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**Public commentary on Svinka et al.**

*"GPR-derived ice thickness of the temperate Hintereisferner glacier (Austrian Alps): evaluation of thickness models"*

**by the Ice and Climate Unit (ACINN), University of Innsbruck**

My name is Christian Wild and I am a geophysicist at the University of Innsbruck, with experience in radio-glaciology and numerical ice-flow modelling in both alpine and polar environments. I write on behalf of the Ice and Climate Unit (ACINN), University of Innsbruck. We discussed the manuscript by Svinka et al. in detail during one of our group meetings, involving participants ranging from MSc students to PhD students, postdoctoral researchers, senior scientists, and emeritus professors. Our feedback therefore reflects a broad cross-section of an academic audience, combining expertise in field-based geophysical measurements and glacier modelling, as well as local knowledge of Hintereisferner and its long-standing observational record.

Overall, we consider the study timely and potentially valuable, and we appreciate the effort involved in collecting and processing GPR data on a temperate alpine glacier. In particular, the manuscript addresses an important topic, as reliable ice thickness information remains a key limitation for many modelling applications and for the evaluation of commonly used thickness estimation approaches. At the same time, we identified several substantial issues regarding the presentation, interpretation, and evaluation of the results, as well as aspects of the methodology and discussion that require clarification or further justification.

Balancing the study's strengths and weaknesses, we recommend **major revisions** before the manuscript can be considered for publication in *The Cryosphere*. We believe that, once revised, the paper has the potential to make a meaningful contribution to the existing body of literature. In the following, we provide detailed comments and concrete suggestions intended to help strengthen the manuscript and improve its clarity, scientific robustness, and relevance to the broader glaciological community. If questions arise, we invite the authors to reach out via the Discussion portal for further clarification, and hope that our feedback is useful.

**1) Summary:**

Svinka et al. present new ground-penetrating radar (GPR) measurements of ice thickness at Hintereisferner (HEF), acquired in August 2023. They compare the derived thickness distribution to results from three ice-flow modelling approaches and report that all models systematically overestimate measured ice thickness by approximately 40-60 m. The authors discuss possible reasons for this mismatch, suggesting that adjustments to basal sliding parameterisations and ice rheology could improve agreement between models and observations. They also reflect on uncertainties associated with their choice of radar wave propagation speed, adopting a value commonly used for cold ice despite the temperate nature of the glacier. The paper concludes that (a) continued field-based ice thickness measurements remain essential for calibrating and evaluating ice thickness models, and (b) reducing observational uncertainty is important for improving projections of future change in temperate alpine glaciers.

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**2) Strengths:**

2.1) GPR data acquisition on temperate ice:
Measuring ice thickness on temperate glaciers is considerably more challenging than on cold glaciers, as the presence of liquid water strongly attenuates radar signals and can obscure bed reflections. The authors effectively address this limitation by using a low-frequency system (38 MHz), which improves bed detectability at the expense of resolving internal stratigraphy. Since the primary objective of the study is ice thickness rather than internal layering, this trade-off is appropriate and well justified.

2.2) Identification of an ice rise:
The GPR dataset reveals an ice rise that appears to mark a transition between areas with clear and poor bed reflections. This feature is potentially important for interpreting the radar data and understanding local glacier dynamics, overdeepening and basal conditions, yet it is not further investigated in the manuscript. Notably, a similar ice rise has been suggested in an earlier GPR survey (1997, see also Span et al., 2005) and an overdeepening was reported earlier by Förtsch and Vidal, 1956, which provides an opportunity for direct comparison and strengthens the discussion.

2.3) Model comparison and contextualisation:
The comparison with three widely used ice thickness modelling approaches (GlabTop2, OGGM, and Millan et al., 2022) is a valuable component of the manuscript. The authors describe the underlying assumptions and methodological differences between these models clearly, and they discuss the model-data mismatch in a structured way that is largely consistent with the presented results.

**3) Weaknesses:**

3.1) Conclusions are not supported by the presented analysis

The two main conclusions of the manuscript (a) that the new ice thickness data allow improved model calibration, and (b) that this reduces uncertainty in future projections of temperate glacier change, are currently not supported by the presented results. While the authors identify a systematic model overestimation of ice thickness, they do not demonstrate that recalibration actually improves the model fit, nor do they quantify any effect on projected glacier evolution.

In particular, the discussion on adjusting basal sliding and ice rheology (e.g. around L240) remains speculative. For a journal such as *The Cryosphere*, these claims should be supported by running sensitivity tests or inversion experiments (at least for OGGM), demonstrating improved agreement with the GPR-derived thickness, and assessing the impact on forward projections. If needed, technical support for implementing such calibration workflows can be obtained via the OGGM community (e.g. the OGGM Slack). Otherwise, we recommend that the conclusions should be toned down accordingly.

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3.2) Model set-up and timing are unclear and may be inconsistent with the 2023 GPR survey

A major concern is that the timestamps of the GPR survey (August 2023) and the model initialisations do not clearly align. While the scripts are available on Zenodo, the associated DEM and outline used are not included, and therefore model results can not be reproduced. Overall, more information is needed to ensure reproducibility and to interpret the reported model-data bias with the appropriate timestamps.

For **GlabTop2**, key parameter choices should be reported, if they differ from the default setup.

For **Millan et al. 2022**, the surface adjustment appears to use a glacier-wide mean thinning rate rather than spatially variable thinning. Given the strong elevation dependence of surface lowering at HEF, this can introduce several metres of surface elevation error and thus tens of metres of thickness bias over multiple years. We recommend using the cumulative mass balance of the respective elevation zones for correction, available via the World Glacier Monitoring Service, 2026. As suggested in the manuscript, surface elevation changes from mass balances can be derived by using the density of ice (900 kg/m³). As HEF is moving slowly, surface elevation changes by ice flux divergence can be neglected.

For **OGGM**, the provided script does not work out of the box: the custom file “params.cfg” should be implemented in the python script via “cfg.initialize(‘params.cfg’)”, or you should explain how to use the “params.cfg” file in “readme-oggm.txt”.

Further, in the manuscript you do not mention how you derive the apparent mass balance, which is central to the ice thickness inversion of OGGM. From the scripts we can see that you use climate information from W5E5 for the calibration of the mass balance model, and that you calibrate towards Hugonnet et al., 2021 (tasks.mb_calibration_from_geodetic_mb). However, Hugonnet et al. (2021) is only valid for the RGI outline and associated hypsometry (the outline year of RGI, for HEF it is 2003). Instead of using Hugonnet et al. (2021) you could also rely on WGMS observations, which are available for Hintereisferner, or compare the OGGM output when using these different sources for mb calibration (and document this in the manuscript). Further, in the default OGGM configuration we use a total volume estimate from Farinotti et al., 2019 (also valid for the RGI outline) to tune the associated dynamic parameters (glen_a and fs) during inversion. However, in your approach you are just using the default values, defined in params.cfg (which should be seen as first guess values). You could adapt your approach by using the actual calibrated values of OGGM v1.6.2 instead of relying on the default values from params.cfg (those values can be extracted from the preprocessed directories: https://docs.oggm.org/en/v1.6.2/shop.html#pre-processed-directories). Either way you should document in the manuscript, which values you have used.

3.3) Radar interpretation and elevation correction require clarification and improved consistency

We note a potential inconsistency between the interpreted bed picks and the presented radargrams. In particular, bed reflections appear to be assigned in sections where no clear bed signal is visible (compare Figs. 3b and 4b), which requires further justification or clarification of the picking strategy. We suggest plotting the bed picks on each radargram. Only two radargrams are shown in the main text (profiles 6 and 13); we suggest adding the along-flow profile to the main manuscript, as it is essential for evaluating data quality (transition from obscure to well-defined bed) and interpretation (ice-rise separating subglacial basins, which could be investigated by calculating a map showing hydrologic potential). We also suggest including figures of all remaining radargrams and corresponding bed picks in the Appendix or SI.

Regarding the elevation correction, the study uses a DEM from September 2022, although precise GPS measurements from the August 2023 GPR survey are available. Given the strong surface lowering in the lower glacier (~5.0 m $a^{-1}$ below 2800 m since 2018), relying on the older DEM likely introduces a systematic bias. We therefore recommend using the GPS-derived elevations for the correction. In addition, the elevation differences between DEM and GPS should be explicitly shown (e.g. as a Figure in the SI) and discussed in the main text, as they provide valuable information on spatially variable ablation that is currently not represented in the analysis, which likely also affects the discussion of positive ice thickness bias along the sides of HEF's tongue.

<u>3.4) Radar wave velocity assumption and uncertainty analysis</u>

The assumption of a cold-ice radar wave velocity (L142) is not justified for a temperate glacier setting, as is well framed in the introduction (L71-74). Rather than relying on values from other regions (Iceland, L144), the authors should directly assess the sensitivity of derived ice thickness to plausible ranges in radar propagation speed and compare the resulting differences.

In addition, the manuscript does not adequately account for picking uncertainty. If multiple operators interpreted the bed reflection, the resulting variability should be quantified explicitly. From Fig. 3a, the picking uncertainty alone could easily be on the order of 10 m, which adds uncertainty on top of the cold-ice assumption.

This uncertainty should be combined with the elevation correction error introduced by using the 2022 DEM (approximately ~5.0 m $a^{-1}$ surface lowering), leading to a cumulative uncertainty that is already a substantial fraction of the reported model-data mismatch (40-60 m). This does not yet include additional systematic uncertainties discussed in earlier comments (e.g. timing inconsistencies, Major comment 3.2).

A rigorous propagation of all relevant error sources is therefore essential to assess the robustness of the reported bias and the strength of the conclusions through a more thorough discussion of the potential error sources.

Once the scale of all the possible sources of uncertainty has been established, it should be compared to the scale of the biases shown in Table 1 (e.g., 48% for OGGM).

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**Figures:**

Fig.1:
While we appreciate the inclusion of field-site imagery and note that panel (b) was useful for estimating antenna spacing (Methods), we do not see a clear scientific necessity for this figure in the main text, particularly as Figure 1. We therefore suggest moving Figure 1 to the SI and choose Figure 2 as revised Figure 1.

Fig.2 (new Figure 1 in revised manuscript):
We also suggest adding a small overview inset in the top left corner showing the location of Hintereisferner within the European Alps (or within Austria) to improve geographical context for non-local readers. To improve interpretability of the radar profile layout, it would be helpful to visually group and label profiles 1–8 as “well-defined basal reflections” and profiles 9–15 as “obscure reflections” (e.g. using a bracket or brace). In addition, profiles 6 and 13 should be highlighted in a distinct colour to link them more clearly to the radargrams shown in subsequent figures. An arrow indicating the location of the identified ice rise would further improve clarity.

Finally, we suggest adding the along-flow radargram (profile 15) as an additional panel beneath the map. This would clearly illustrate the transition from more ambiguous to well-defined basal reflections, include the ice rise location, and allow direct visual connection to the interpreted bed picks.

Fig.3 (new Figure 2 in revised manuscript):
We suggest adding the interpreted bed picks as a dashed black line to all radargrams, both in the main text and in the SI, to improve transparency and reproducibility of the interpretation. At present, the bed reflection in panel (a) is only inferable with difficulty, and is not clearly visible in panel (b). However, comparison with Fig. 4b suggests that a bed pick was nevertheless made from this radargram, which requires clearer visual justification.

In panel (a), the reflections labelled “2” appear to indicate coherent features that may be of interest. It would be helpful to clarify whether these features are consistently visible across all radargrams, i.e. whether they represent along-flow persistent structures. If so, their glaciological interpretation should be discussed more explicitly. If not, clarification is needed on their origin and significance. Are these potentially associated with surface features such as moraines or debris-covered ice?

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<u>Fig.4 (new Figure 3 in revised manuscript):</u>
(a) We suggest cropping this panel to the same spatial extent as panels (b)–(d) for consistency.

(b) The locations of all GPR profiles (red lines in Fig. 2) should be overlaid on the ice thickness map. In particular, profiles 6, 13, and 15 should be clearly highlighted to ensure consistency with the radargram figures.

(c) The colorbar limits (0–162 m) should be consistently applied across all ice thickness maps shown in Fig. 5 (a–c) to ensure comparability.

(d) We recommend keeping the scale bar only in panel (d), and removing it from panels (a)–(c) to reduce visual clutter.

<u>Fig.5 (new Figure 4 in revised manuscript):</u>
We suggest improving consistency and clarity by harmonising the colour scales and reducing visual clutter across all panels. For panels (a), (c), and (e), the colourbar limits should be unified to 0–162 m, corresponding to the range observed in the GPR-derived ice thickness. We recommend retaining the contour lines, as they help visualise important spatial structures such as the ice rise. For panels (b), (d), and (f), the current asymmetric colour scale may be misleading, as it can visually exaggerate gradients not supported by the data. We therefore suggest using a centred diverging colormap (e.g. “seismic”), with limits set symmetrically to -216 m to +216 m to better represent differences. Finally, to reduce clutter, scale bars should be removed from all panels except panel (f).

<u>Fig.6 (new Figure 5 in revised manuscript):</u>
We have concerns regarding the statistical robustness and interpretability of this scatterplot, which is currently not assessed or discussed. In addition, the origin of the apparent horizontal clustering warrants clarification, as it may reflect methodological artefacts rather than physical relationships. We suggest restructuring this figure into two rows of three panels. The top row should show one panel per model, allowing clearer comparison between approaches. In this row, the scatter points could be coloured by elevation to better illustrate any systematic elevation dependence. The second row should also consist of three panels, but with points coloured by GPR profile number (1-15). This would help identify whether the observed clustering or horizontal patterns are linked to specific transects. In its current form, the apparent horizontal bands suggest that modelled ice thickness may be constant within certain elevation bands, which should be explicitly investigated and discussed. If both rows provide more or less the same information, we encourage the authors to pick either color-coding by elevation or profile number for a revised Figure.

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<u>Table 1:</u>
We recommend adding standard deviations (±1σ) to all reported mean values to better reflect the variability in the dataset. In addition, these standard deviations should also be consistently included when reporting mean differences in the abstract and main text, to provide a more complete and transparent representation of uncertainty.

**Minor comments:**

L21-23: Remove this sentence as public data access is the modern standard

L25: Replace “for the last few decades” with “ since YYYY”

L26: Replace the reference to highlight the efforts of the glacier monitoring community providing robust data on glacier changes from in-situ observations and remote sensing (Zemp et al., 2019; Hugonnet et al., 2021; The GlaMBIE Team et al., 2025).

L28: This sentence needs to be rephrased. Fischer and Kuhn, 2013 actually represent a number of ice thickness measurements, which are considered to be sparse in the manuscript, but actually are plenty for the Austrian Alps at least around the year 2000. For Austrian glacier inventory please refer to Hartl et al., 2026, in review or to Fischer et al., 2015.

L29: Rephrase to “reliable ice thickness maps are essential for calibrating ice-flow models, which in turn are used to predict glacier evolution under different emission scenarios.”

L33: Add citations of the models discussed in the present study, which were also used for global glacier evolution modelling (OGGM citation, maybe also Rasper et al. 2000)

L39: Reword “validate modelling results” to “calibrate modelling efforts”.

L44-47: Why not using geographically closer inventories than the Swiss ones ? There are recent inventories of glaciers in South Tyrol (Galos et al., 2025), which experience a more similar atmospheric regime than the glaciers in the Swiss Alps.

L49: Add Van Tricht et al., 2026 as most recent reference.

L50: Reword “one of the world reference glaciers of the WGMS” to “one of 60 reference glaciers, along ChooseYourFavoriteReferenceGlaciers of the WGMS”, [https://wgms.ch/products_ref_glaciers/](https://wgms.ch/products_ref_glaciers/)

L51: Use the correct glacier area of HEF (5.17 km² in 2022). You might have derived a misleading area from the DEM, which might not cover some tributary glaciers. (Note: For the mass balance calculations for the year 2022/23 the glacier area of the previous survey (6.14 km², 2019) was still used and reported to the WGMS).

L50-66: Move these two paragraphs to a dedicated “Study area: Hintereisferner, Austrian Alps” section.

L54: Add more recent citations of HEF’s evolution since the early 2000s e.g., Klug et al., 2018.

L57: Add Nicholson et al., 2025 as the most recent reference for this field.

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L67-74: Move this paragraph to the “Data and Methods” section.

L75-78: Great guidance paragraph. Reword “hand-held” with “terrestrial”

L79: Reword “Methods” to “Data and Methods”

L90-156: Please use active language for everything that was done by the authors (We conducted a GPR survey, we oriented GPR profiles transverse to flow direction, we processed the GPR data in Prism, we applied elevation-correction, etc) to better distinguish the author’s contribution to the work done by others. In its current form it is sometimes hard to differentiate the novelty, especially in the call-outs to Murray et al., 2000 in Section 2.3.

L138: Cold-ice velocity value ? Compare this choice to the statements in L71 and L167 about the importance of liquid water on the measured ice thicknesses. This choice requires justification, and direct quantification on the derived results (not a call-out to Falljökull, Iceland).

L158: What is a “B-scan” ? Consider rephrasing to “GPR profiles” for simplicity, here and throughout (f.e., L161,163)

L158: Add “covering about 1.3 $km^2$ of the lower tongue of HEF (revised Fig. 1)”

L168: What is a “piezometric surface”, please add a definition. The reader shouldn’t have to go to Murray et al., 2000 or Lamsters et al., 2020b to learn about this term.

L171: typo in “Fig.1”, which should be “Fig.2” at least in this version of the manuscript.

L179: Add “that separates the obscure from well-visible bed reflections”, then come back to this result in the discussion of subglacial hydrological regimes and overdeepening of the bed.

L197: This pattern of overestimation along the glacier margins is likely an artefact of spatially-heterogeneous surface ablation, which could be partly corrected for with using a spatially-heterogeneous map of surface lowering, and not the mean value of the glaciated area.

L230-243: Great motivational paragraph

L240: For publication in The Cryosphere, this exercise would be desirable, given the strong motivational stage that the authors have just set up.

L255: Typo “Underestimation” should be “Overestimation”.

L253: But the models can be compared against each other in these upper regions of HEF, which is especially interesting given the steeper surface slope that controls model performance of GlabTop2.

L272: Is this meant to be Ruols et al., 2025, who present data for a single glacier (Otemma glacier), so reword this sentence to a single successful survey, or add other citations.

L273-278: The ice-rise appears to be a strong control on these patterns, we suggest further discussing these observations in light of the hydrological potential.

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L282: The GPR-surveyed area of 1.3 km2 is somewhat difficult to interpret without context, consider rephrasing to “the lower tongue of HEF” or state the surveyed percentage of HEF’s total area including its tributaries.

L292: The presented results do not directly support these important conclusions. We suggest performing these exercises as further detailed in the major comments above.

L294: Remove this sentence

Consider adding an Appendix with figures of the remaining profiles, or a “Supplemental Information” section, where these figures can be seen.

We hope our feedback is helpful in improving the manuscript and in supporting a clear and robust presentation of this valuable dataset.

**Sincerely,**

**The Ice and Climate Unit**

Christian T. Wild

Patrick Schmitt

Rainer Prinz

Robert Peal

Nils Sandt

Leopold Schlagbauer

Simon Widy

Marie Schroeder

Jan Niklas Richter

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