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.

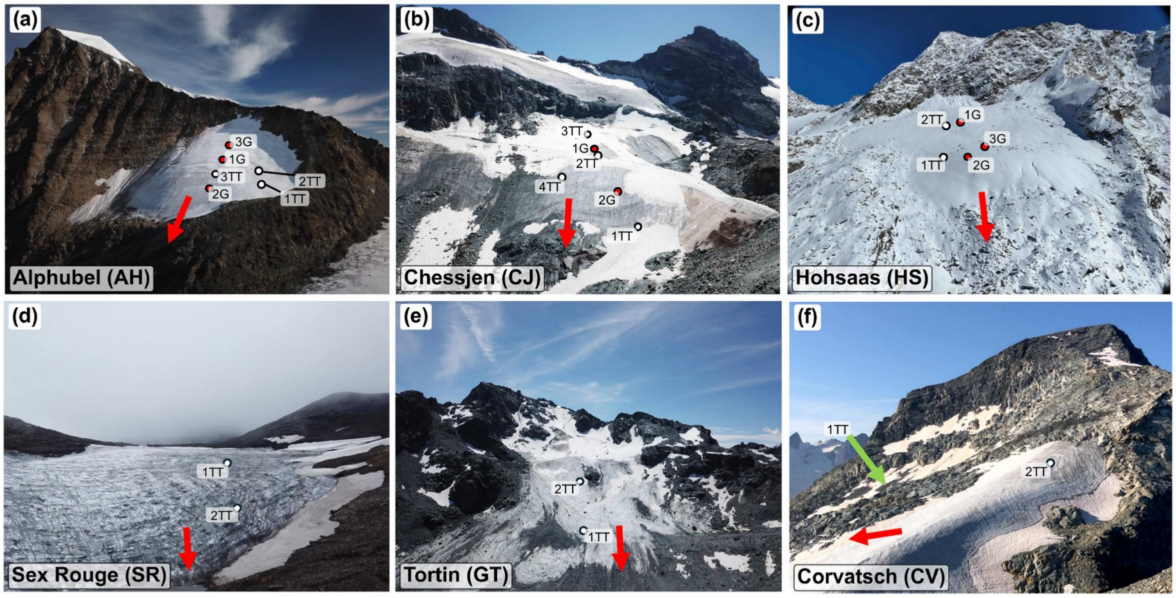


Figure 2 (revised). Overview images of the six study sites; an arrow now indicates the location of borehole CV1TT.

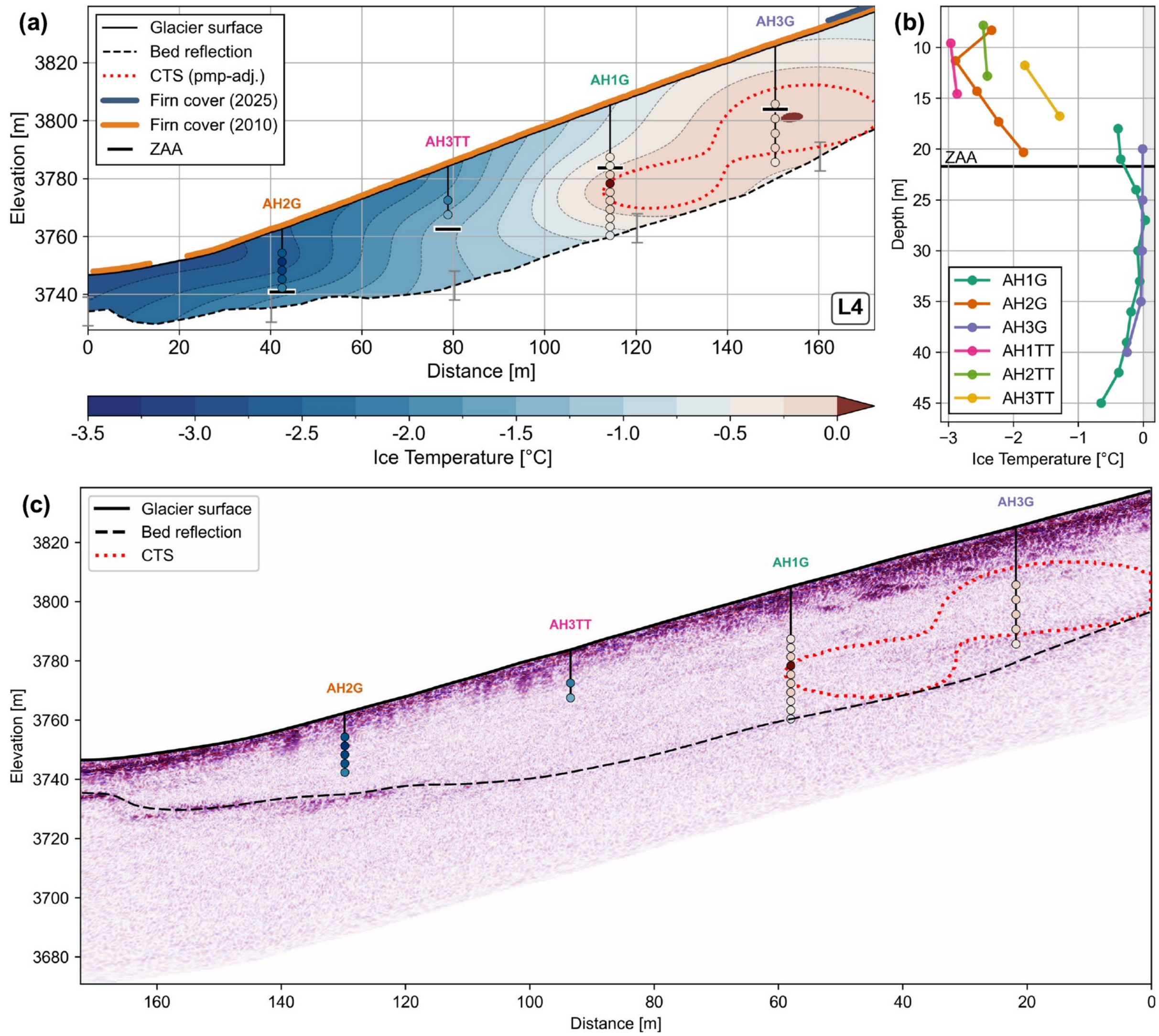


Figure 5, revised (Fig. 6 in the discussion paper). Englacial temperature structure and radar reflectivity along Alphubel profile L4.

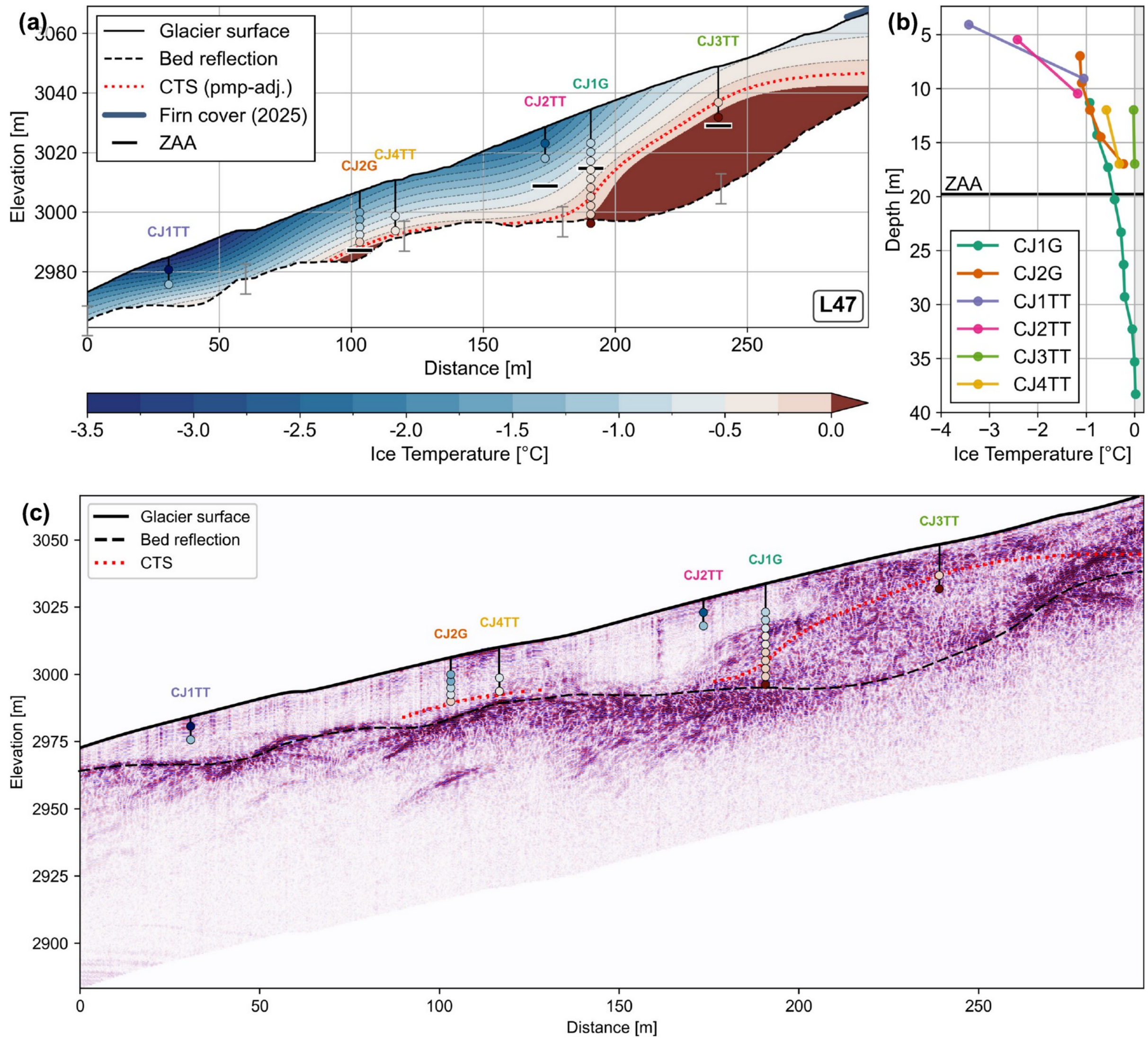


Figure 6, revised (Fig. 7 in the discussion paper). Englacial temperature structure and radar reflectivity along Chessjen profile L47.

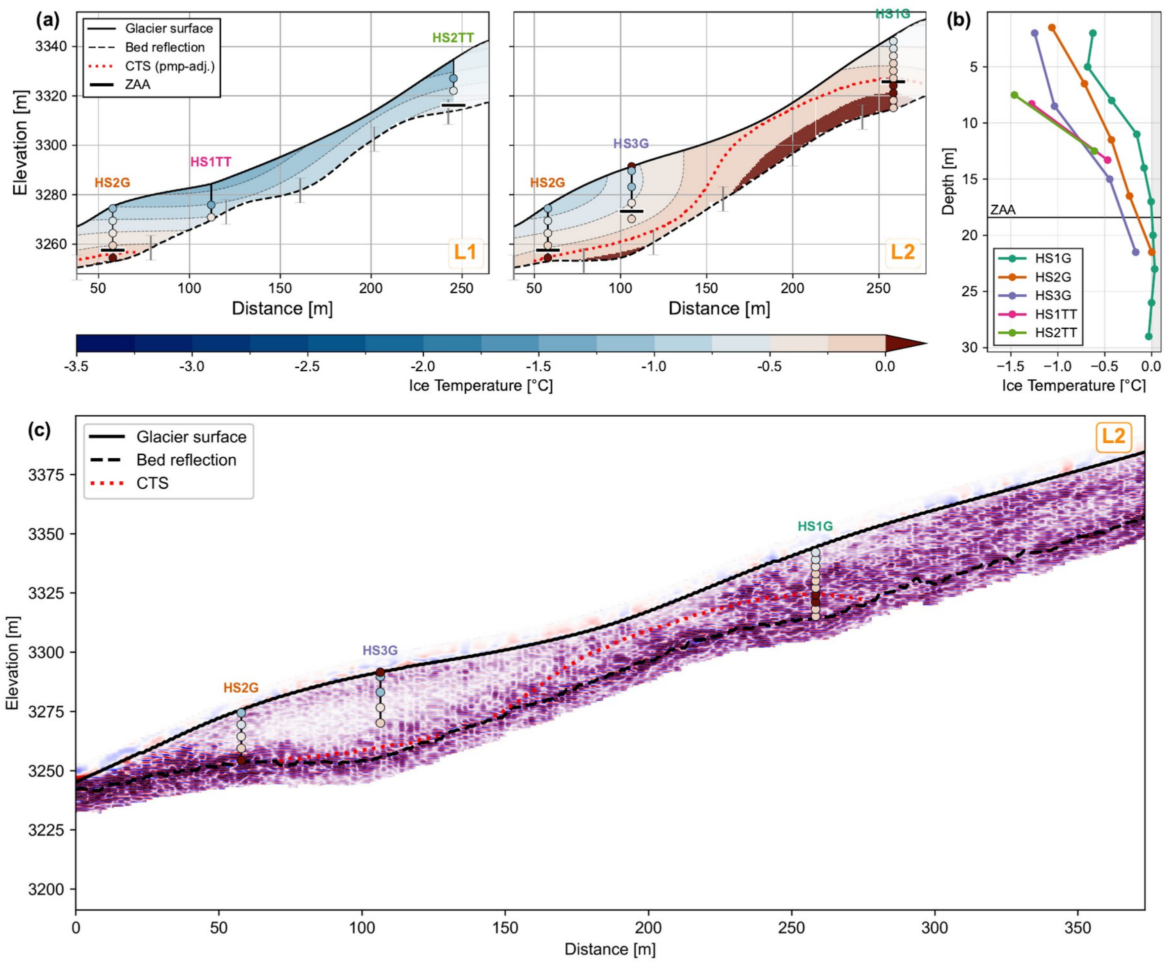


Figure 7, revised (Fig. 8 in the discussion paper). Englacial temperature structure and radar reflectivity at Hohsaas (profiles L1 and L2).

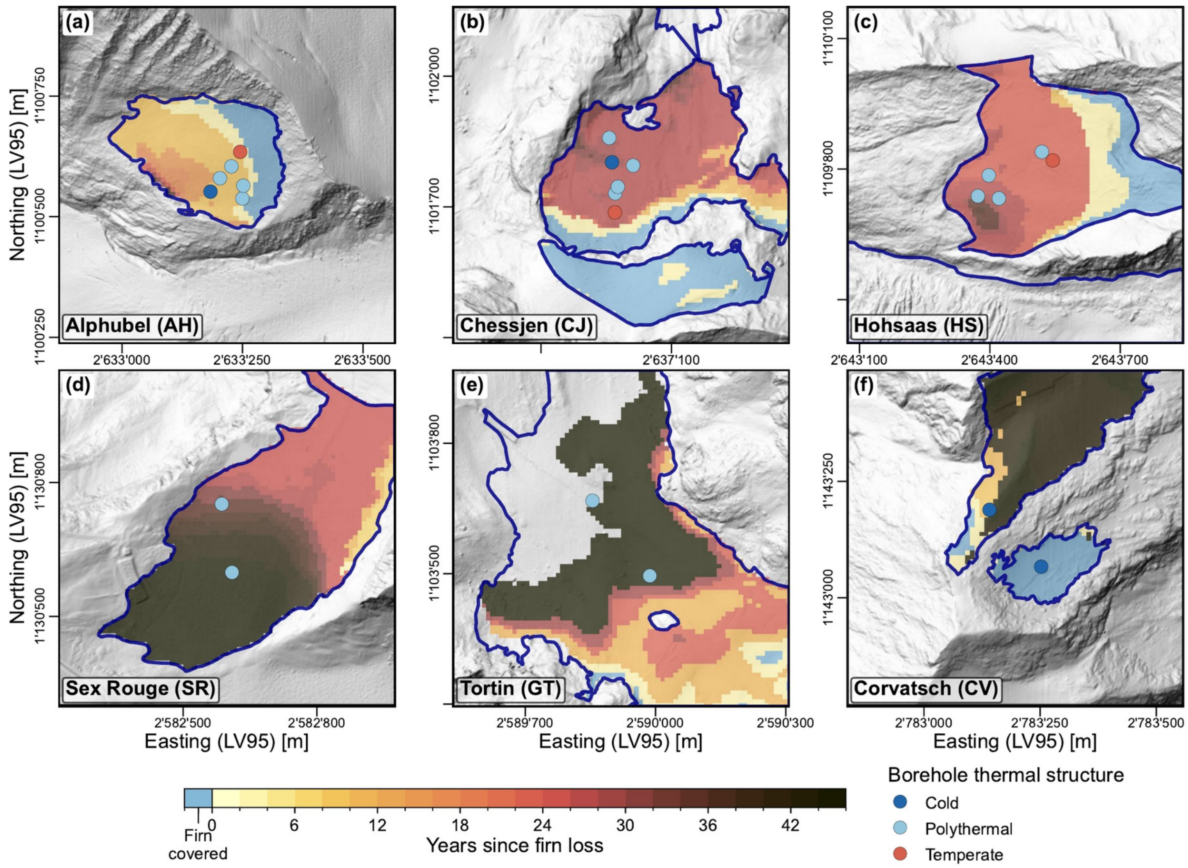


Figure 8, revised (Fig. 9 in the discussion paper). Number of years a given area was firn-free, mapped across all six study glaciers.

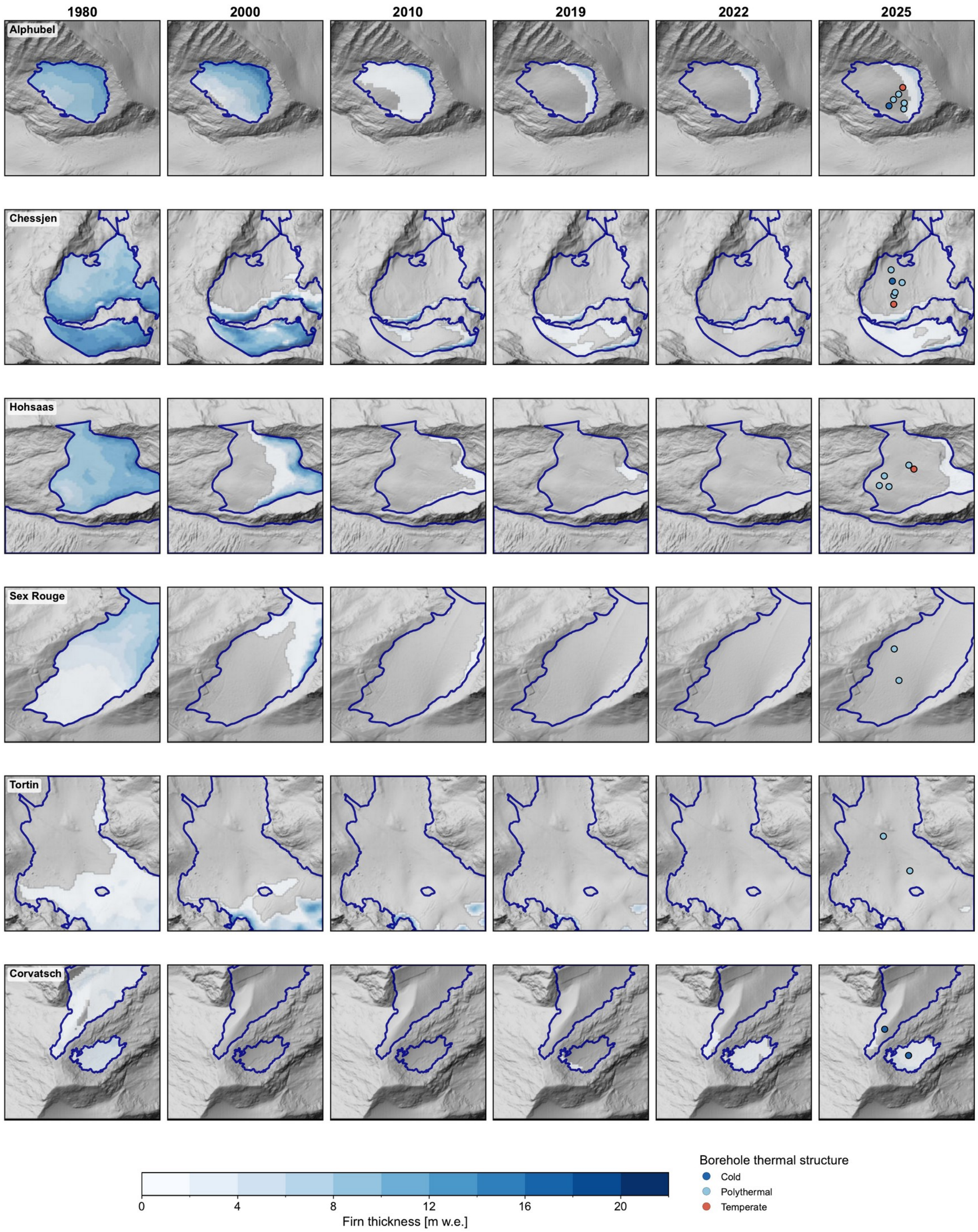
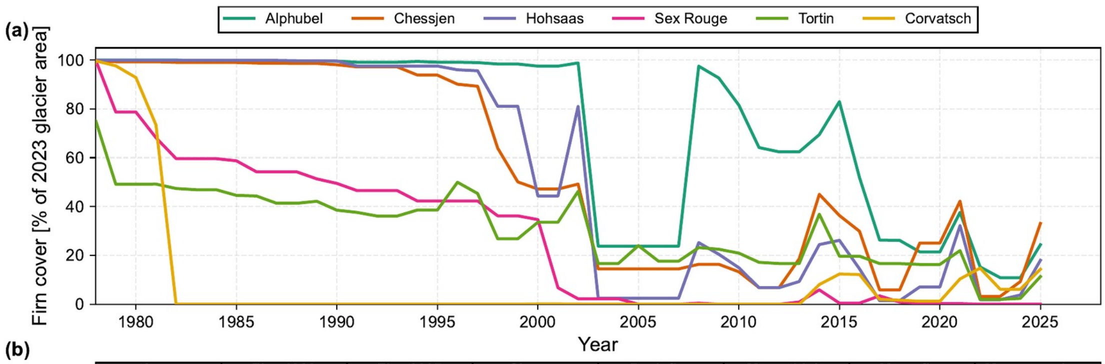


Figure S10, revised (Fig. S9 in the discussion paper). Modelled firn thickness at the six study sites for selected years (1980–2025).



(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.

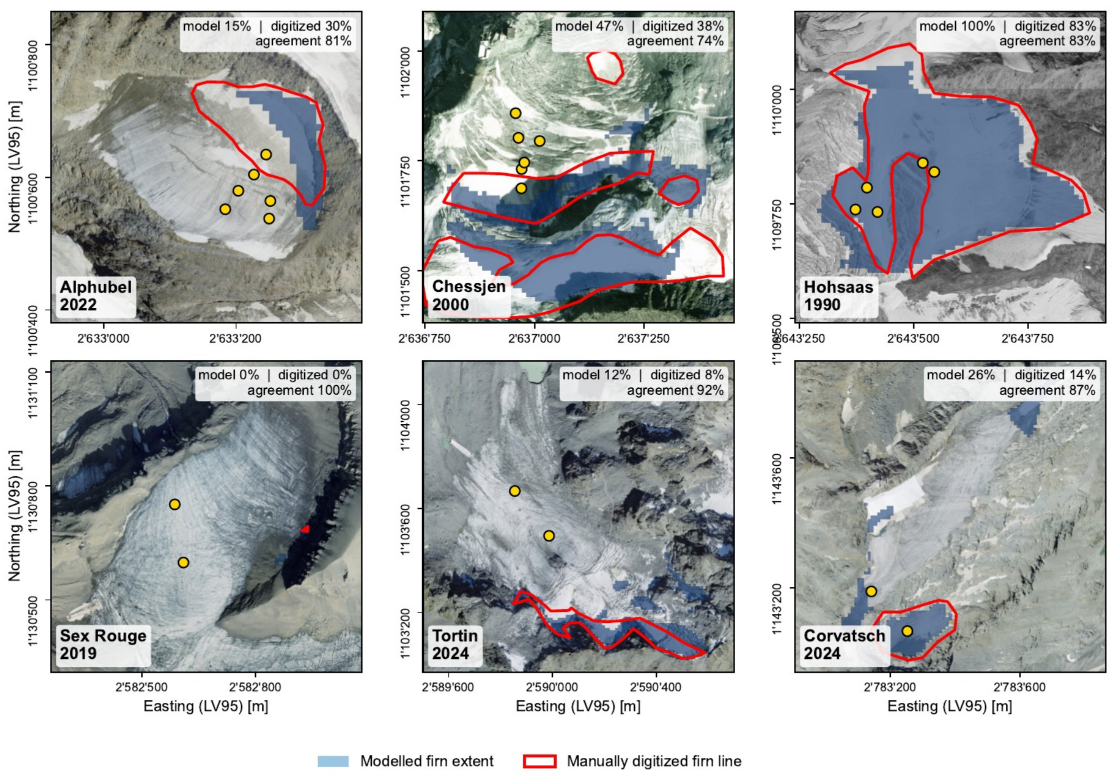


Figure S12 (new figure). Spatial comparison of modelled and manually digitized firn extent.

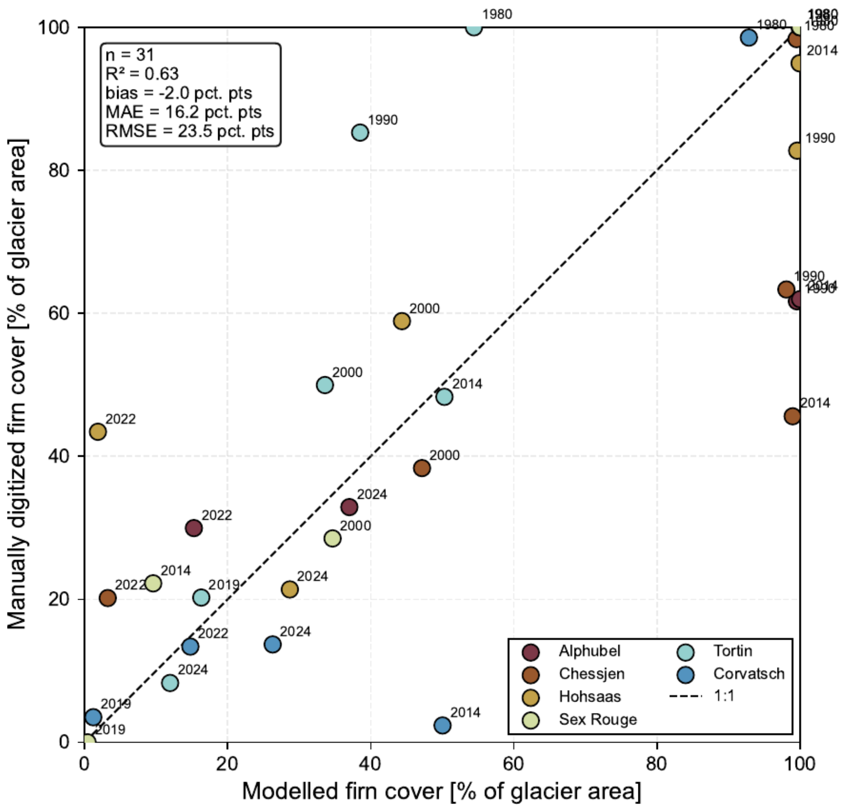


Figure S13 (new figure). Summary comparison of modelled versus manually digitized firn cover fraction.