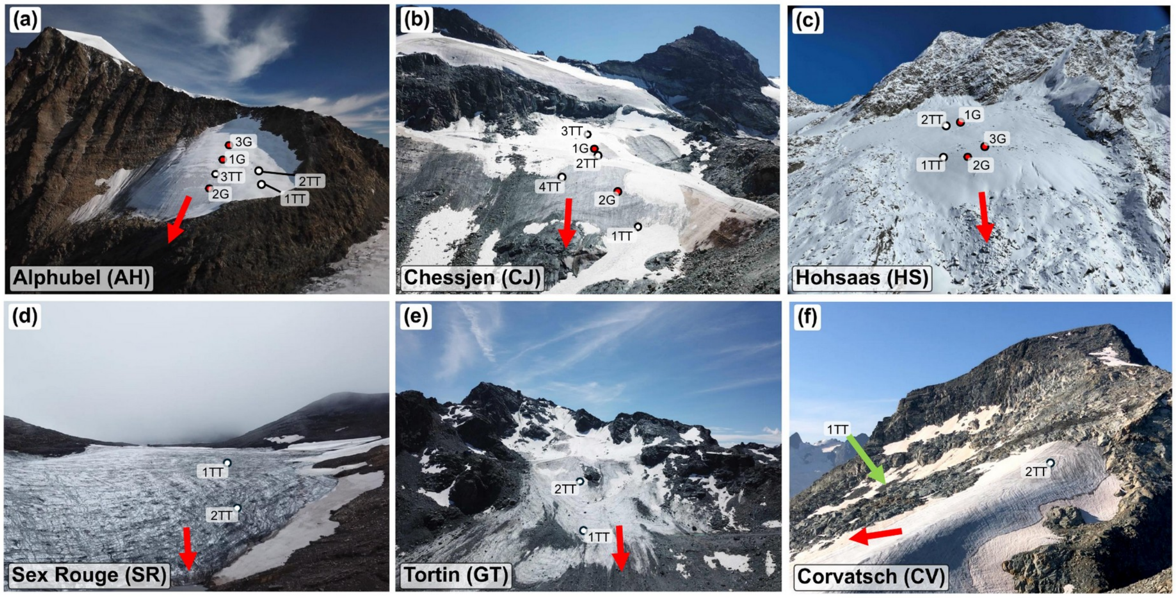


Figure 2 (revised). Overview images of the six study sites; an arrow now indicates the location of borehole CV1TT.

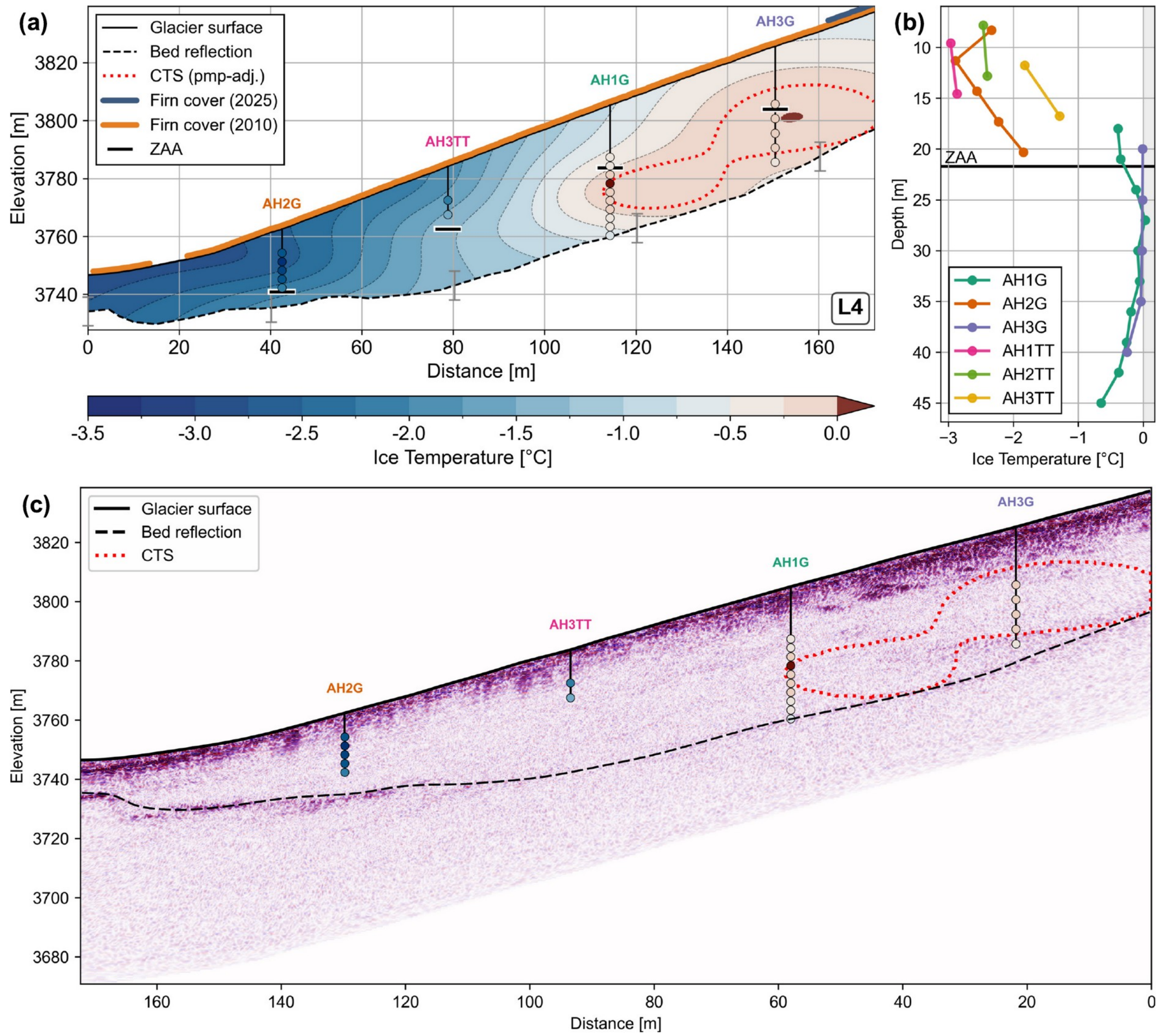


Figure 5, revised (Fig. 6 in the discussion paper). Englacial temperature structure and radar reflectivity along Alphubel profile L4.

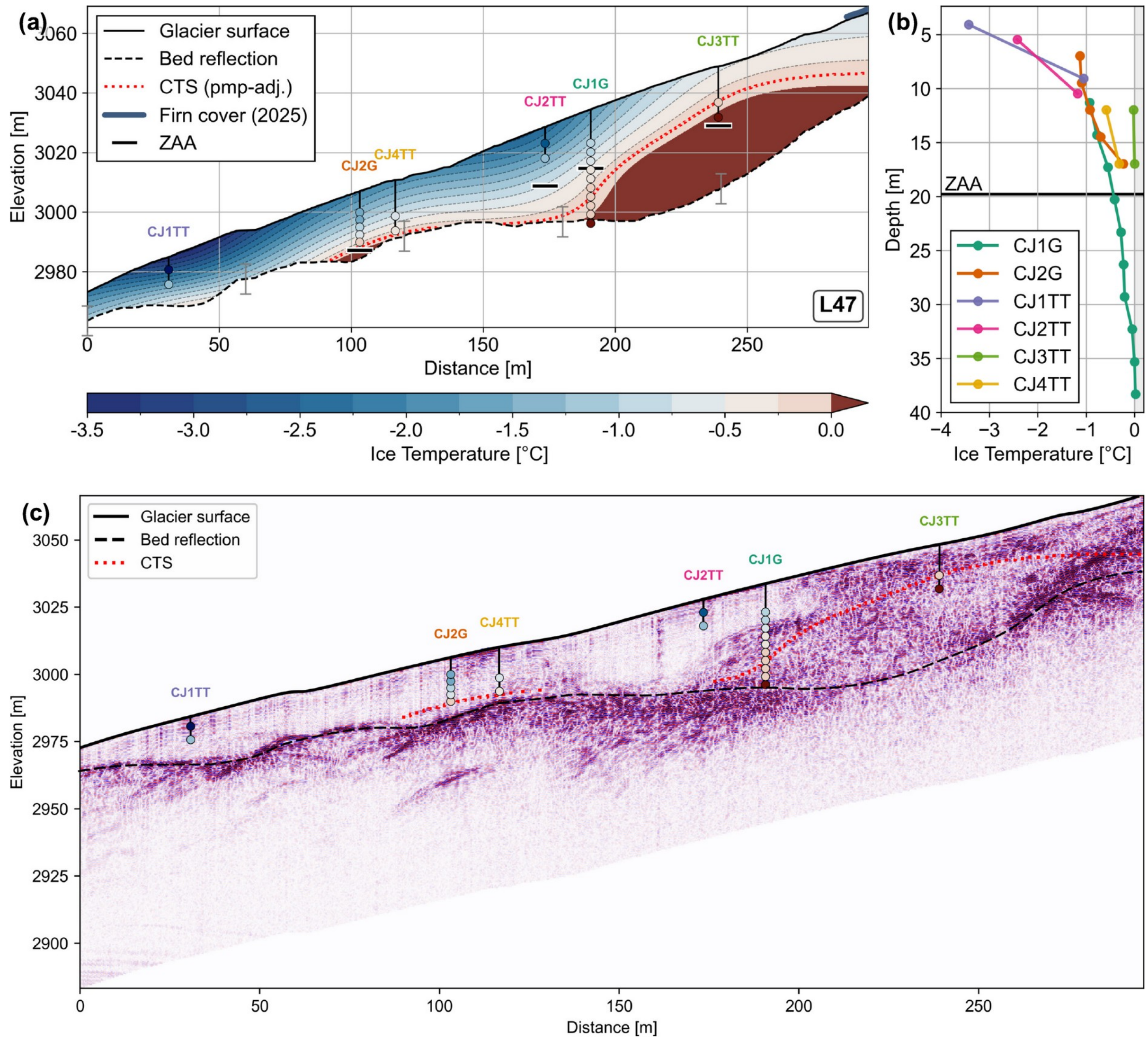


Figure 6, revised (Fig. 7 in the discussion paper). Englacial temperature structure and radar reflectivity along Chessjen profile L47.

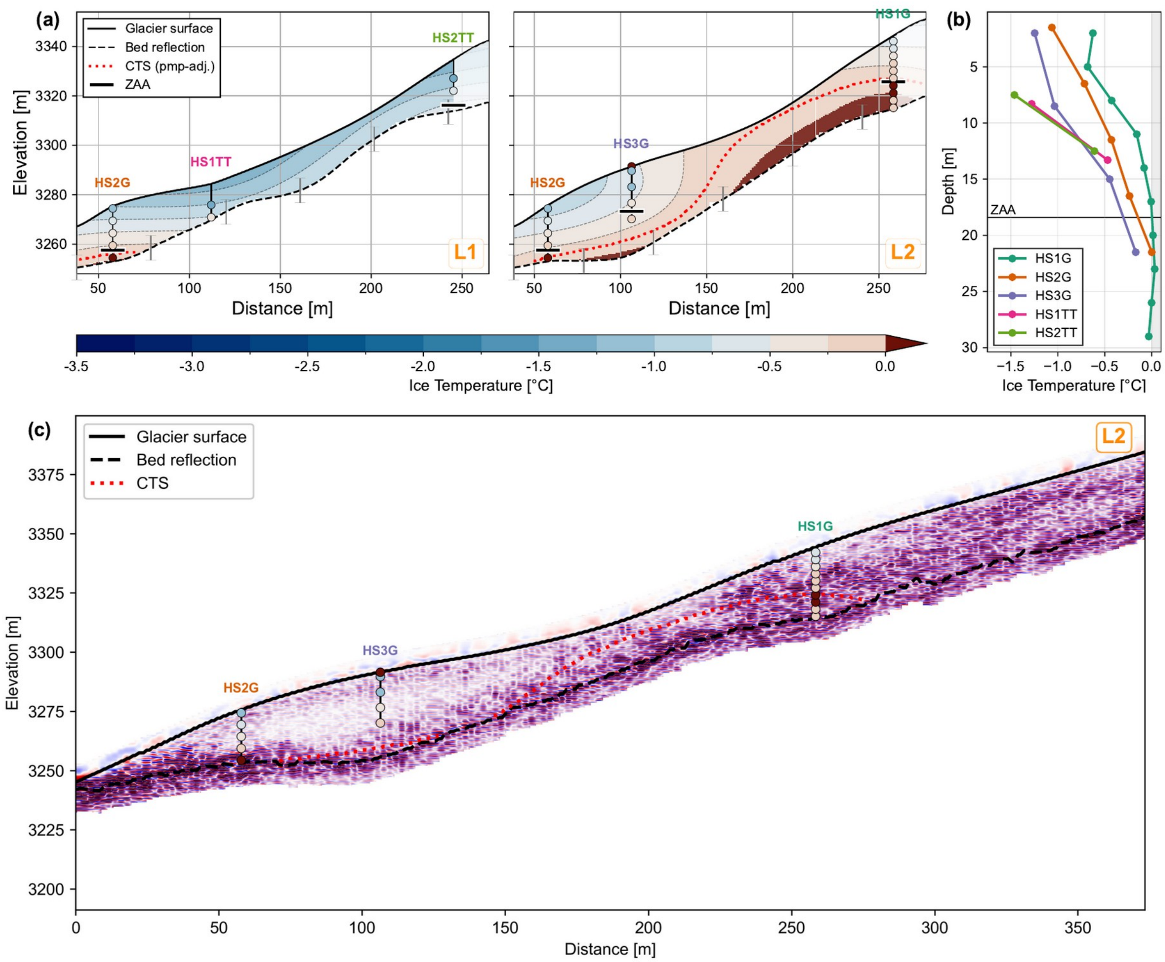


Figure 7, revised (Fig. 8 in the discussion paper). Englacial temperature structure and radar reflectivity at Hohsaas (profiles L1 and L2).

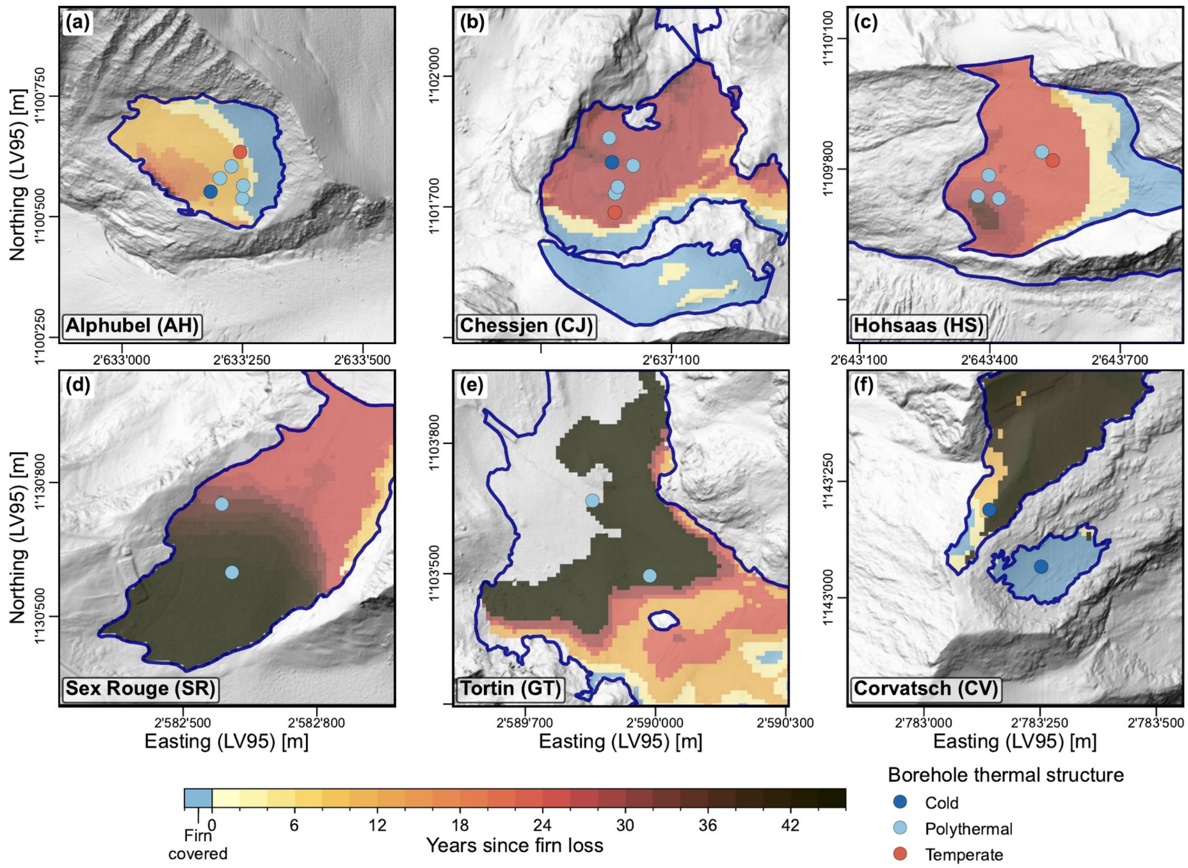


Figure 8, revised (Fig. 9 in the discussion paper). Number of years a given area was firn-free, mapped across all six study glaciers.

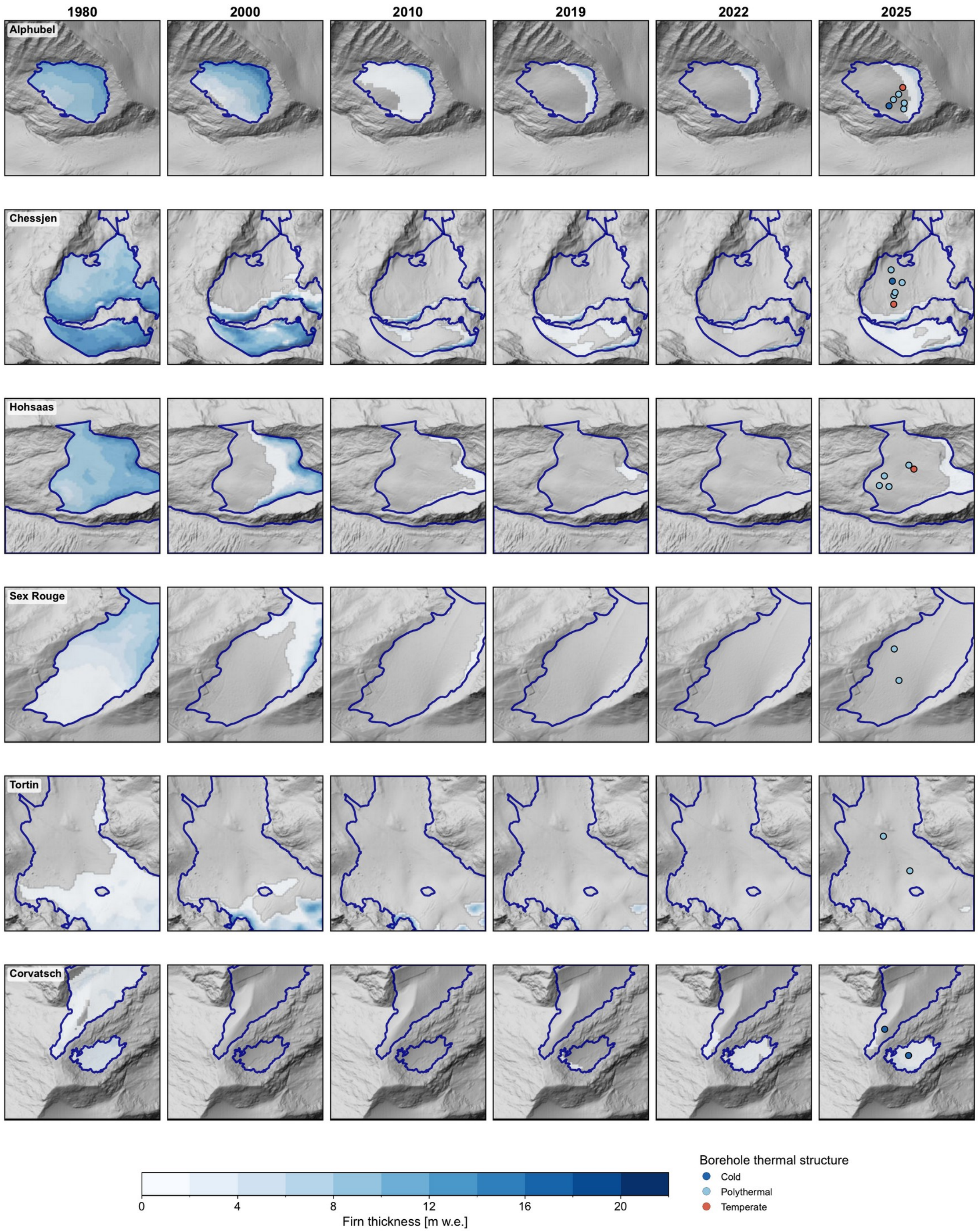
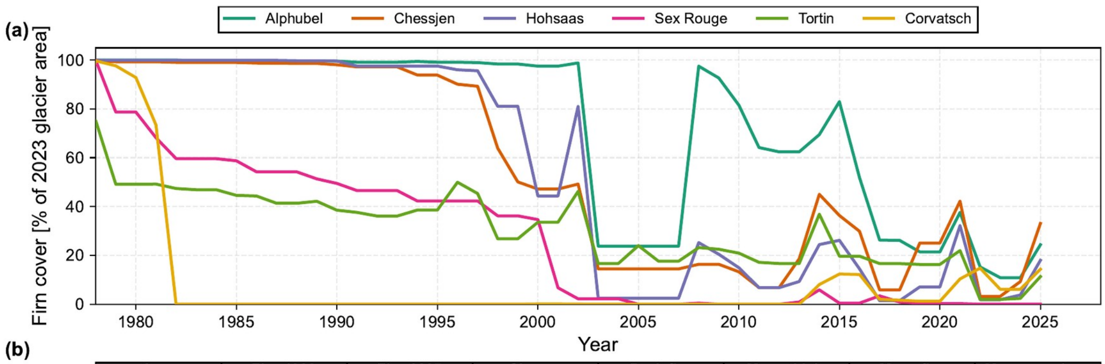


Figure S10, revised (Fig. S9 in the discussion paper). Modelled firn thickness at the six study sites for selected years (1980–2025).



(b)

	Area [km²]	Firn 1980 [km² / %]	Firn 2025 [km² / %]	Change 1980→2025 [%]	Peak firn [km² / yr]	Mean thick. 2025 [m w.e.]	Mean firn age [yr]	Mean yrs since firn loss
Alphubel	0.07	0.068 / 100	0.017 / 24	-76	0.068 / 1975	1.9	5.6	9.5
Chessjen	0.20	0.203 / 99	0.068 / 33	-67	0.203 / 1978	1.2	1.4	22.2
Hohsaas	0.15	0.150 / 100	0.027 / 18	-82	0.150 / 1976	1.0	1.2	19.8
Sex Rouge	0.22	0.174 / 79	0.000 / 0	-100	0.221 / 1978	—	—	33.6
Tortin	0.48	0.236 / 49	0.054 / 11	-77	0.361 / 1978	2.8	2.1	26.2
Corvatsch	0.17	0.153 / 93	0.024 / 14	-85	0.165 / 1978	0.9	2.1	40.5

Figure S11, revised (Fig. S10 in the discussion paper). Firn cover evolution and summary statistics for all six study sites.

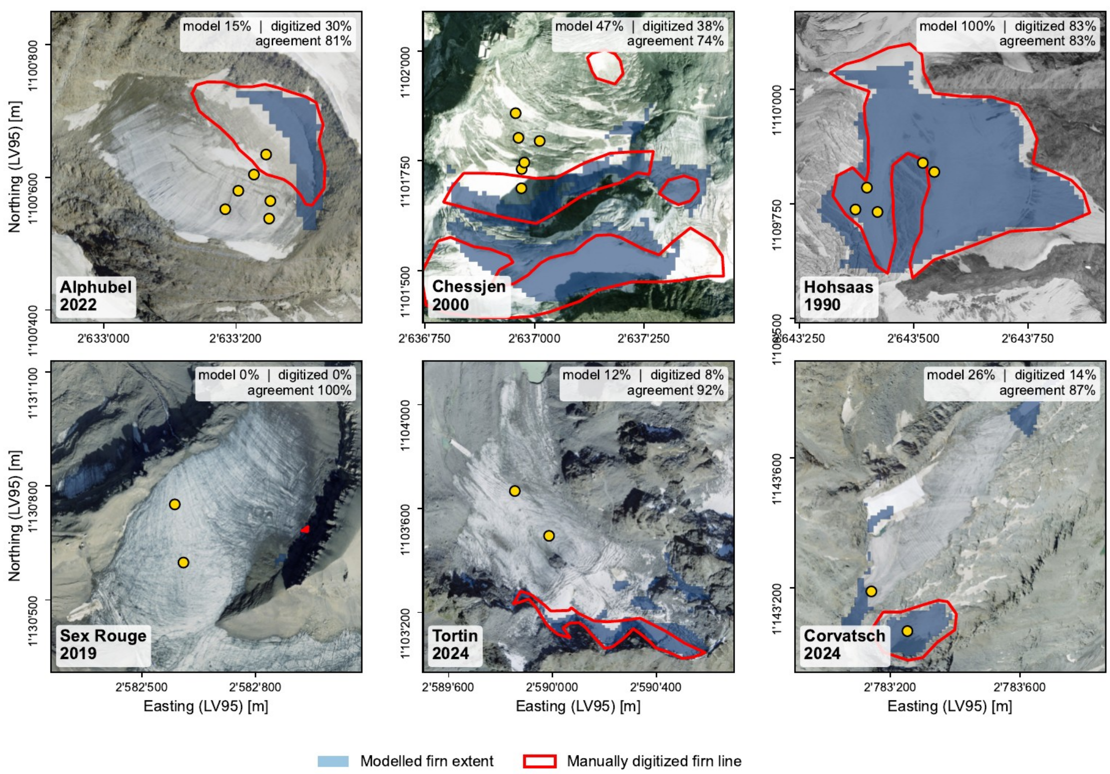


Figure S12 (new figure). Spatial comparison of modelled and manually digitized firn extent.

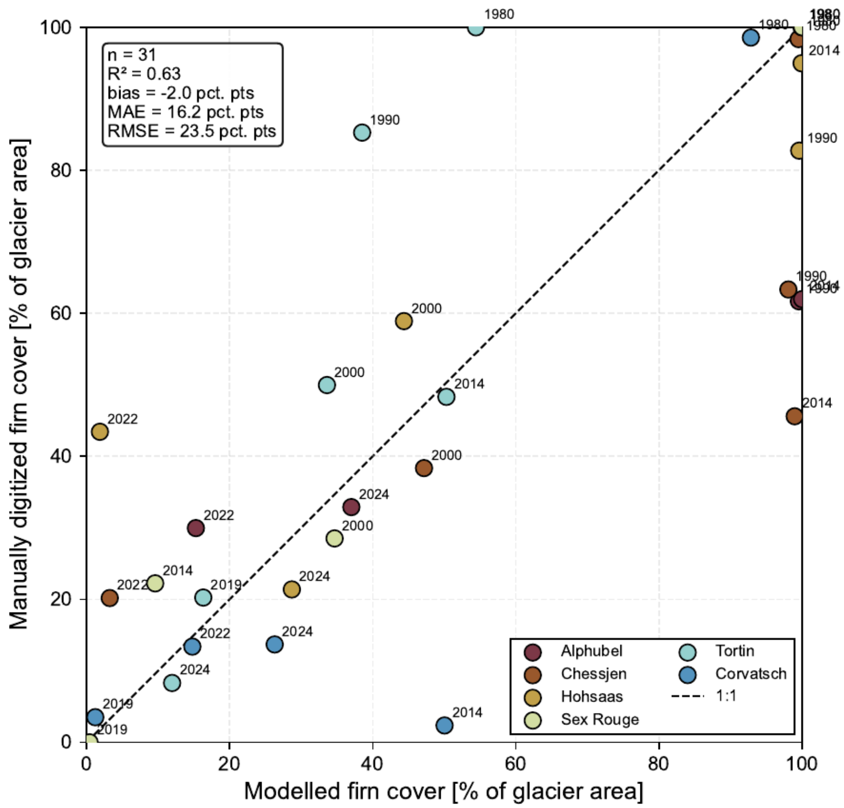


Figure S13 (new figure). Summary comparison of modelled versus manually digitized firn cover fraction.

Polythermal conditions in small glaciers in the Swiss Alps

Response to Referee #2

27 August 2026

Dear Dr. Caroline Clason & referee 2,

We thank the reviewer for your careful and constructive assessment of our manuscript. We greatly appreciate the time and effort you invested. Below we respond to each of your comments individually. Our replies are shown in blue, and the corresponding changes in the revised manuscript are quoted in green.

We have also increased the font size in Figure 5 (now Figure 4) to improve its readability.

Note on figure numbering: because the deployment-timeline figure was moved to the Supplement in this revision, the subsequent main-text figures shifted by one. The profile figures referred to in the comments as Figures 6–8 are now Figures 5–7 in the revised manuscript, the firn-loss map (Figure 9) is now Figure 8, and the seasonal-temperature and historical-comparison figures (Figures 4 and 5) are now Figures 3 and 4; several supplementary figures were renumbered accordingly. To match the comments, the responses below use the original numbering, and each revised figure reproduced at the end also gives its new number in the revised manuscript.

R2.S1 (Summary)

This manuscript presents a valuable new dataset of borehole thermometry and ground-penetrating radar observations from six small Swiss glaciers, addressing an important gap in our understanding of the thermal structure of small Alpine glaciers. The study has the potential to make a meaningful contribution to the field, particularly through the combination of direct temperature measurements, geophysical observations, and long-term mass balance records. However, I believe the manuscript would benefit from substantial revisions to improve its clarity, presentation, and interpretation before the conclusions can be fully supported. My principal comments relate to the overall organisation of the manuscript. This would improve the flow of the manuscript and make the interpretation of the thermal structures considerably easier to follow. In addition, the manuscript would benefit from greater methodological detail, particularly regarding calibration, and measurement uncertainties. Given that many of the reported temperature differences are small, a more explicit treatment of measurement uncertainty is important. Several figures could also be revised to improve clarity, including clearer symbology, integration of complementary datasets, and more effective figure layouts. Finally, more comprehensive engagement with the existing literature and additional supporting references would lend credence to the authors interpretations. The specific comments below are intended to help improve the clarity, robustness, and overall presentation of the manuscript. Overall,I think this is a noteworthy manuscript that has had a lot of work and time put into its data collection and analysis and with consideration of the points detailed below, would be an excellent fit for The Cryosphere.

Response: We thank the reviewer for the thorough and constructive assessment, and for recognising the value of the dataset and the contribution of the study. We have taken the principal comments seriously and made substantial revisions in each of the areas raised. On the overall organisation, we have restructured the Methods to lead with the geophysics as suggested, and we have split the former combined section into separate Results and Discussion sections, which together improve the flow and readability of the manuscript. On methodological detail, we have expanded the description of the instrumentation and, in particular, integrated the calibration information into the description of each thermistor type, reporting the mean offsets determined for each, and we have added an explicit statement of measurement uncertainty and made it explicit where reported differences are comparable to it. On the figures, we have substantially reworked the three principal figures (now Figs. 6 to 8) for clarity of symbology, added scale bars, moved the timeline figure to the supplement, and improved the firn-cover figures. On the literature, we have added supporting citations throughout the process descriptions, most substantially in the Discussion synthesis. We believe these revisions address the reviewer's concerns and have strengthened the clarity, rigour, and presentation of the manuscript, and we explain each change individually in the point-by-point responses below.

R2.C1 (General)

Structure of methods: As it stands, I feel the information within the methods section is good, but some restructuring could aid the reader in understanding the process. As it stands the thematic order is Drilling, Temperatures, GPR, Temperatures, Temperature interpolation, Mass balance modelling. I would suggest a minor restructure, beginning with the geophysics, then moving on to temperature measurements etc as this would be easier to follow.

Response: We thank the reviewer for this suggestion, which we have adopted. We have restructured the Methods section to lead with the geophysics, as proposed. The order is now: ground-penetrating radar surveys, then borehole temperature measurements (drilling and instrumentation), then the borehole temperature analysis, and finally the mass balance and firn cover reconstruction. Besides following the reviewer's recommended sequence, this reordering has two further benefits: it groups all temperature-related content together, rather than splitting it around the GPR section as before, and it establishes the glacier geometry from the GPR before the temperature interpolation that depends on it. We found the revised order does indeed read more fluidly and thank the reviewer for prompting the change.

Changes in the manuscript: Reordered Sect. 3: GPR surveys → borehole temperature measurements (drilling, instrumentation) → borehole temperature analysis → mass balance / firn-cover reconstruction; cross-references updated.

R2.C2 (General)

Results section: I find the results section hard to follow as it stands. Firstly, this should be clearly split into Results then Discussion as at the moment the mixing of the two makes it hard to follow. Secondly, I feel the data should either be presented as discrete datasets (temperature, GPR etc) or done by site integrating all datasets within one section to explain what is being observed at that glacier. I prefer the later of these options and although this will involve some restructuring the content will largely remain the same. Begin with the non-polythermal glaciers then move onto the polythermal sites which then makes the discussion flow better. You can then come together in the discussion section for an explanation of processes and comparison between sites (Sections 4.4, 4.5 and 4.6).

Response: We thank the reviewer for this detailed and constructive comment. It has two parts, which we have addressed differently. First, on splitting Results and Discussion: we agree, and have done so, dividing the former combined section into a Results section (Sect. 4: seasonal temperature variations, decadal changes at Sex Rouge and Corvatsch, and the polythermal-site thermal structure) and a separate Discussion section (Sect. 5: firn cover loss as the primary control, implications for Alpine thermal regimes, and hazard implications). Second, on reorganising the results site by site: we considered this carefully but have chosen to retain the thematic organisation, for reasons rooted in the paper's development. An earlier version of this manuscript was in fact structured site by site, as the reviewer suggests, but we found that this made the paper both repetitive, as each dataset and analysis step had to be presented separately for every glacier, and considerably longer and harder to follow, so we reorganised it thematically to improve conciseness and flow. A further consideration is that three of the six glaciers (Sex Rouge, Tortin, and Corvatsch) have only shallow boreholes with little or no bed control, so dedicated per-site sections for them would not be warranted. The thematic structure instead lets us present the seasonal signal across all six sites together, and then focus the detailed thermal-structure analysis on the three sites where we have measurements below the depth of zero annual amplitude (Alphubel, Chessjen, Hohsaas). We would also note that our Results already progress from the general seasonal signal across all sites, through the shallower Sex Rouge and Corvatsch comparison, to the detailed polythermal-site analysis, which partly follows the reviewer's suggestion of moving from the less complex sites toward the polythermal ones. We hope the reviewer agrees that the Results and Discussion split addresses the central concern, while the thematic organisation keeps the paper concise and readable.

Changes in the manuscript: Split the former “Results and discussion” into Results (Sect. 4: 4.1–4.3) and Discussion (Sect. 5: 5.1–5.3), renumbering and updating cross-references. Thematic organisation retained (see Response).

R2.C3 (General / Figs 6-8)

Following my above suggestion, I would advise integrating Figure 4 within Figures 6-8 for the relevant glaciers. This would allow us as the reader to see how the temperatures have evolved at depth over a season and how this then can be interpreted to the polythermal structure described. I also feel the symbology of these figures could be reworked to clearly distinguish what is a scale and what is a location/depth.

Response: We appreciate this suggestion, and reworking these figures for clarity was one of the more substantial revisions we made. On the symbology, the reviewer is right that it was not always clear what encoded a scale and what encoded a location or identity, and we have restructured the figures with exactly that distinction in mind. Ice temperature is now shown consistently through a single temperature colour map, used both for the interpolated field and for the borehole markers in panels (a) and (c), so that colour always means temperature. The individual borehole profiles in panel (b) are instead distinguished using a separate qualitative colour scheme from ColorBrewer (Harrower and Brewer, 2003), chosen for maximal categorical distinctness, so that borehole identity can no longer be mistaken for a temperature. We have also given the glacier surface, bed reflection, and CTS distinct and explicitly labelled line styles, and the ZAA depth is now marked both in panel (a) and in panel (b), directly showing which depths are seasonally variable and which represent the longer-term thermal structure. On integrating the seasonal time series (Fig. 4) into these figures, we gave this careful thought, as we agree that linking the seasonal evolution to the polythermal structure is valuable. On balance, however, we felt that adding it would overload figures that are already information-dense, and the seasonal behaviour is developed in its own right in Sect. 4.1. To capture the reviewer's underlying intent, that the reader should be able to see which part of the profile is seasonal, we added the ZAA depth to panels (a) and (b), which now shows directly which depths are seasonally variable and which reflect the longer-term thermal structure. We hope this offers a good compromise between completeness and legibility.

Changes in the manuscript: Revised Figs. 6–8 (see the captions quoted under R1.C25): single temperature colour map across panels (a)/(c); qualitative palette in panel (b); ZAA marked in panels (a) and (b); grey band for temperatures above 0 °C. Fig. 4 not embedded.

R2.C4 (General)

The authors describe processes in sufficient detail throughout the paper, however there are multiple instances where these processes need to be cited in the established literature and referenced throughout, lending credence to their argument.

Response: We fully agree that the process descriptions should be consistently grounded in the established literature, and this was a helpful prompt to strengthen the referencing throughout the manuscript. In response, we have added supporting citations at the specific points where processes are described but were previously under-referenced. Most substantially, the conceptual synthesis in Sect. 5.2 is now grounded throughout, with references for meltwater percolation and latent-heat release in firn (Gilbert et al., 2012; Huss and Fischer, 2016; Gastaldello et al., 2025), crevasse-mediated cryo-hydrologic warming (Phillips et al., 2010; Gilbert et al., 2019), strain heating (Cuffey and Paterson, 2010), and the cold-wave penetration that follows firn loss (Wilson and Flowers, 2013; van Pelt et al., 2016). We have likewise added references for the melt processes in the Introduction (Hock, 2005; see R2.C8), for the drainage-inhibition configuration underlying the hazard discussion (see R2.C32), and for the reduction of basal friction by elevated water pressure (Iken and Bindschadler, 1986; Cuffey and Paterson, 2010; see R2.C33). We are grateful for this comment, which we feel has strengthened the credibility of the process arguments throughout the paper.

Changes in the manuscript: Added targeted citations at the flagged process statements (principally Sect. 5.2; see R2.C30). No blanket changes.

R2.C5 (Line 4)

Perhaps name these 3 glaciers given you name other glaciers in the following sentence.

Response: Agreed; this has been revised as suggested.

Changes in the manuscript: Abstract: “We confirm polythermal conditions in three glaciers — Alphubel South, Chessjengletscher, and Hohsaasgletscher — …”.

R2.C6 (Line 23)

I suggest re-phrasing this not to include "high-altitude" as this is a comparative term, perhaps around a specific altitude or just use the term accumulation zone.

Response: We agree and have removed the comparative term 'high-altitude', replacing it with a specific elevation threshold and broadening the cited examples (see R1.C4, which addresses the same sentence).

Changes in the manuscript: Introduction (Line 23): "In the Alps, existing observations are biased toward accumulation areas above ~3800 m a.s.l., including sites in the Monte Rosa, Mont Blanc, and Bernese Alps massifs (…) (Supplementary Fig. S1)…".

R2.C7 (Line 33)

Unclear whether you are referring to a comparison between different glaciers or different components of the same glacier

Response: We agree the phrasing was ambiguous. The revised sentence makes clear that this is a comparison between different glaciers, not between components of the same glacier (see also our response to R1.C7, which addressed the same sentence).

Changes in the manuscript: Line 33: "At these lower elevations (< 3800 m a.s.l.), glaciers may respond to atmospheric warming differently from higher-elevation glaciers that still retain firn-covered accumulation areas, because firn loss alters how latent heat from meltwater is retained."

R2.C8 (Line 34)

I suggest referencing: Hock 2005 and Brock et al., 2010

Response: We have added Hock (2005) as a general reference for the melt processes invoked here. We note that the specific process referred to in this sentence, reduced meltwater retention and the associated loss of latent heat from refreezing where firn is absent, is supported by the citations in the immediately following sentence (Wilson and Flowers, 2013; van Pelt et al., 2016). Regarding Brock et al. (2010), which concerns surface energy fluxes on a debris-covered glacier, we felt it was not a natural fit for this statement about firn meltwater retention; we have instead considered it in the context of the reviewer's comment on debris cover (R2.C10).

Changes in the manuscript: Line 34: added Hock (2005): "…as firn loss fundamentally alters how latent heat from meltwater is retained (Hock, 2005)."

R2.C9 (Line 38)

This has been extensively researched in Antarctica so perhaps rephrase around comparatively little research in alpine and high-mountain environments?

Response: We agree and have narrowed the scope of this statement, replacing 'comparatively little attention elsewhere' with 'comparatively little attention in alpine and high-mountain environments,' which more accurately delimits the claim. We have deliberately not added an Antarctic citation here, however. While meltwater refreezing and latent-heat release are indeed extensively studied in Antarctica, that body of work concerns firn processes on ice shelves and ice sheets (firn-air depletion, meltwater ponding, and their role in ice-shelf stability and collapse), where refreezing warms the firn and the setting and thermal consequences differ fundamentally from the ablation-zone cooling of small, firn-losing mountain glaciers that we address. Citing that literature here would therefore risk conflating two distinct processes. The rephrasing acknowledges that the broader process is not unstudied globally, while keeping the specific claim accurate, namely that the ablation-zone cooling effect is understudied in alpine settings, and appropriately supported by the alpine and Arctic references cited.

Changes in the manuscript: Line 38: "…it has received comparatively little attention in alpine and high-mountain environments (Gilbert et al., 2012; Huss and Fischer, 2016)."

R2.C10 (Line 48)

this is true for alpine environments but consider the role of debris cover in high-altitude glaciers and perhaps make this statement more specific.

Response: We thank the reviewer for the suggestion. The reviewer is correct that debris cover can influence surface melt and the thermal regime on glaciers where it is present, and that its role can be significant for debris-covered glaciers. At our six study sites, however, debris cover is negligible to absent, so it does not play a meaningful role for the glaciers examined here. The statement at Line 48 describes a drainage-inhibition mechanism established in the cited literature for clean-ice polythermal glaciers, which is the setting relevant to our study. We have therefore kept the statement as is and consider a treatment of debris-covered glaciers to be outside the scope of this work.

Changes in the manuscript: No manuscript change.

R2.C11 (Line 82)

I would like to see some more information here, namely how were the holes drilled, what thermistors did you use and what is its uncertainty, were the thermistors calibrated prior to deployment, what depths were the thermistors deployed to? I appreciate some of this information is in S.I. however, a sentence or two would help me understand your method. I now see this is explained partially later on but I still think some details here could help.

Response: We appreciate this comment and understand the reviewer's wish for more detail at this point, as the paragraph could indeed read as though it were already introducing the methods in full. Our intention with this opening paragraph is different: it is meant to give the reader a broad, chronological overview of what was carried out and when across the two field campaigns, which we consider important for the reader to be able to place the later results and interpretations in context. The methodological detail the reviewer asks for (drilling systems, thermistor types and accuracy, calibration, GPR, and mass-balance methods) is provided in full in the respective subsections. To make the purpose of the paragraph clearer and avoid the impression that it is already a detailed methods description, we have restructured its opening so that it now explicitly introduces itself as a chronological overview, with a forward reference to the detailed subsections (Sects. 3.1 to 3.4).

Changes in the manuscript: Rewrote the opening of Sect. 3: "We conducted two field campaigns, in the summers of 2024 and 2025. Before detailing the individual methods (Sects. 3.1–3.4), we first give a chronological overview of this fieldwork."

R2.C12 (Line 85)

please define deep in the context of this paper

Response: We have clarified the terminology at first use. In this study, shallow boreholes refer to the approximately 15 m holes drilled with the Heucke steam drill in 2024, and deep boreholes refer to the hot-water-drilled holes reaching the glacier bed in 2025, with depths of 20 to 58.3 m. This distinction has been made explicit at Line 85.

Changes in the manuscript: Line 85: "…the drilling of additional deep boreholes (here defined as bed-reaching boreholes of 20 to 58.3 m depth) at Alphubel, Chessjen, and Hohsaas."

R2.C13 (Figure 1)

I appreciate some scale is apparent given the gripping of panels b-g, however for easier comparison please could you add a scale bar to each panel.

Response: We agree and have added a scale bar to each panel of Figure 1, placed in the lower right of each panel, to allow easier comparison between the sites."

Changes in the manuscript: Added a scale bar to the lower right of each panel (b–g) of Figure 1.

R2.C14 (Figures 1 & 2)

I wonder if these figures could be combined with the overview images being added as inserts? I appreciate this may make the figure messy but could be worth considering.

Response: We thank the reviewer for the suggestion and have considered it carefully. As the reviewer anticipates, we found that combining the two figures would overload what is already a very densely populated figure. Figure 1 currently carries the overview map, six detailed site maps with contour lines, glacier outlines, borehole positions, flow directions, and gpr profiles, and adding photographic inserts would compromise its legibility. We also feel the two figures serve distinct and complementary purposes: Figure 1 provides the spatial and instrumentation context (site locations, topography, and borehole and profile positions), while Figure 2 provides the visual context of each site through photographs showing the actual terrain and borehole settings. For these reasons we prefer to keep them as two separate figures.

Changes in the manuscript: No manuscript change (figures kept separate).

R2.C15 (Figure 3)

Move to SI

Response: We agree and have moved Figure 3 to the Supplement, where it now appears as Fig. S3 under a new subsection '2.1 Timeline of borehole instrumentation.

Changes in the manuscript: Moved Figure 3 to the Supplement (now Supplementary Fig. S2); in-text references updated (see R1.C16).

R2.C16 (Line 117)

was this accuracy from calibration you conducted or from the manufacturer? This is quite low accuracy considering the data you are analysing

Response: The stated accuracy is the manufacturer specification for the Tinytag PB-5010 thermistors. In addition, we calibrated all sensors ourselves prior to deployment to quantify and correct sensor-specific offsets (Lines 126 to 130), which reduces the effective offsets below this nominal value. We have added '(manufacturer specification)' at Line 117 to make this explicit. We would also note that, while ±0.2°C is a relatively low accuracy in absolute terms, the temperature contrasts underlying our interpretations are generally substantially larger than this, and the deepest, bed-reaching measurements are additionally constrained by the higher-accuracy Geoprecision chains (±0.05°C). Where individual values fall within sensor accuracy of the melting point, we refrain from a definitive classification and interpret them with appropriate caution.

Changes in the manuscript: Added "(manufacturer specification)" after ±0.2 °C at Line 117 (and after ±0.05 °C at Line 125, per R2.C17).

R2.C17 (Line 125)

was this accuracy from calibration you conducted or from the manufacturer?

Response: As with the Tinytag sensors, the stated accuracy is the manufacturer specification, here for the Geoprecision TNode HD thermistors. We have added '(manufacturer specification)' after the ±0.05°C figure at Line 125 to make this explicit. In addition, all sensors were calibrated prior to deployment to quantify and correct sensor-specific offsets (Lines 126 to 130), which reduces the effective offsets below this nominal value. The ±0.05°C accuracy of these chains is at the high-accuracy end of what is achievable for englacial point thermometry, and the deep vertical profiles and cold-temperate transition detection rely primarily on these sensors.

Changes in the manuscript: Added "(manufacturer specification)" after ±0.05 °C at Line 125.

R2.C18 (Lines 126-130)

This information answers my above questions but please integrate it within the relevant text for each thermistor, which would make it easier to follow from the readers perspective. State how your calibration improved the uncertainty for each.

Response: We have integrated the calibration information into the description of each thermistor type, as suggested, rather than keeping it in a separate paragraph. The ice-bath calibration is now described with the Tinytag sensors and the alcohol-bath calibration with the Geoprecision chains. For each we state the mean sensor offset determined by calibration: 0.3°C for the Tinytag sensors, which exceeds their typical accuracy and therefore represents a meaningful correction, and −0.004°C for the Geoprecision chains. The detailed calibration procedure and the statistical overview of the offsets remain in Supplement Sect. 2.3.

Changes in the manuscript: Sect. 3.1.2: for the Tinytag: "Prior to deployment, we calibrated these sensors in an ice-bath setup … The mean offset was 0.3 °C, larger than the nominal accuracy and thus a meaningful correction." For the Geoprecision: "These chains were calibrated in a temperature-controlled alcohol bath … The mean offset was only −0.004 °C."

R2.C19 (Line 136)

Why did you change antenna frequency?

Response: We understand the reviewer's point that the sentence, as written, states the different antenna frequency at Alphubel without explaining why. We have therefore added the rationale for the choice. We used 100 MHz antennas at most sites as a conservative choice, providing good-quality bedrock reflections given the expected ice thickness and the scattering from englacial water, while at Alphubel the steep terrain favoured the more compact 250 MHz antennas, which still imaged the bedrock reliably.

Changes in the manuscript: Sect. 3.2 (GPR): "The choice of antenna frequency balanced penetration and robustness against scattering on the one hand, and ease of handling on the other. We used 100 MHz antennas at most sites as a conservative choice, ensuring good-quality bedrock reflections given the expected ice thickness and the scattering from englacial water and inhomogeneities. At Alphubel, the steep terrain favored the more compact 250 MHz antennas which still imaged the bedrock reliably."

R2.C20 (Line 137)

What was the uncertainty of this receiver? Was this carried by the operator or mounted on the radar? If carried by the operator has the data been offset?

Response: We have added the requested information. The Leica differential GNSS operated in real-time kinematic (RTK) mode, providing centimetre-level positioning accuracy, and antenna elevations were corrected for the offset between the GNSS and radar antennas and for the antenna height above the ground surface. To put this in perspective, the centimetre-level positioning uncertainty is negligible compared with the dominant geometric uncertainty in our analysis, the ±5 m bedrock-depth uncertainty, which arises from the manual picking of the basal reflection and the vertical resolution of the radar.

Changes in the manuscript: Sect. 3.2: "Antenna positions were tracked with a Leica differential GNSS receiver operating in real-time kinematic (RTK) mode, providing centimeter-level positioning accuracy. Antenna elevations were corrected for the offset between the GNSS and radar antennas and for the antenna height above the ground surface." Bedrock-depth uncertainty now stated explicitly: "We estimate the resulting bedrock-depth uncertainty at approximately ±5 m, arising from the manual picking of the basal reflection and the vertical resolution of the radar."

R2.C21 (Line 144)

How high a precision, do you have some measure of uncertainty?

Response: We have added the positioning uncertainty of the UAV GNSS system. The D-RTK 2 mobile station provides centimeter-level RTK positioning, with a nominal accuracy of 1 cm + 1 ppm horizontally and 2 cm + 1 ppm vertically. As for the ground-based GNSS, this precision is well below the dominant source of geometric uncertainty in our analysis, the ±5 m bedrock depth, and is therefore not a limiting factor for the results.

Changes in the manuscript: Sect. 3.2: "…a D-RTK 2 high-precision GNSS mobile station (DJI), providing centimeter-level RTK positioning (nominal accuracy 1 cm + 1 ppm horizontal, 2 cm + 1 ppm vertical)."

R2.C22 (Line 212)

Split results and Discussion

Response: As described in our response to R2.C2, we have split the former combined section into separate Results (Sect. 4) and Discussion (Sect. 5) sections.

Changes in the manuscript: Split into Results (Sect. 4) and Discussion (Sect. 5); see R2.C2.

R2.C23 (Line 218)

please add uncertainty to these and all measured temperature values, especially given the magnitude of your data.

Response: We agree that measurement uncertainty should be stated explicitly. Rather than appending an uncertainty to every value, which would impair readability, we have added a statement of the uncertainty convention in Sect. 4.1: reported temperatures carry an uncertainty corresponding to the calibrated sensor accuracy, ±0.2°C for the Tinytag and ±0.05°C for the Geoprecision sensors. We have also added the Tinytag reading resolution (0.02°C) in Sect. 3.1.2, so that both sensor types now have a stated resolution alongside their accuracy (the Geoprecision resolution, 0.0001°C, was already given). We note that these accuracies refer to absolute temperature values; differences between sensors are resolved considerably more finely, since the systematic offsets are removed by calibration and the resolution is well below the reported differences.

Changes in the manuscript: Added to Sect. 4.1: "Unless stated otherwise, reported temperatures carry an uncertainty corresponding to the calibrated sensor accuracy, ±0.2 °C for the Tinytag and ±0.05 °C for the Geoprecision sensors." Added the Tinytag reading resolution (0.02 °C) in Sect. 3.1.2.

R2.C24 (Line 221)

what data is this referring to, please reference a figure

Response: The described amplitudes and depth-dependent phase lag are derived from the seasonal temperature time series shown in Fig. 4 panel a-d. We have added an explicit reference to Fig. 4 at this point in the text.

Changes in the manuscript: Line 221: "…the temperatures at depths of 10 m to 15 m lagged roughly six months behind the surface (Fig. 4 a–d)…".

R2.C25 (Sections 4.2 & 4.3)

I would present these sections clearly differentiating your results from your interpretation (whether it is or isn't polythermal) to more easily split the results.

Response: We have split the manuscript into separate Results (Sect. 4) and Discussion (Sect. 5) sections, with the mechanistic interpretation of the thermal structures, why they arise and what they imply, now developed in the Discussion. We would like to be transparent, however, that we have deliberately retained a level of direct, on-the-spot interpretation within the Results, in particular the classification of each site and borehole as temperate, cold, or polythermal directly from the measured profiles. In our view, fully separating the temperature values from their immediate meaning, for instance stating that a borehole reached 0°C without noting that this indicates temperate ice, would make the Results substantially harder to follow, and this kind of direct, data-level reading is difficult to disentangle from the presentation of the observations themselves. We have therefore kept the immediate interpretation of what each measurement shows within the Results, while reserving the broader interpretation of why these thermal structures develop, and their wider implications, for the Discussion. We hope this provides the clearer separation the reviewer is looking for while keeping the Results readable.

Changes in the manuscript: Split Results/Discussion (see R2.C2). Direct thermal classification kept in Results (Sects. 4.2–4.3); mechanistic interpretation moved to Discussion (Sect. 5).

R2.C26 (Lines 296-314)

This section on GPR appears to me without a clear lead on, please see my suggestion for restructuring in main comments.

Response: We agree the GPR results would read better with a clearer lead-in. This is now improved in two ways. First, following the restructuring of the Methods to lead with the geophysics (see our response to R2.C1), the GPR approach and its purpose as a spatially continuous proxy for the englacial thermal structure are established before the reader reaches the GPR results. Second, we have added a lead-in sentence to the GPR paragraph in the Results, framing it explicitly as complementing the borehole point measurements with a spatially continuous view before the site-by-site comparison. Together with the separation of Results and Discussion (R2.C2), we believe the GPR results now follow more clearly from what precedes them.

Changes in the manuscript: Added to the polythermal-sites Results: “To complement the point measurements from the boreholes with a spatially continuous view of the thermal structure, we next consider the radar profiles.”

R2.C27 (Section 4.4)

Here you are presenting interpretations before clearly laying out all of your results (GPR sections etc). This should be clearly split.

Response: This is resolved by the Results and Discussion split. The section in question (former Sect. 4.4, firn cover loss as the primary control) is now the opening section of the Discussion (Sect. 5) and therefore follows the full presentation of the results, rather than preceding it.

Changes in the manuscript: Former Sect. 4.4 is now Sect. 5.1 (first Discussion section); see R2.C2.

R2.C28 (Pages 16-18)

small lines of text are hard to follow. Collate this text on a separate page top the figures.

Response: We understand the reviewer's point that the body text on these pages is fragmented into short segments around the large figures (Figs. 6 to 8), which makes it hard to follow. This resulted from the automatic placement of these full-width figures in the preprint layout. For the revised manuscript we have adjusted the placement of these three figures so that the surrounding text runs more continuously and is easier to follow. We note that the final layout of the figures and text is determined during the journal's typesetting of the accepted manuscript, which will further optimise their arrangement.

Changes in the manuscript: Adjusted the float placement of Figs. 6–8 (now [tp]) so body text runs continuously.

R2.C29 (Figures 6-8)

At present I am finding these figures hard to follow and my main questions are: • Is the black dashed line in panel A and C the isotherm or glacier bed, this is unclear when compared to panel B • I would invert your temperature scale so it increases as it moves right across the page, this is more standard and coincidentally is similar to the profiles drawn in panel A which will make it easier to follow. • I cannot see seasonal changes in the figure given that it is average, consider integrating Figure 4 within this as this will make the changes clearer • Symbology between each panel could be more distinct.

Response: We have reworked all three figures (Figs. 6 to 8) to address these points. (1) We have removed the ambiguity around the dashed line: previously a dashed line denoted the 0°C isotherm in panel (b) but the bed reflection in panels (a) and (c). We have replaced the dashed 0°C isotherm in panel (b) with a shaded grey band, so a dashed line now consistently denotes only the bed reflection throughout, and the panel legends explicitly identify the glacier surface (solid), bed reflection (dashed), and CTS (red dotted). (2) We have inverted the temperature axis in panel (b) so that temperature increases from left to right, consistent with the adjacent panels. (3) Regarding the seasonal changes, we have added the depth of zero annual amplitude (ZAA) both to panel (a), as ticks per borehole, and to panel (b), as a horizontal line, so that the reader can see directly in both views which depths are subject to seasonal variability and which are representative of the longer-term thermal structure. We would prefer, however, not to integrate the seasonal time series (Fig. 4) into these figures: they are already densely loaded, and the seasonal behaviour is presented and discussed separately in Sect. 4.1. Combining the two would, in our view, reduce rather than improve the clarity of both. (4) We have also made the symbology more distinct and consistent between panels, including a consistent spatial orientation between panels (a) and (c).

Changes in the manuscript: Revised Figs. 6–8 (captions quoted under R1.C25): grey band for temperatures above 0 °C and dashed line reserved for the bed reflection; inverted panel-(b) temperature axis; ZAA in panels (a) and (b); consistent panel symbology.

R2.C30 (Lines 369-382)

An interesting discussion of the processes but this should be grounded in the established literature and referenced throughout, lending credence to your argument.

Response: We agree that this synthesis paragraph should be more explicitly grounded in the established literature. The mechanisms it describes are supported by the studies cited in the preceding Sect. 5.1, and we have now added the relevant references directly within this paragraph at the points where each process is invoked: meltwater percolation and latent-heat release in firn (Gilbert et al., 2012; Huss and Fischer, 2016; Gastaldello et al., 2025), crevasse-facilitated meltwater access and the associated latent-heat warming at depth (Phillips et al., 2010; Gilbert et al., 2019), strain heating (Cuffey and Paterson, 2010), and latent-heat loss following firn loss (Wilson and Flowers, 2013; van Pelt et al., 2016). We have additionally introduced the concept of cryo-hydrologic warming (Phillips et al., 2010) to explicitly name the crevasse-mediated latent-heat process central to our argument, with Gilbert et al. (2019) providing the mountain-glacier context for this mechanism.

Changes in the manuscript: Added the above citations at the corresponding statements in Sect. 5.2, and introduced cryo-hydrologic warming, e.g.: "…creating englacial entry points for meltwater and further supporting temperate conditions throughout the ice column via cryo-hydrologic warming (Phillips et al., 2010; Gilbert et al., 2019)."

R2.C31 (Line 383)

This is not high elevation when compared to some other glaciated regions, rephrase.

Response: We use "high-elevation" here relative to European Alps, which is the relevant frame of reference for a study of Swiss Alpine glaciers, rather than in a global cross-regional sense. Within the Alps, Alphubel (up to 3890 m a.s.l., on the south face of a 4000 m peak) is high-elevation by any regional standard and is the highest of our six study sites. We do not intend a comparison with other glaciated mountain ranges, where absolute elevation standards differ substantially and are not relevant to the regional scope of this study, and have therefore retained the original wording. We note a related comment from the other reviewer regarding the elevation framing at line 413, which we have addressed separately by sticking to the actual elevation range rather than relative labels; for this instance specifically, we retain "high-elevation" since it refers to Alphubel's position within the regional context of the Alps.

Changes in the manuscript: No change at Line 383 (see Response); the related Line 413 label was removed (R1.C29).

R2.C32 (Line 397)

reference required

Response: We have added references supporting this configuration, citing Wadham et al. (2001) and Vincent et al. (2012) for the drainage-inhibition mechanism, alongside Dutto et al. (1991), Bondesan and Francese (2023), and Jacquemart et al. (2024) for documented cases of hazardous polythermal configurations.

Changes in the manuscript: Line 397: "…a frozen terminus combined with a temperate upper glacier (Dutto et al., 1991; Wadham et al., 2001; Vincent et al., 2012; Jacquemart et al., 2024)."

R2.C33 (Line 400)

reference required

Response: We have added references for the relationship between elevated subglacial water pressure and reduced basal friction. The statement now cites Iken and Bindschadler (1986) and Cuffey and Paterson (2010), which describe how high subglacial water pressure lowers the effective pressure at the bed and thereby reduces basal drag.

Changes in the manuscript: Line 400: "…basal friction may be substantially reduced locally (Iken and Bindschadler, 1986; Cuffey and Paterson, 2010)."

Sincerely,
Janosch Beer, on behalf of all co-authors

Revised and new figures

For reference, we reproduce below the revised and new figures referred to in our responses above; each caption also gives the figure's new number in the revised manuscript.

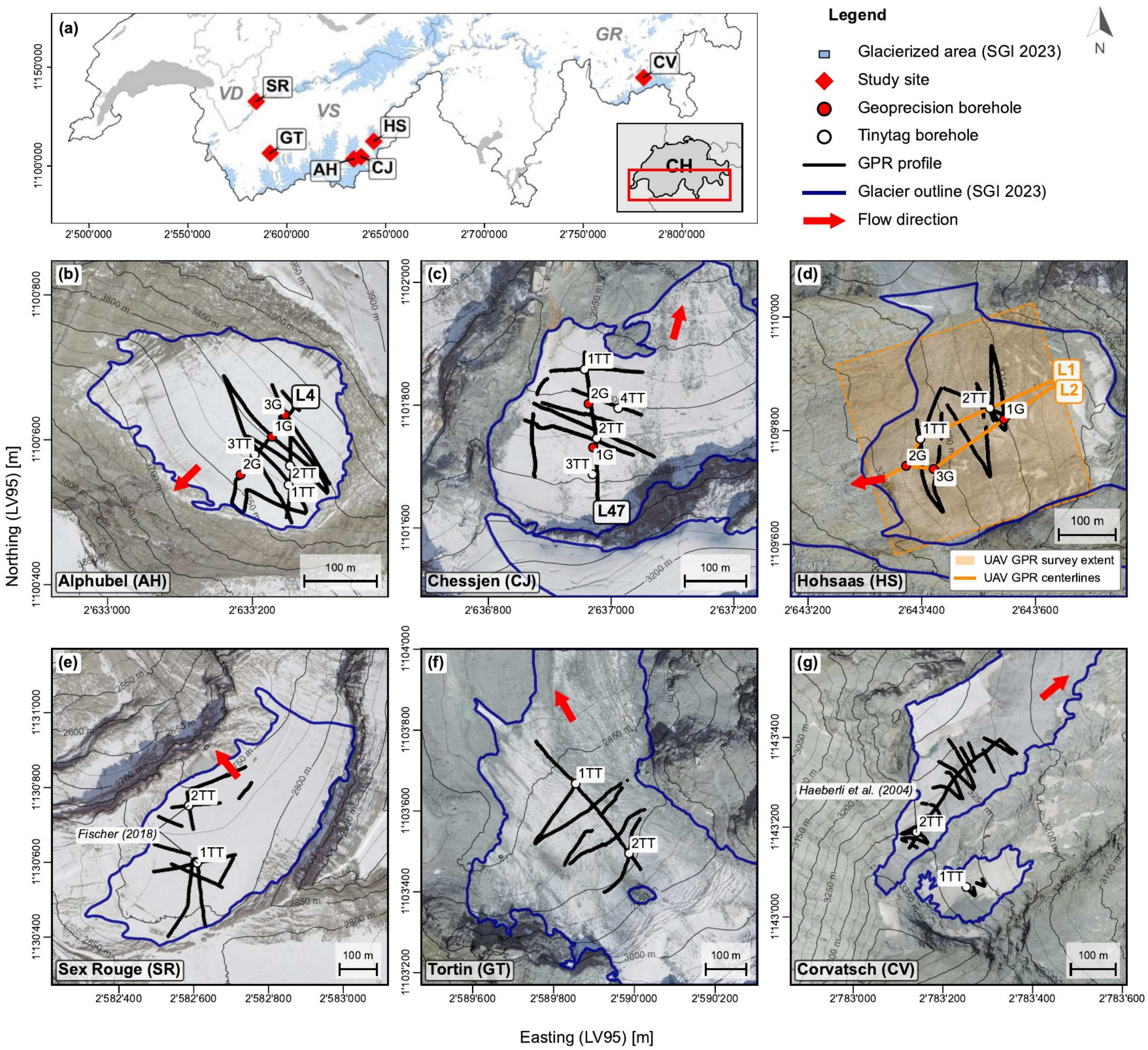


Figure 1 (revised). Overview and detailed maps of the six study glaciers; scale bars added to each panel.

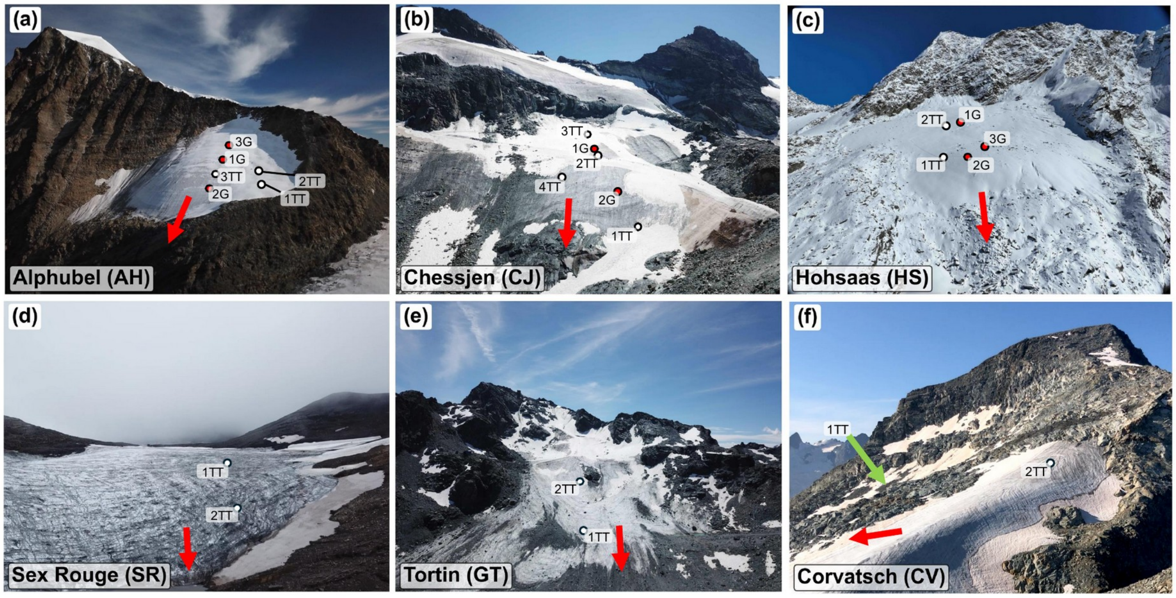


Figure 2 (revised). Overview images of the six study sites; an arrow now indicates the location of borehole CV1TT.

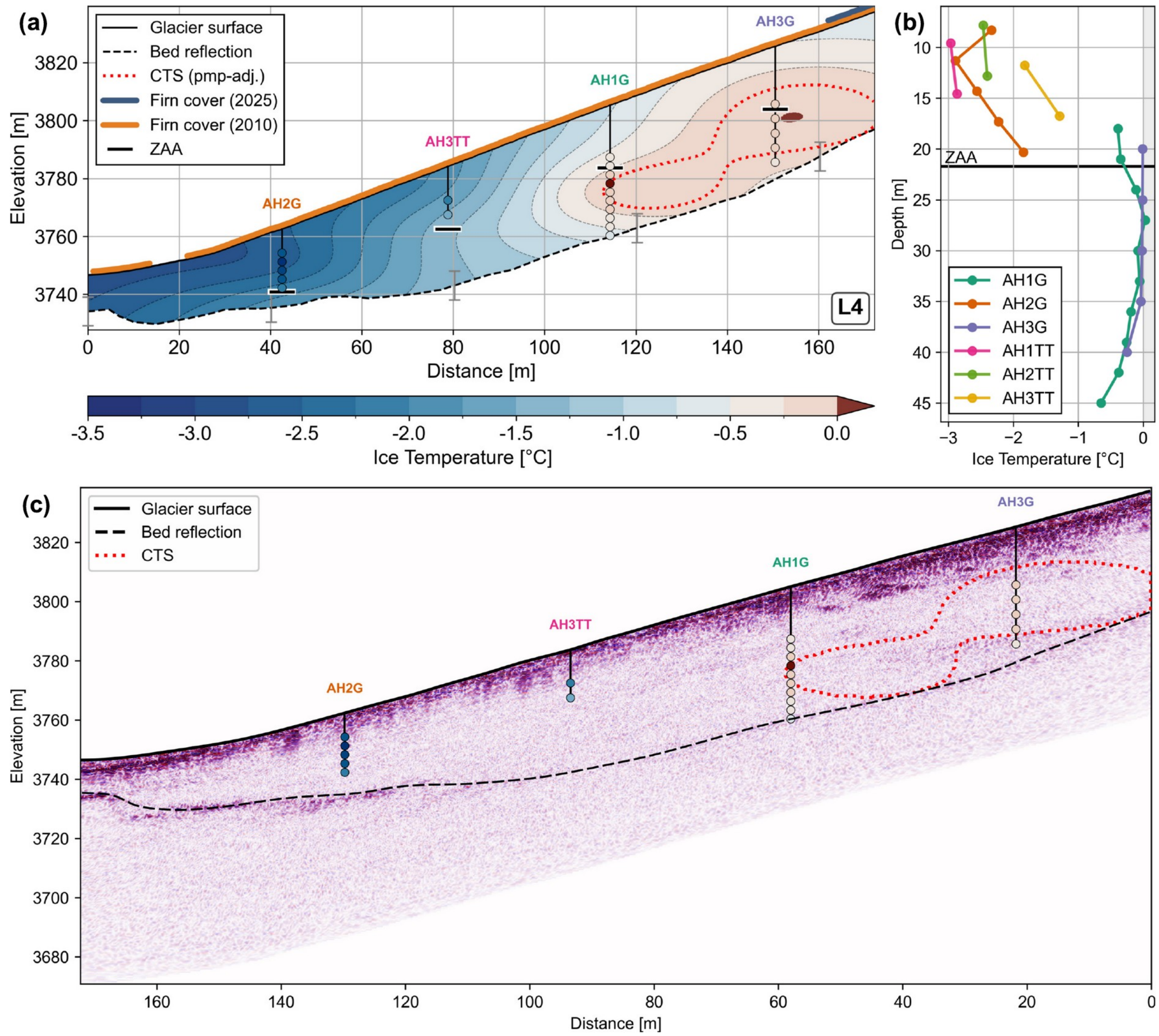


Figure 5, revised (Fig. 6 in the discussion paper). Englacial temperature structure and radar reflectivity along Alphubel profile L4.

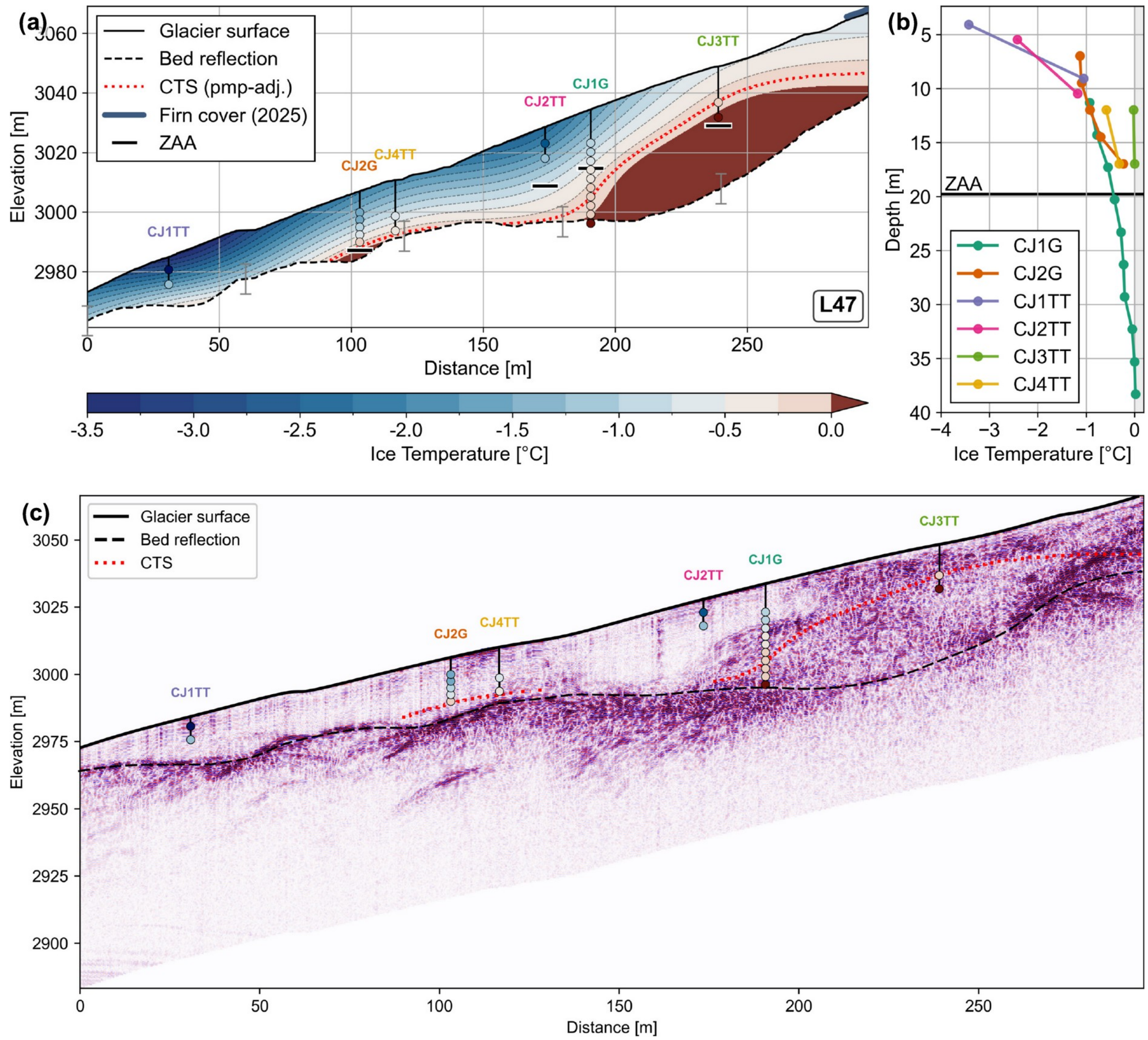


Figure 6, revised (Fig. 7 in the discussion paper). Englacial temperature structure and radar reflectivity along Chessjen profile L47.

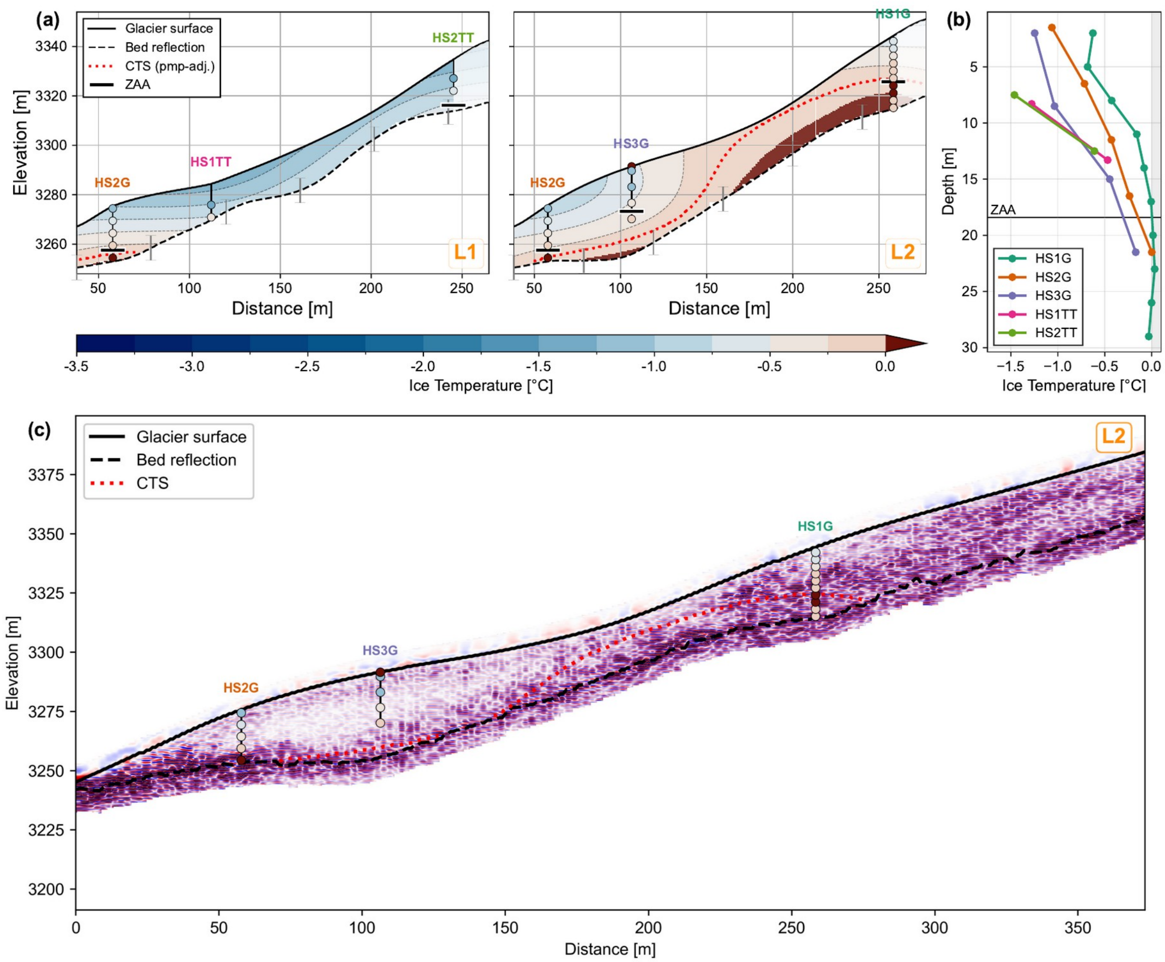


Figure 7, revised (Fig. 8 in the discussion paper). Englacial temperature structure and radar reflectivity at Hohsaas (profiles L1 and L2).