

Referee Comment - The Cryosphere (EGUsphere)

Manuscript: The thermal regime of the Whymper hanging glacier under climate change (Grandes Jorasses, 4050 m a.s.l., Italy) (egusphere-2026-4058)

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Recommendation: Major revisions (the study is valuable and well suited for TC, but the conclusions on the future basal thermal regime currently rest on two weakly constrained processes that need sensitivity analyses, the observational data need to be documented, and the manuscript needs restructuring).

1. Summary and overall assessment

The manuscript presents rare repeated borehole temperature measurements (1997 and 2020–2022) on the Whymper hanging glacier, a cold, steep and hazard-relevant ice mass above Val Ferret, and interprets them with a 3D thermo-mechanical model (Elmer/Ice) coupled to a 1D sub-grid surface model and a steady-state heat diffusion model of the Grandes Jorasses. The main findings are that near-surface conditions have become temperate since about 2016, that a strong negative heat flux towards the colder north face keeps the base cold, and that the future basal thermal regime will mainly depend on deep water percolation through fractures. Repeated englacial temperature data at such a site are hard to obtain, the modelling framework is state of the art, and the topic (thermal destabilisation of cold hanging glaciers) is highly relevant for hazard management. The study is a welcome contribution to TC.

My main reservations are: (i) the north-face heat flux, which is the key control on the projected basal temperatures, is derived from three ground temperature sensors and kept constant from 1907 to 2050, without a sensitivity range; (ii) the deep water percolation, which the authors themselves identify as the decisive process, is represented by a single model setting that likely underestimates water input, and neither its dependence on the emission scenario nor the evolution beyond 2050 is shown; (iii) the model–data evaluation is qualitative, and the direct 1997–2021 borehole comparisons pair boreholes that are 18–39 m apart; (iv) for most observational datasets it is unclear whether they were collected in this study or taken from previous work, and how the measurements were made; and (v) the structure of the introduction and of the data/methods/results sections could be improved considerably, which would make the strong hazard motivation much clearer.

2. General (major) comments

G1 - North-face heat flux: constraint, temporal evolution and sensitivity

The negative basal heat flux is the single most important factor for the projected basal

temperatures (Fig. 9 vs. Fig. 11), yet it is the least constrained. (a) Eq. 1 is fitted to only three sensors at 55 cm depth (two on the south side, one on the north side) with a fixed lapse rate, so the two remaining coefficients are essentially exactly determined and no uncertainty can be given. Please also clarify the length of the record (“6-years timeseries”, L76, vs. “10 years average”, Fig. 4b caption) and what the reference elevation of 4010 m in Eq. 1 corresponds to. (b) The flux is derived from present-day ground temperatures but applied unchanged from 1907 (a colder climate) to 2050 (a warmer one). (c) The statement that the timescale of temperature change in the rock is >100 years (L183) needs a justification. With the rock properties of Table 3, the thermal diffusivity is $\kappa = k/(\rho c) \approx 1.3 \times 10^{-6} \text{ m}^2 \text{ s}^{-1}$, which gives a diffusive timescale z^2/κ of roughly 20 years at the 30 m depth where the flux is imposed. Timescales of >100 years only apply at depths of about 70 m or more. (d) Fig. 11 only tests the flux switched on or off. I would ask for a sensitivity range (e.g. $\pm 50\%$ of the flux, or a flux derived from 1907–1937 rather than present-day surface conditions), so that readers can judge how robust the “cold base until 2050” result is. Statements such as “its amplitude is also fairly well constrained” (L261–262) should be revised accordingly.

G2 - Deep water percolation, future meltwater supply and simulation horizon

The authors conclude that the future basal temperature “will be determined by how much meltwater reaches the bed each year” (L273–274), but this process is represented by a single setting of the damage-based percolation scheme, which adds about $+1^\circ\text{C}$ at the bed (L243), does not capture the main crevasse crossing the glacier (L249), and likely underestimates water input given the temperate BH1 (L251–253). I suggest (a) an upper-bound experiment, e.g. prescribing water input along the recurrently observed crevasse locations, to show whether basal thawing by 2050 is plausible at all; and (b) showing how surface meltwater supply evolves under the different scenarios. The statement that all RCPs give identical results because the surface is at 0°C (L226–227) is not convincing on its own: the amount of melt, and thus percolation and thinning (about 20 % by 2050, L235–236), should differ between scenarios. Even a simple diagnostic such as positive degree days or modelled melt per RCP would help to frame how the potential for meltwater production evolves. This also concerns the statement that the basal evolution “does not depend on future climate conditions” (L231), which is in tension with the title.

Relatedly, the future simulations end in 2050, which is roughly when the emission scenarios start to diverge. Since the authors state that a “permanent regime” is reached by 2050 (L227), I suggest extending the simulations to 2100 (EURO-CORDEX data are available), at least for RCP2.6 and RCP8.5. This would show whether the quasi-equilibrium holds and whether the scenario-dependent meltwater supply matters for basal thawing on the timescale relevant for hazard management.

G3 - Model–data evaluation and the 1997–2021 comparison

(a) The “same site” pairs compared in Fig. 1e–h are not co-located. Based on Table 1, the 1997 and 2020–2022 boreholes with the same number are about 18 m (site 3), 29 m (site 4), 35 m (site 6) and 39 m (site 1) apart, and the 1997 coordinates are only estimates. The data themselves show that temperature differences over such distances can be large (e.g. BH1 is temperate in 2021 while BH4 and BH6, about 23–24 m away, are $1\text{--}2^\circ\text{C}$ colder at depth). The

direct comparisons therefore overstate how robustly the warming signal is observed. Please state the separation distances and rely more on the model-based comparison. (b) The agreement between model and observations is described only qualitatively (“overall good agreement”, L211). Please provide quantitative misfit statistics (e.g. mean bias and RMSE per campaign and per borehole). (c) The statement that 1997 temperatures “were still close to steady condition” (L195–196) is not supported by Fig. 6b: BH1 is about 3 °C and BH3 and BH6 about 2 °C warmer than the steady state at depth, while BH7 is colder. (d) The grid search for accumulation and E (L200–204) is reported as yielding a “unique solution”; please show the misfit surface (e.g. in the supplement).

G4 - Observational data: provenance and measurement description

The manuscript draws on a range of observational datasets, but for most of them it is unclear (i) whether they were collected as part of this study or taken from previous work, and (ii) how the measurements were made. Only the 1997 profiles (Pralong and Funk, 2006) and the surface velocities (Dematteis et al., 2021; Faillettaz et al., 2016) are referenced. The 2020–2022 boreholes appear to be new (L59–60, author contributions, Zenodo data set), but the drilling dates in Table 1 span November 2020 to January 2022 while the text refers to a 2021 campaign. For the density profile (August 2022, Fig. 7c), the 2010 GPR survey, the Pléiades and drone DEMs, and the rock temperature sensors installed in 2009 (only credited in the acknowledgements), neither the source nor the method is given. Please state for each dataset whether it is new or previously published (with reference), and describe the methods for the new data.

For the borehole temperatures, which are the main observational contribution of the study, this should include the drilling method, the sensor type, accuracy and calibration, the sensor spacing, the time elapsed between drilling and measurement, and whether the profiles are single readings or averages over a period. This is particularly relevant here: if the boreholes were drilled with hot water or steam and read before the drilling heat had dissipated, the profiles could be biased warm, which directly affects the interpretation of the temperature BH1. Similarly, the profiles measured in January 2022 carry a seasonal winter signal in their upper part. For the density profile, the GPR survey and the DEMs, please give the method, resolution and uncertainty. A table summarising all datasets (type, date, source or reference, new or published, accuracy) would be very helpful.

G5 - Structure of the introduction and of the data, methods and results sections

(a) Introduction: the current storyline starts with the hazard of hanging glaciers, moves to an overview of thermal modelling, and ends with “each glacier requires a dedicated analysis” (L46–47), which is a rather weak motivation directly before the study aims. Since the primary motivation is the future stability of the Whympfer serac (L268), I suggest starting with the research overview and ending with the specific hazard situation of the Whympfer serac (L50–59), followed by “The present study quantifies...”. The strongest argument should directly precede the description of what was done. (b) Study site and data: the description of the study site is currently embedded in the introduction, and the observational data are mixed with the model description. I suggest a dedicated “Study site” section with a short site description and an overview of the observational record (see G4), followed by “Data and methods” (geometry and

velocity data, meteorological data, thermo-mechanical model, heat flux model). (c) Results: Sect. 3.2 and the first part of Sect. 3.3 mix the model spin-up and calibration (thickness, velocity, density as calibration targets) with the results of the transient thermal simulations. I suggest a “Spin-up and calibration” section, followed by the thermal results for the past century (Fig. 8) and for the future (Fig. 9). This would also make clear that the future runs continue from the same calibrated historical run.

3. Specific (minor) comments

General

- **Terminology:** Terms such as “internal temperature” (L19) and “glacier temperature field” (L26) are ambiguous, as they could also refer to cavities, water pockets or englacial channels. I recommend consistently using “ice temperature” or “englacial temperature”, or defining the terms at first use. Similarly, it is often not immediately clear whether ice, rock, air or near-surface temperatures are meant (e.g. L191), and the terms cold, temperate and close to the melting point should be used consistently (e.g. L218–220).
- **Consistency of the conclusions:** The future runs end in 2050, but the abstract states that the cold base “will persist for several years” (L9–10), the results speak of “coming decades” (L220–224), and a “permanent regime” is said to be reached by 2050 (L227). Please harmonise the wording across abstract, results, discussion and conclusions (see also G2), and check that the title (“under climate change”) is consistent with the statement that the future basal evolution is insensitive to the climate scenario (L231).

Introduction

- L17: The sentence is correct but rather abstract for readers less familiar with glacier thermal structure. Consider phrasing it more directly (e.g. naming the transition from a frozen to a thawed bed explicitly).
- L36–42: For completeness, consider also mentioning distributed surface energy balance and firn models that resolve near-surface and firn temperatures (e.g. Gastaldello et al., 2025, *The Cryosphere*, 19, 2983, <https://doi.org/10.5194/tc-19-2983-2025>), possibly where the surface boundary condition is introduced.
- L55–59: This would fit better in the hazard motivation at the end of the introduction (see G5).

Data and methods

- L71–73: Please explain why the temperate zone is expected to extend towards the western side, or cross-reference Sect. 3.2/3.3, where the model provides the explanation.
- L76–78: Please clarify the record length (6 vs. 10 years, see G1) and the period covered.
- L83–84: The GPR data are only described as being used for the bedrock topography, but later serve as a calibration target for ice thickness (L201, Fig. 7b). Please state this here (see also G4).
- L86–87 and L93–95: Please give the spatial resolution of SAFRAN and of the EURO-CORDEX simulations, and list the 14 GCM/RCM combinations (e.g. in the supplement).
- L140–141: What is the initial glacier geometry for the steady-state run? Is the

- present-day geometry used, or is the glacier grown from zero ice thickness?
- L155–156: Please clarify that the future runs continue the calibrated historical run, and whether the switch from SAFRAN to bias-corrected EURO-CORDEX forcing in 2023 causes a discontinuity.
- L156–158: Please state here which parameters are calibrated and how (the grid search on accumulation and E is only described in Sect. 3.3), and how the initial guess was derived.
- L168: What does the reference elevation of 4010 m correspond to?
- L183: Please justify the >100-year timescale (see G1).
- L185–186: Please clarify how the steady state is forced: are the 1907–1937 mean surface conditions (temperature, melt, potential radiation) applied as constants, or is the 30-year daily series cycled?
- Table 1: The table is important for readers who want the exact borehole locations, but could be moved to the appendix. Please also clarify the drilling and measurement dates (see G4).
- Table 3: $P_r = 1.5 \text{ m w.eq. yr}^{-1}$ is cited as Gilbert et al. (2023), but it was calibrated in this study (L203–204). Please also give the source of the enhancement factor $E = 1.3$.

Results and discussion

- L191: Please specify that this refers to ice temperatures (see terminology comment above).
- L192–193: The influence of colder ice advected from the summit area is an interesting model result. Please support it, e.g. with velocity vectors in Fig. 8b/c or a short sensitivity test.
- L214–216 and L219: The text refers to 2022, while Fig. 8 and its caption refer to 2021. Please make these consistent.
- L216: The statement that “the western side of the glacier may already be entirely temperate” is speculative. BH4 and BH6, about 23–24 m from BH1, are 1–2 °C colder at depth and follow a similar profile, which points rather to a local feature (e.g. water input along a crevasse, trapped water or a cavity, or a drilling-related bias, see G4), and the model itself does not show a temperate western side.
- L218–220: Fig. 1d and Fig. 8f show about -0.5 to -1.5 °C at 20 m depth in most boreholes and about -4 °C in BH2. “Close to the melting point” would be more accurate than “temperate”.
- L227: “permanent regime”: consider “quasi-equilibrium”, and only if supported by longer simulations (see G2).
- L240: Please refer to the experiment in Fig. 11 here, which supports this statement.
- L263–264: The argument that the transient change of the flux is negligible because warming mainly occurred since the 1980s should be quantified (see G1).
- L278: Please explain how the potential collapse volume of about 500 000 m³ was derived.

Figures

- Fig. 1: A nice overview of the measurements. Since the study focuses on a single site and the topographic setting is central to the model (heat flux towards the north face), please add a regional overview map (Grandes Jorasses range and location in the Alps) and a north arrow.

- Fig. 1e–h: Please indicate the horizontal distance between the compared 1997 and 2021 boreholes (see G3).
- Fig. 4b: Please state the period over which the observed ground temperatures were averaged (see G1).
- Fig. 7: The sentence “The inset shows comparison between modeled thickness and observed thickness” appears in both (a) and (b); it seems to belong to (b) only.

Technical corrections

- L186: “meterological” should be “meteorological”.
- L194: “cool down basal temperature of up to 2 °C” should be “cools the basal temperature by up to 2 °C”.
- L235: “which leads in a decrease” should be “which leads to a decrease”.
- References: the author list of Perret et al. (2021) is duplicated; several DOIs read “https://doi.org/https://”; Vernay et al. (2021) is cited as ESSD Discussions, please cite the final published version.