

Response to public commentary by ACINN

We thank the Ice and Climate Unit (ACINN) at the University of Innsbruck for their detailed review and for the local knowledge of Hintereisferner they bring to the evaluation of our work. We have considered all comments and will address them below.

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.

Response: Thank you for noting this. We are going to add some details on subglacial valley geometry to our manuscript, but we want to avoid the overinterpretation as the possible connection between the location of rise and bedrock signal strength is very speculative and would require specific investigations in detail which falls out of the scope of this paper. Thus, we would like to avoid the direct comparison between data sets you mentioned and our study due to data and methodological discrepancies, but we are definitely including these references for the support of our interpretation.

“We do not further expand the probable connection and causes between the bedrock rise and bedrock signal weakening up-glacier due to the lack of other evidence, and to avoid overinterpretation. But it is worth to note that the bedrock rise and overdeepening has been reported in the previous studies as well (Förtsch and Vidal, 1956; Span et al., 2005).”

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.

Response: Thank you for the comment. We agree that the conclusions as currently written overstate what is demonstrated by our study. We will tone down the conclusions accordingly,

removing claims that our ice thickness data directly enables improved model calibration and that this reduces uncertainty in future projections. These will be reworded to make clear that our dataset provides a benchmark that *could* be used in future calibration efforts, rather than implying we have demonstrated improved model performance.

Regarding the discussion around L240, we will revise the text to more clearly frame the rheological and basal sliding arguments as possible contributing factors rather than demonstrated mechanisms (the language will be softened). We additionally note that the intention of this study was to evaluate model performance as it would be applied in practice, i.e., without prior knowledge of the true ice thickness, using default parameterisations. This reflects the real-world scenario faced for most glaciers globally, where no thickness data exist for calibration. Tuning of models after the fact, to match known observations, while valuable, is a separate task and was not the aim of this study.

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.

Response: Thank you for your concern. Regarding reproducibility, we note that neither the DEM nor the glacier outline used in this study can be deposited in our repository, as both were provided by Rainer Prinz (Department of Atmospheric and Cryospheric Sciences, University of Innsbruck) and are not ours to redistribute. Both data sources are clearly documented in the manuscript and Zenodo repository metadata. Should these data be made publicly available by their owners, we would be happy to add direct links to the relevant repositories.

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

Response: Thank you for noting this. Upon review we identified that `eIntervals` in config file was inadvertently set to 14 rather than the default value of 20 during testing and was not reset prior to the final model run. We apologise for this oversight and will correct it in the revised manuscript. We have rerun GlabTop2 with the correct default value of `eIntervals` = 20 and confirmed that the results are not sensitive to this parameter choice. The mean modelled thickness over the 1313 GPR measurement points changed by less than ~0.5 m and the mean bias by less than ~0.5 m. All other GlabTop2 parameters were kept at their default values as specified in the model configuration. The Zenodo repository will be updated with the corrected configuration file.

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/m^3). As HEF is moving slowly, surface elevation changes by ice flux divergence can be neglected.

Response: Thank you for this suggestion. We agree that a spatially variable thinning correction based on elevation band mass balances is more appropriate than a glacier-wide mean value. We will implement this correction using cumulative mass balance data for the respective elevation zones available from the WGMS database and update the Millan et al. (2022) comparison statistics accordingly in the revised manuscript.

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.

Response: Thank you for the comments. Regarding “params.cfg”: we will update the script to load this file correctly.

Regarding MB calibration: we agree that calibrating against Hugonnet et al. (2021) geodetic mass balance is not ideal in this context, as those estimates are valid for the 2003 RGI outline, which differs from the custom outline used in this study. We will therefore update the calibration to use WGMS direct glaciological observations (`tasks.mb_calibration_from_wgms_mb`) available for Hintereisferner.

Regarding ice dynamic parameters: we used the OGGM default values (`inversion_glen_a = $2.4 \times 10^{-24} \text{ s}^{-1} \text{ Pa}^{-3}$` , `inversion_fs = 0`), which we will add in the methods section in our manuscript. We could use the calibrated value available from the OGGM pre-processed directory you provided; however, these values are derived by tuning `glen_a` until OGGM's inversion agrees with the ice volume of Farinotti et al. (2019). That volume is itself inferred from different models, and using the RGI6.0 outdated outline (Farinotti et al., 2019, Methods). Thus, the calibrated `glen_A` inherits all the potential problems from the models, as well as outdated RGI outline, that you already noted was a problem in our previous MB calibration strategy.

Preliminary testing with the calibrated *glen_a* value does show an improvement with GPR observations; however, given that this value was derived through a three-step chain of potential biases: the Farinotti et al. (2019) volume was itself modelled, then used to calibrate *glen_a* against the RGI6.0 (2003) outline, and finally would be applied here to a different outline and DEM representing 2023 glacier geometry. Therefore, it is not possible to determine whether this improvement is a physically meaningful parameter choice or a cumulative summation of errors through each step of this chain. We therefore consider it more transparent and scientifically defensible to retain the default values throughout. A comprehensive sensitivity analysis of ice dynamics parameters is beyond the scope of this study, which focuses on benchmarking three models under consistent configurations.

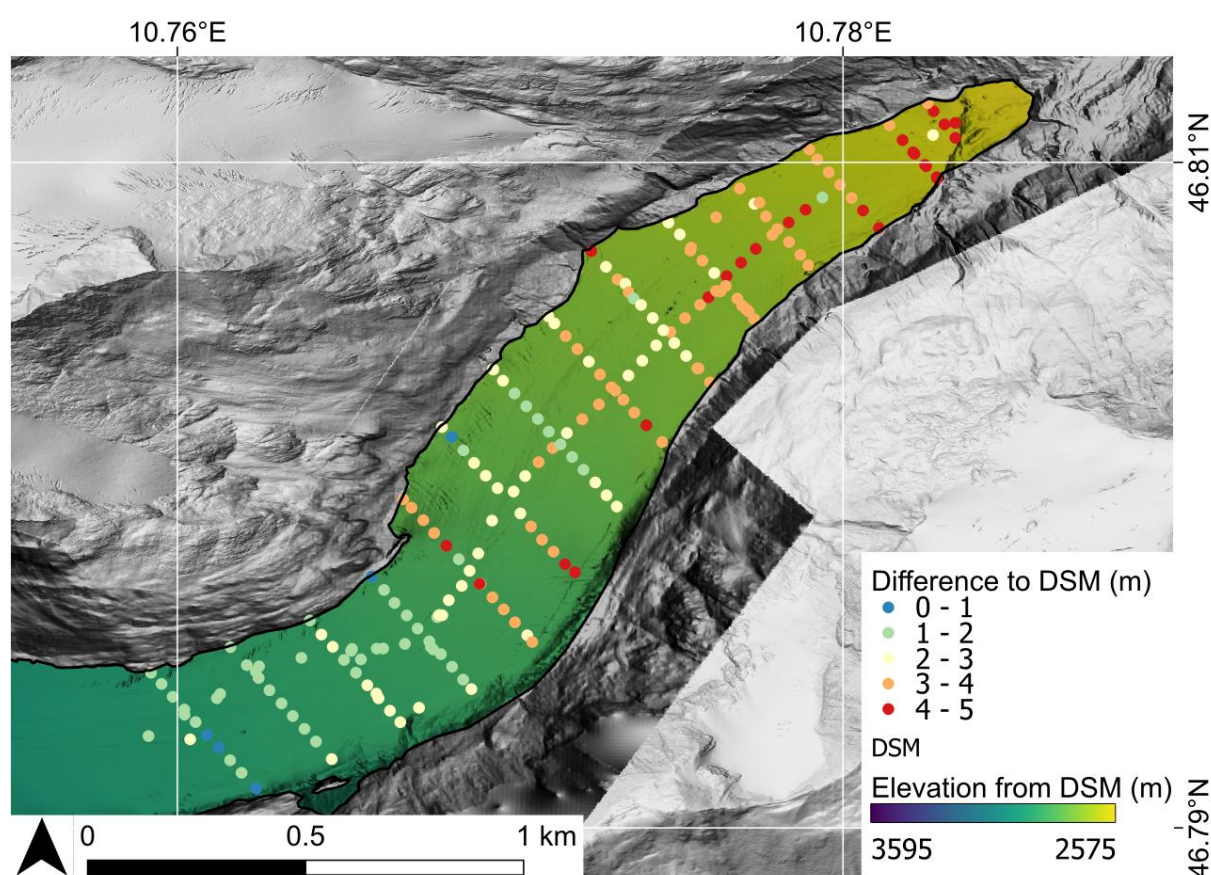
3.3) Radar interpretation and elevation correction require clarification and improved consistency

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.

Response: Thank you for the comment. Radargrams with overlaid bed picks for all profiles will be added to the Appendix or SI, and the along-flow profile to the revised manuscript, allowing others to evaluate the picking strategy directly. It must be noted that reflections were clearer on the profiles perpendicular to the glacier flow direction. Such observations have been published previously by different authors. This is reason why we did not include the along-flow GPR profile to begin with. Regarding the concern about picks in sections without a clear bed signal, we acknowledge that GPR reflection interpretation inherently involves a degree of professional judgement, particularly in zones of complex stratigraphy, strong attenuation and so on, where the bed reflection may be weak but still identifiable to an experienced interpreter. This subjectivity is an intrinsic characteristic of GPR data analysis and other geophysics methods where data interpretation is a big part of the method itself. Where picks were assigned in less clear sections, these were based on continuity of the reflection horizon from adjacent clearer sections and by looking at the overall reflection patterns around.

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 ($\sim 5.0 \text{ m a}^{-1}$ 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.

Response: Thank you for the comment. We agree, however precise GNSS measurements are only for GPR profile start and end points ~50 m apart, and do not provide continuous elevation coverage along the profiles. Thus, the overall quality of the available DSM is better than it would be interpolated from the relatively scarce GNSS measurement points. However, we definitely agree with your comment that elevation differences between DSM and GNSS measurements should be shown and discussed with a possible influence on measured ice thickness, and we will add this to our revised manuscript. Overall, it should be noted that the DSM–GNSS elevation difference is on average ~2.8 m, ranging from less than 1 m to ~5 m, with the largest discrepancies concentrated in the upper tongue and along the right lateral margin (preliminary figure below). However, the spatial extent of model thickness overestimation is considerably broader than these high-discrepancy zones, indicating that surface elevation errors from the one-year DSM offset cannot fully account for the observed spatial pattern of thickness bias.



3.4) Radar wave velocity assumption and uncertainty analysis

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 $\sim 5.0 \text{ m a}^{-1}$ 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).

Response: Thank you for the comments. Initially we were planning to use the CMP method, but equipment malfunction prevented us from carrying it out. Also, we could not use hyperbola fitting, because simply there were no clear hyperbolic reflections.

Regarding the radar wave velocity assumption, we note that our chosen value of 0.168 m/ns is consistent with GPR studies on temperate Alpine glaciers, for example, Church et al. (2020) used 0.1689 m/ns for the temperate Rhonegletscher, Switzerland, and Rutishauser et al. (2016) applied the same for Swiss Alpine glaciers. The Falljökull (Iceland) value of 0.156 m/ns represents an exceptionally water-saturated glacier and was cited precisely because it provides a conservative lower bound on velocity and thus an upper bound on possible temperate ice EM velocity-induced uncertainty. The intense englacial scattering visible in our radargrams indicates possible significant water content in HEF, which motivated our choice to include this extreme bound in the uncertainty analysis to ensure we do not underestimate the potential effect of water content on our derived thickness. Using this value, we calculate a worst-case 7% systematic overestimation of GPR-derived ice thickness, which is already quantified in the manuscript at L136–149.

Regarding picking uncertainty, bed picks for several profiles were performed independently by two experienced GPR specialists. We note that our $\pm 2.2 \text{ m}$ picking error estimate is derived directly from the physical constraints of our antenna frequency (38 MHz) and the applied EM velocity, and is grounded in established methodology following the standard approach of Navarro and Eisen (2009), a widely adopted reference for GPR uncertainty estimation in glaciological applications. This value is also explicitly quantified in the manuscript at L136–149. Your suggested estimate of $\sim 10 \text{ m}$, while not impossible in cases of very ambiguous bed reflections, is not representative of the overall dataset and is not derived from a cited quantitative approach. We also note that one interpreter was consistently more conservative in their picks while the other identified the bed at some additional locations, and we retained the picks from the more conservative interpreter throughout.

Regarding the elevation correction due to different DEM acquisition year of 2022 and our survey date of 2023, we note that the DEM has no effect on GPR-derived ice thickness at all, as thickness is directly calculated from the two-way travel time between the surface and bed reflections. Therefore, the DEM temporal offset is not a source of GPR uncertainty. However, we acknowledge that both GlabTop2 and OGGM used the 2022 DEM as input, meaning the modelled

surface was on average ~2.8 m higher than the true 2023 glacier surface, with spatial variation ranging from less than 1 m to ~4.8 m (as mentioned in the response to the comment above). Both GlabTop2 and OGGM derive ice thickness from surface slope via the driving stress equation and determine glacier geometry from relative elevation differences. A spatially variable surface lowering of 0–4.8 m produces negligible changes in surface slope and relative geometry. To directly quantify this effect, we reran both GlabTop2 and OGGM with a DEM uniformly lowered by 5 m (exceeding the maximum observed surface lowering of 4.8 m). The mean modelled ice thickness changed by less than 1 m for OGGM and by an even smaller amount for GlabTop2, confirming that the DEM temporal offset has a negligible effect on model output relative to the reported model biases.

We note that the bedrock elevation map (Fig. 4d in the manuscript) is derived by subtracting GPR-derived ice thickness from the 2022 DSM surface elevation and therefore inherits the ~2.8 m average DSM offset in the bedrock elevation values. However, this map is presented for visualisation purposes only and no quantitative conclusions are drawn from it in the manuscript.

Regarding cumulative uncertainty in GPR-derived ice thickness, the only systematic error source is the velocity assumption, amounting to a worst-case 7% (~5.7 m), while the picking uncertainty of ± 2.2 m is bidirectional and effectively cancels out. This would yield a corrected mean GPR thickness of ~75 m (instead of ~81 m), increasing all reported model biases accordingly. The cumulative uncertainty therefore strengthens rather than weakens our conclusions. We note that model results will be updated in the revised manuscript following other methodological corrections suggested in this review, but the direction of the uncertainty argument remains unchanged regardless of the specific updated values.

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.

Response: Thank you for the suggestion, we will do that.

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.

Response: Thank you for the suggestion, we will do that.

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.

Response: Thank you for the suggestion, we will do that.

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?

Response: Thank you for the comment. As noted in the figure caption, the features labelled "2" are interpreted as reflections from supraglacial channels and crevasses. This interpretation is supported by their characteristic hyperbolic diffraction pattern, consistent with point or line scatterers such as crevasses and supraglacial channels. These features are not persistent across all radargrams, consistent with the spatially variable distribution of surface crevassing and channelling observed in the field. As they are surface-related noise sources rather than englacial structures of glaciological significance, we did not consider them further in the interpretation. However, we will add a brief clarifying sentence to the manuscript to make this explicit.

Fig.4 (new Figure 3 in revised manuscript):

(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.

Response: Thank you for the suggestion, we will do that.

Fig.5 (new Figure 4 in revised manuscript):

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).

Response: Thank you for the suggestion, we will do that.

Fig.6 (new Figure 5 in revised manuscript):

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.

Response: Thank you for the suggestion, we will do that.

Table 1:

We recommend adding standard deviations ($\pm 1\sigma$) 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.

Response: Thank you for the suggestion, we will do that.

Minor comments:

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

Response: Thank you for the suggestion, we will do that.

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

Response: Thank you for the suggestion, we will do that.

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).

Response: Thank you for the suggestion, we will do that.

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.

Response: Thank you for the suggestion, we will do that.

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.”

Response: Thank you for the suggestion, we will do that.

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)

Response: Thank you for the suggestion, we will do that.

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

Response: Thank you for the suggestion, we will do that.

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.

Response: Thank you for the suggestion, we will do that.

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

Response: Thank you for the suggestion, we will do that.

L50: Reword “one of the world reference glaciers of the WGMS” to “one of 60 reference glaciers of the WGMS”

Response: Thank you for the suggestion, we will do that.

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).

Response: Thank you for the comment, we will fix this.

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

Response: Thank you for the suggestion, we will do that.

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

Response: Thank you for the suggestion, we will do that.

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

Response: Thank you for the suggestion, we will do that.

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

Response: Thank you for the suggestion, we will do that.

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

Response: Thank you for the suggestion, we will do that.

L79: Reword "Methods" to "Data and Methods"

Response: Thank you for the suggestion, we will do that.

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.

Response: Thank you for the suggestion, we will do that.

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).

Response: Thank you for the comment. This concern is addressed comprehensively in our response to comment 3.4 above.

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

Response: Thank you for the comment. Multiple terms are used in scientific literature: Radargrams, GPR profiles, B-scans, GPR section etc, none of which is inherently more correct than the others. The term B-scan originates from approach where GPR data is classified in 3 types: A-scan – more commonly known as a single trace; B-scan – classical 2D GPR data representation; C-scan - more commonly known as a slice map. In different regions different terms are used. Yes, some are used more often than others. If reviewers prefer GPR profiles, we will correct manuscript accordingly. We agree that strict standardisation of terms used in GPR literature would be helpful.

L158: Add "covering about 1.3 km² of the lower tongue of HEF (revised Fig. 1)"

Response: Thank you for the suggestion, we will do that.

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.

Response: Thank you for the comment, indeed this term may be confusing in case of glaciers, however it is basically the same as well-known term in hydrogeology meaning the potential surface to which artesian water would rise in boreholes and is described in mentioned and other publications as well and also well demonstrated in other studies. We are going to add some more details and references such as “...and demonstrated by a combination of GPR measurements and borehole data by Jania et al. (1996). Also, Bradford and Harper (2005) found a similar distinct boundary (upper low-water content/lower high-water content layer) in temperate glacier in Alaska, which specifically coincided with the average piezometric surface measured in boreholes. Therefore, the lack of EM wave scattering suggests that the transparent upper ice is not a cold ice but reflects the boundary between high and low water content within temperate ice due to the absence of water that could trigger such scattering.”

We would not like to extend the discussion on this issue more here as it was done by Lamsters et al., 2020b.

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

Response: Thank you for noticing, we will fix this.

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.

Response: Thank you for the suggestion. We will expand the description of the glacier bed and bedrock rise slightly, adding references to previous investigations as suggested. However, we would like to avoid an extended discussion on the implications of the bedrock rise for subglacial hydrology, as this would be too speculative without borehole or other supporting data. We also note that you raised concerns in comment 3.3 about bed picks assigned in sections of weak signal visibility, and we don't deny that the bed reflections were weak in places and completely invisible in others. Therefore, drawing conclusions from those weak and at places ambiguous reflections would risk overinterpretation and would be highly speculative.

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.

Response: Thank you, but we are not sure we understand the comment - if you are suggesting that spatially heterogeneous surface lowering could explain the marginal overestimation pattern, we note that even in the worst case, elevation correction errors would amount to at most a few metres, consistent with the observed DSM–GNSS differences of up to 5 m. This is negligible compared to the modelled thickness overestimation of over 100 m along the margins and therefore cannot be the primary explanation for this pattern.

L230-243: Great motivational paragraph

Response: Thank you!

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

Response: Thank you for the suggestion. We agree that a sensitivity analysis using temperate ice rheological parameters would be a valuable exercise and would strengthen the discussion. However, implementing such an analysis across all three models (each with different underlying assumptions and input requirements) is beyond the scope of the present study, which focuses on benchmarking model performance under standard default configurations rather than parameter optimisation. The rheological discussion at L240 is intended to provide physical context for the observed biases, though as mentioned before, we will soften the language in this section to make clear that these are proposed mechanisms rather than demonstrated causes and suggest that systematic rheological sensitivity testing would be a valuable direction for future work.

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

Response: Thank you for noticing, we will fix this.

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.

Response: Thank you for the suggestion, however, without GPR data in the upper glacier area, comparing models against each other would only quantify inter-model disagreement rather than actual model performance, which is beyond the scope of this study.

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.

Response: Thank you for the suggestion, we will do that.

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.

Response: Thank you for the suggestion, we will investigate whether there are any strong relations between the bedrock rise specifically and GPR signal strength, or whether other features may be responsible for the observed patterns. However, as mentioned previously, we would like to avoid an expanded discussion on the implications of the bedrock rise for subglacial hydrology, as this would be too speculative without borehole or other supporting data.

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.

Response: Thank you for the suggestion, we will do that.

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

Response: Thank you for the comment, as we mentioned before – we will remove conclusions, that are not directly based on our findings.

L294: Remove this sentence

Response: Thank you for the suggestion, we will do that.

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

Response: Thank you for the suggestion, we will do that.

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