Author’s response to reviewer comments

Contents

We thank the community reviewer, both anonymous referees, and Adriano Ribolini for their thorough and constructive evaluation of our manuscript. Below we provide a point-by-point response to all comments raised during the interactive discussion. Reviewer comments are reproduced in plain text; our responses are given in blue-tinted paragraphs below each comment.

Response to Anonymous Reviewer 1

Line 90: The manuscript does not specify the date of the GPR campaign nor the snow-cover conditions at that time. The reader is left to assume that the glacier was snow-free, which may not be correct.

Response: GPR campaign date and snow-cover conditions: the survey was conducted in August 2023 (stated in line 80), but you are correct to point out that additional information is needed. In revised manuscript we have added (L76-77) that the survey took place from 2nd to 7th of August, and all GPR profiles were recorded on a snow-free glacier surface (conditions visible in Fig.A1).

Figure 5: The current caption should explain what the sub-panels a)–f) represent, making the figure easier to interpret.

Response: we agree that the caption should be more descriptive. Note that this figure is renumbered as Figure 4 in the revised manuscript. The figure itself has more visible titles to distinguish between models. The revised caption now reads: "Modelled ice thickness (a, c, e; contour interval 20 m) for GlabTop2, OGGM, and Millan et al. (2022), and the corresponding difference between modelled and GPR-interpolated thickness (b, d, f). Positive values in (b, d, f) indicate model overestimation relative to GPR-interpolated thickness. Background hillshaded DEM data source: basemap.at (basemap.at, 2024)."

Line 255 : It is written that "using a constant EM wave velocity representative of cold ice likely leads to a slight underestimation of the true thickness in temperate ice". To my understanding, it should be "a slight overestimation of... ".

Response: thank you for catching this error! You are correct that a cold-ice EM wave velocity (0.168 m ns^{-1}), which is higher than the true velocity in water-rich temperate ice, causes calculated thickness to be *greater* than true thickness, i.e., an overestimation rather than an underestimation. This section of the manuscript has since been substantially revised; the relevant section is now expanded and covers L314-317.

Response to Anonymous Reviewer 2

General Comments

The main focus of this paper is to compare existing ice thickness models, with a specific application to the Hintereisferner glacier in the Austrian Alps. While this topic has been addressed in previous studies, as mentioned in the introduction, the depth and scope of this analysis appear to push the boundaries further than what I've seen before. It certainly adds valuable insights to the field. It is particularly interesting to note the consistent overestimation of ice thickness across all models - a finding that warrants further discussion.

The methods of analysis are clearly explained. I especially appreciated the attention to detail regarding uncertainties linked to velocity and the decision to compare models only at GPR points (to avoid additional uncertainties from cubic interpolation).

However, I believe the manuscript could be slightly refined by placing more emphasis on the model comparison rather than the data acquisition. For example, the conclusion currently dedicates equal space to both topics, but the model comparison is where the paper truly shines. Additionally, I noticed a lack of cross-citations throughout the paper. While one reference is often provided, I would recommend citing at least three relevant studies when available. This would not only strengthen the validity of the points made but also better acknowledge the extensive work done in the past on these topics. Overall, previous work could be acknowledged more thoroughly.

That said, the paper is already of high quality, and after addressing these comments, I would strongly support its publication. I therefore encourage acceptance with minor revisions.

Thank you for your comments and recommendations that would improve our manuscript. We address your comments below.

We agree that the model comparison is the paper's primary contribution and have put more emphasis on that in our revised manuscript. We also acknowledge that the paper would benefit from more references to other studies and have added them where applicable.

Specific Comments

Line 67: The previous use of GPR on glaciers could be discussed more broadly, especially if you keep emphasizing your data acquisition.

Response: we usually describe our GPR methodology in a manner that allows the reader to fully understand how the data was gathered and processed which we consider important for transparency and scientific rigour.

Regarding a broader introduction to GPR applications in glaciology, we note that this topic has been thoroughly covered in dedicated resources, including the foundational work by Bogorodsky et al. (1985) on radioglaciology and the GPR chapter in Pellikka and Rees (2009). We have added references to these (L85-86) and other relevant recent studies in the revised manuscript, while keeping the introduction focused on aspects directly relevant to our study. We hope this represents a satisfactory compromise between accurate representation of previous studies and conciseness of the introduction.

Line 83: I don't question the data - it's often challenging to obtain the most accurate measurements - but the time gap between the DSM acquisition (September 2022) and the GPR data (August 2023) seems significant. Unless I missed it, this isn't addressed later in the paper. I could be wrong, though.

Response: yes, there is one-year gap, because of the issues with the laser scanner, thus only the data from the previous year was available. Thanks for mentioning this. To assess this influence, we used 193 GNSS points gathered on field at the start and end of each GPR profile and compared them to the DSM at the same locations. The elevation difference averages 2.8 ± 0.9 m (1σ), ranging from 0.4 to 4.9 m. This is negligible compared to the maximum model overestimation of up to 207 m and cannot account for the observed spatial pattern of thickness bias. We confirmed this directly by rerunning GlabTop2 and OGGM with the DEM uniformly lowered by 5 m (exceeding the maximum observed offset), which changed mean modelled thickness by less than 1% for both models. These details are now provided in the revised manuscript (Sect. 5.2 L317-327 and Appendix A L372-377 and Fig. A2).

Figure 1 caption: Please include the exact date(s) of acquisition.

Response: thank you for noting this issue. Note that the figure has been moved to Appendix A and caption has been updated with exact dates: "Figure A1: Field photographs from the GPR survey on Hintereisferner, Ötztal Alps, Austria, August 2023. (a) Aerial view of the GPR survey area on the lower glacier tongue, showing light snow cover following a snowfall (2 August 2023). (b) GPR survey in progress on snow-free glacier ice, showing two GPR operators carrying the Zond-xLF GPR system along a transverse profile (3 August 2023). (c) Moulin on the glacier surface, illustrating the crevassed and hydrologically active character of the lower tongue (5 370 August 2023)."

Line 90: Consider adding a link or reference to the full set of parameters (e.g., trace length, stacking number, sampling interval) for readers who may be interested.

Response: we have added the acquisition parameters to methods section L95-97: "We selected a time window of 95 2560 ns with a range per sample of 1250 ps for shallower parts and 2500 ps for deeper sections, and 2048 samples per trace, which allowed for a theoretical maximum penetration depth of ~210 m, assuming a dielectric permittivity of 3.2 (cold-ice value, Church et al., 2020; Robin, 1975; Rutishauser et al., 2016)."

Line 95: Why was this approach chosen? For example, is it based on proven methods, such as those discussed in Langhammer et al. (2017), which suggest a preferred orientation?

Response: there are two reasons why we usually orient profiles transverse to the longitudinal axis of a glacier. First, from a practical standpoint, walking perpendicular to the glacier's longitudinal axis avoids the need to continuously ascend and descend the glacier slope, which is both physically demanding and time-consuming. Second, and more importantly from a scientific perspective, englacial conduits are typically oriented parallel to the glacier's longitudinal axis. Recording profiles perpendicular to flow maximises the chance of crossing these features and produces the characteristic hyperbolic reflections that allow them to be identified and characterised. Profiles oriented parallel to flow would instead produce sub horizontal reflections from conduits that are much harder to distinguish, and there is a risk of missing conduits entirely if the profile does not pass directly overhead. Transverse profiles also better characterise the cross-sectional shape of the subglacial valley. These aspects represent standard GPR survey practice are described in numerous textbooks about GPR (for example Jol, 2008. Ground Penetrating Radar Theory and Applications).
We have added this explanation to the manuscript L100-103.

Section 2.1 (GPR surveys): Some points are based on very few surveys. Including more references would strengthen consistency. For instance, the velocity of 0.168 m/ns has been widely used and explained in previous studies.

Response: we have added more references i.e. Church et al. (2020) and Rutishauser et al. (2016), who used the same values in recent studies in European Alps. L123.

Section 2.2 (Thickness data and modeling): One global observation : why is the interpolation used for ? You only compare the location of the GPR data (which is consistent as the 200m spacing introduce high level of uncertainties between the lines) and don't really use the map to build ice volume I believe. Out of curiosity and on a personal note, I would be interested to know what's the results oft he model comparison when it comes to ice volume as well.

Response: interpolation was used only for visualization purposes (we mention this in Sect. 3.3), and all statistical comparisons are made at GPR measurement points only. However, in response to your interest, we have added volume estimates for the GPR-surveyed area as an additional metric (L258-263). The volume for the GPR-surveyed area is ~0.103 km^3, while the modelled volume is ~0.161 km^3 (RE 56.7%) for GlabTop2, ~0.194 km^3 (RE 89.0%) for OGGM, and ~0.175 km^3 (RE 70.9%) for the Millan model (without temporal adjustment). We must note that these numbers have changed from our previous response to your comment, since we reran GlabTop2 and OGGM models, based on other reviewer comments.

Section 2.3 (Uncertainty and accuracy assessment): This section is excellent, but again, more citations would be beneficial. For example, the fact that EM velocity decreases with increasing liquid water content has been discussed in numerous papers and should be acknowledged more broadly.

Response: we have added additional citations throughout Sect. 3.3 (L156-158). For the specific point on EM velocity decreasing with liquid water content, the relevant sentence now cites Bradford and Harper (2005), Delf et al. (2022), Murray et al. (2000, 2007), and Navarro and Eisen (2009).

Figure 4: Why does panel (a) not focus on the same area as the other three plots? Also, the figure is well done, but it might be helpful to add the locations of the GPR lines to panel (c) to better see (?). This would provide additional context for what's happening between the lines - though it's not crucial as again, I don't think you use this interpolation.

Response: we have updated this figure (now Figure 3) with all you suggestions (page 11).

Figure 5 (panels a, c, and e): Why do these panels not focus solely on the surveyed area? Additionally, I found it somewhat challenging to understand what each plot represents. Consider adding more detail to the captions (in addition to the legend) to improve clarity.

Response: we have updated this figure (now Figure 4) with all your suggestions (page 13).

Table 1: I understand what "n" represents, but it's only briefly introduced earlier. Renaming it to "number of data points" or something like this might make it clearer. Since "n" is only used twice, the abbreviation may not be necessary.

Response: we have added the description to Table caption to make it clearer. The caption now reads: "Table 1: Statistical comparison between GPR-derived and modelled ice thickness values, based on 1313 GPR measurement points."

Line 249: In the introduction, you cite several studies that compare models with data, but in the discussion, only Lamsters et al. (2024) is referenced. While I understand you may be most familiar with this study, it would be valuable to include notes on the others to identify any consistent trends in model estimation.

Response: we have added multiple new references to other comparison studies across the whole discussion section (and greatly updated the discussion in general).

Line 262: Could you provide examples of winter surveys as reference?

Response: we have added example from Church et al., 2020 (L338).

Line 269: More citations would be helpful here. Many helicopter/airborne surveys have been conducted, and their limitations (e.g., not always probing to the bottom) could be shortly introduced further.

Line 270: Drone-based GPR has probed depths of up to ~100 m (Klahold et al., 2026 - 10.1017/jog.2026.10145). It is shallower but is this still considered shallow?

Line 270: Ruols et al. (2023 and 2025) were not conducted on snow-covered glaciers.

Response to L269, 270 comments: Thank you for these comments and for the additional references. Upon further reflection, we decided that this discussion about drone and helicopter based GPR surveys are not relevant to our study and its main purpose, also taking into account your comments about main focus of this paper being modelling. Hence, we decided to remove this topic from discussion altogether. We did find the Klahold et al. (2026) and Ruols et al.

references you provided particularly useful context on UAV-GPR depth capabilities.

Line 273: I am not sure to understand if the comparison between the data and the model is better or worse on the parallel to flow line than on the other lines. Again, I feel that the strenght of this paper is in this comparison between models and the comment on the reasons why reflections are varied is here rather shallow to my view – as some papers have entirely focused on that.

Response: We agree that this is not the main focus of the paper and have deleted the paragraph from the revised manuscript.

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 have updated the manuscript and mentioned the ice rise and cited your suggested references. However, we want to abstain from overinterpretation. The relevant updated section can be found at L214-220.

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 have toned down our conclusions (L356-361).

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 have corrected it in the updated manuscript and in our Zenodo repository. All GlabTop2 data has been updated – from figures (Fig. 4,5) to statistics (Table 1), but as expected the change was almost negligible (less than 0.5%).

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 have applied the suggested change in the updated manuscript. Changes mainly visible in statistics (Table 1).

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. “params.cfg” file has been updated in Zenodo repository.

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 have updated the calibration to use WGMS direct glaciological observations (*tasks.mb_calibration_from_wgms_mb*) available for Hinteresiferner. This affected all OGGM results (Fig 4, 5 and Table 1, and in text). The results got worse throughout – overestimations got larger. We also added in text, that MB calibration was done against WGMS in-situ data (L135-136).

Regarding ice dynamic parameters: we used the OGGM default values (*inversion_glen_a* = 2.4×10^{-24} s^{-1} Pa^{-3}, *inversion_fs* = 0), which we have added these parameters in methods section (L141).
We could have 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 have been added to supplementary materials in our zenodo repository, however we did not add any new profiles to the manuscript itself, apart from the same profile 13 with overlaid picks was added to Figure 2 (page 9). We did not add along flow profile as the bed was only partially visible there and it would not add new information to the manuscript. 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 (~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.

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. To assess the influence of DSM temporal offset, we used 193 GNSS points gathered on field at the start and end of each GPR profile and compared them to the DSM at the same locations. The elevation difference averages 2.8 ± 0.9 m (1σ), ranging from 0.4 to 4.9 m. This is negligible compared to the maximum model overestimation of up to 207 m and cannot account for the observed spatial pattern of thickness bias. We confirmed this directly by rerunning GlabTop2 and OGGM with the DEM uniformly lowered by 5 m (exceeding the maximum observed offset), which changed mean modelled thickness by less than 1% for both models. These details are now provided in the revised manuscript (Sect. 5.2 L317-327 and Appendix A L372-377 and Fig. A2).

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 ~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).

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 ±2.2 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 ~10 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.
We have explained this all in the manuscript in discussion section (L314-327), as well as methods section (L151-168).

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 have moved the figure to Appandix A.

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 have applied the suggestions (Figure 1).

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 have applied suggestions both added picked images to our repository and updating the figure (Fig. 2).

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. We have added explanation in L196-199.

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 have updated the figure with all changes (Fig. 3).

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 have updated the figure (Fig. 4).

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 have updated the figure (fig. 5), however we did not include classification by profile lines as those plots matched exactly with classification based on elevation (since we record profiles transverse to flowline, profiles end up being on same elevation bands).

Table 1:

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.

Response: Thank you for the suggestion, we have done that (see Table 1 and in text everywhere where means or errors are mentioned).

Minor comments:

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

Response: Thank you for the suggestion, we have done that.

L25: Replace "for the last few decades" with " since YYYY"

Response: Thank you for the suggestion, we have done that (L22-23).

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 have done that (L23).

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 have cited Fischer et al., 2015 (L26).

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 have done that (L27-29).

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 have done that (L32).

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

Response: Thank you for the suggestion, we have done that (L39).

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 have done that (L44).

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

Response: Thank you for the suggestion, we have done that (L34).

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, have done that (L50).

L51: Use the correct glacier area of HEF (5.17 km^2 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 km^2, 2019) was still used and reported to the WGMS).

Response: Thank you for the comment, we have fixed that (L56).

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

Response: Thank you for the suggestion, we have created a study area section (L55-75).

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 have done that (L59-64).

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

Response: Thank you for the suggestion, we have done that (L66).

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

Response: Thank you for the suggestion, we have done that (L84-91)

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

Response: Thank you for the suggestion, we have done that (L92).

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

Response: Thank you for the suggestion, have done that (L76).

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 have done that throughout data and methods section.

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. We have used only "Gpr profiles" in the updated manuscript.

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

Response: Thank you for the suggestion, we have done that (L177).

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 have added explanation to manuscript in L190-196.

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

Response: Thank you for noticing, we have fixed it.

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, we have elaborated in the manuscript that DSM-GNSS difference cannot account for model overestimations (L320-325).

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 is intended to provide physical context for the observed biases. We have softened the tone (L292-301).

L255: Typo "Underestimation" should be "Overestimation".

Response: Thank you for noticing, we have fixed the typos.

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, however we have removed this paragraph altogether in the updated manuscript.

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, 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 have done that (L346).

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 (L356-361).

L294: Remove this sentence

Response: Thank you for the suggestion, have removed it.

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 have added all profiles to our data repository in Zenodo. The profiles are too large and too many, to add them to the manuscript itself.

Response to reviewer comments by Adriano Ribolini

Thank you for the careful reading of our manuscript and for the detailed comments. We have addressed all comments point by point below.

- The paper is essentially based on Ground-Penetrating Radar, and how this type of data can validate or reject model-based glacier thickness assessments. For this reason, I believe more attention should be paid to describing the individual steps of GPR data processing, data interpretation, and data visualization. It is unclear to me why data migration was not applied once the radar wave propagation velocity was determined.

The radar profiles do not appear to have been interpreted unambiguously; the areas indicated as characterized by strong scattering appear similar to others interpreted differently.

Some signals are not interpreted, despite being frequent in the two images presented.

Response: Thank you for the comment. We must note that the bed reflection was weak in many profiles, and in some cases bed detection was only possible by identifying changes in the overall character of the GPR signal scattering rather than a distinct reflection. Initially we tried to apply migration, however it had the adverse effect of further attenuating the weak bed signal, making the bed impossible to trace.

We acknowledge that the dataset is challenging to interpret. For this reason, bed picks were made only in sections where a reflection was traceable, and we deliberately avoided picking in sections where no reliable signal could be identified. Englacial reflections visible within the ice column were not interpreted, as their detailed analysis falls outside the scope of this study, although most are consistent with hyperbolic diffraction arms from point scatterers within the ice.

- The issue of propagation velocity is a delicate one, as is well known. However, interpreting radar images by emphasizing the existence of areas with different water content ("characterized by strong scattering signals") implies a non-constant propagation velocity of EM waves, whatever the value assumed. It could be useful to use a velocity model, even with values derived from the literature.

Response: We agree that EM velocity in a temperate glacier is likely spatially variable and likely exhibits 3D characteristics. However, without CMP measurements or clear hyperbolic diffractions (neither of which were available in this survey) it is not possible to determine spatially variable velocity values. 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 Rhonegletscher and Rutishauser et al. (2016) applied the same for Swiss Alpine glaciers. Using a single well-supported velocity value is standard practice in glaciological GPR studies and provides transparency about the assumptions made. To show the magnitude of this assumption quantitively, we used an extreme temperate ice value of 0.156 m/ns (Murray et al., 2000), representing a highly water-saturated glacier, that showed that in a worst-case scenario there would be a 7% systematic overestimation of GPR-derived ice thickness (~5.7 m on our mean thickness of ~81 m), confirming that the velocity assumption has no significant effect on our conclusions. (L153-169 and L314-317).

- The (admitted) uncertainties in choosing a constant velocity value, the possible lateral/vertical velocity variations, and the (in my opinion) unclear interpretation of zones of the radar profiles could undermine the validation of glacier thickness based on modelling.

Response: We acknowledge that the dataset presents interpretational challenges, as discussed in our previous responses. However, the differences between modelled and GPR-derived ice thickness are substantial - reported model biases of 40–60 m far exceed any plausible GPR uncertainty. Furthermore, as quantified above, using a lower temperate ice velocity would reduce GPR-derived thickness, which would increase rather than decrease the apparent model bias. The conclusions of this study are therefore robust regardless of the specific velocity assumption made.

- finally, I believe the GPR methodological part should be enhanced, and the interpretation improved, also in light of additional processing steps.

Response: Thank you for the suggestion. The main GPR processing steps are already described in the manuscript: bandpass filtering (Ormsby filter, 20–70 MHz), manually adjusted time-dependent signal gain function, topographical correction using the DSM, and bed reflection picking at approximately 10 m intervals. We have added acquisition parameters (L95-98).

Comments in the .pdf file:

P4. please provide (in brackets) according to which EM velocity, and how this velocity was assumed a priori

Response: We used ε=3.2, and have added this to the manuscript (L97-98).

P5. Please explain what you actually picked. A travel time (ns) picked every 10 m is not fully clear to me. Perhaps it is a travel-time depth converted?

Response: We picked two-way travel times of the basal reflection from each GPR profile. These travel times were subsequently converted to ice thickness values using the assumed EM velocity of 0.168 m/ns. The ~10 m spacing refers to the horizontal distance between individual picks along each profile, not a depth interval. We have clarified it in the manuscript (L113-119).

P5. do you mean internal to the glacier? can the existence of scattering points led to point-sourced hyperbola diffractions? Is it possibile that the absence of hyperbolas is related to the low central frequency of the anrtenna?

Response: Yes, we are referring to scattering points internal to the glacier, most likely water inclusions. Point sources in general produce hyperbolic diffractions, but this is not always the case. The visibility of hyperbolas depends on the size of the scattering object relative to the wavelength, the antenna frequency, and the contrast in dielectric properties. The low central frequency of our 38 MHz antenna results in a wavelength of ~4.4 m in ice (calculated as v/f = 0.168 m/ns / 38 MHz). The commonly cited rule of thumb requires scattering objects to be at least $\lambda/3 \approx 1.5$ m in size to produce detectable hyperbolas, though published values vary and this threshold is not always met in practice even when larger scatterers are present. In practice, from our experience working with GPR antennas across a wide frequency range (38 MHz to 2 GHz) in various environments, the theoretical conditions for hyperbola formation are not always reliably met in field settings.

P5. Did you also consider other values from more modern publications?

Response: We selected the most widely used and well-supported value for glacier ice (0.168 m/ns), which is also consistent with direct measurements from temperate Alpine glaciers as discussed in our previous responses. We do not consider a more recently published value to be any more accurate for this specific glacier, because without direct velocity measurements, any value selected from the literature carries the same fundamental uncertainty. The sensitivity of our results to this choice is already quantified in the manuscript, where using the extreme temperate ice value of 0.156 m/ns yields a worst-case 7% systematic overestimation, confirming that the velocity assumption does not affect the conclusions of this study (L153-169 and L314-317).

P5. The data processing section seems a bit limited to me. The GPR data and their interpretation are crucial to this paper, and data processing would have deserved a little more space. For example, the signal gain function is not described, and a power spectrum justifying the bandwidth selection is not shown. Why was migration not attempted?

Response: Thank you for the comment. Regarding the signal gain function, we applied a manually adjusted time-dependent gain function based on the classical assumption of exponential EM wave attenuation with depth. Manual gain adjustment is a standard and widely used approach in GPR data processing, particularly when the primary goal is to identify and trace specific reflections rather than preserve amplitude fidelity.

Regarding the power spectrum and bandwidth selection, the Ormsby bandpass filter parameters (20–70 MHz) were centred on the nominal antenna frequency of 38 MHz. In the low-frequency range applicable here, dielectric permittivity shows weak frequency dependence, which significantly constrains the meaningful spectral range available for filtering. Bandpass filter selection at low antenna frequencies is therefore relatively straightforward, and a power spectrum figure is not standard practice in glaciological GPR publications, at least we are not aware of any published glacier GPR study that includes such a figure.

Regarding migration, this is addressed in our response above.

P6. If you are referring to vertical resolution, could you explain why it's not lambda/4? Or do you mean ±lambda/2?

Response: The λ/4 criterion refers to the theoretical vertical resolution limit under ideal conditions, where the minimum separation between two distinct reflectors can be distinguished. As noted by Navarro and Eisen (2009), in practice this may worsen to λ/2, which represents a conservative estimate of picking uncertainty for a single reflection. The figure below illustrates this distinction: at λ/4 separation the two reflectors are just barely resolvable, while at λ/2 they are relatively clearly separated, demonstrating that λ/2 represents a realistic upper bound on the uncertainty in identifying the true peak position of a weak reflection. Given the weak and noisy bed reflection in our data, where the wavelet peak cannot always be unambiguously identified, we consider ±λ/2 the appropriate and conservative choice, fully consistent with Navarro and Eisen

(2009). We have updated to ±λ/2 in the manuscript (L165).

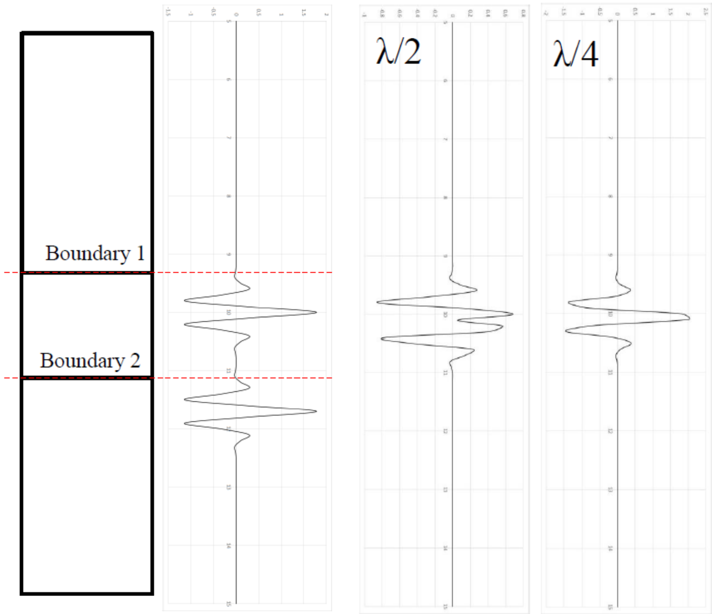

Figure: Schematic illustration of vertical resolution concepts showing two boundaries separated by λ/2 (left) and λ/4 (right). It is our own simulation for illustrative purposes.

P6. I think the problem could be a little more complicated. If you've identified parts of the glacier as potentially water-rich, this means that the velocity can vary vertically and laterally. So assuming a constant velocity, and evaluating a bias while also considering another constant velocity, I'm not sure can lead to a very refined evaluation of the picking error. However, I also believe that in the end, the percentage deviation shouldn't be relevant for the purposes of the paper. And besides, without CMP surveys and diffraction hyperbolas, there aren't many other alternatives.

Response: We agree that a spatially variable velocity would complicate the uncertainty assessment, and that a precise evaluation is not possible without CMP surveys or clear hyperbolic diffractions - as you already note in the comment. The ~10% worst-case uncertainty cited in the manuscript represents the combined effect of the velocity assumption (~7%, derived from using the extreme temperate ice value of 0.156 m/ns) and the picking uncertainty (~3%), assuming both act in the same direction simultaneously. In practice these uncertainties are unlikely to fully combine, and the true uncertainty is likely lower. We agree with your conclusion that given the available data, there are no better alternatives for velocity determination, and add that regardless of the precise uncertainty estimate, the reported model biases far exceed any plausible GPR uncertainty.

P7. In the radar profiles in Fig. 2, two sets of regularly oblique reflections intersecting at approximately 90° are observed (particularly evident in Fig.2 b, I tried to mark on the figure). I would like to ask whether these could correspond to ice layering, or to hyperbola arms that locally interfere positively, or to noise that could be eliminated with FK filtering.

Response: Thank you for this observation. The marked oblique reflections visible in Fig. 2b, we interpret as arms of hyperbolic diffractions from point scatterers within the ice. FK filtering could theoretically suppress some of these features but given that the bed reflection is already at the noise level, applying FK filtering risked further attenuating the weak bed signal and was therefore not attempted.

In Fig. 2b, the area indicated as characterized by strong signal scattering doesn't seem very different from many other parts of the profile.

Response: We agree - the scattering characteristics in the indicated area are not clearly distinct from other parts of the profile. We have removed this from the manuscript as it may be misleading. In this case we meant signal scattering that is visible basically everywhere in radar images and created by temperate ice.

It might be useful to apply a constant velocity migration.

Response: As we explained to the comments before, migration was attempted during data processing. However, in profiles where the bed reflection was already at or near the noise level, migration did not improve bed visibility and in some cases made bed tracing more difficult. We therefore proceeded without migration for those profiles.

P7. Is this the only possible interpretation? I would be more cautious.

Response: Of course, there is also possibility that there are debris incorporated into the ice, but in this case and many other cases in temperate alpine glaciers, also glaciers in Iceland, Alaska etc., it is commonly interpreted as an indication of temperate ice with water inclusions. If there are some scattered debris inclusions, they would produce the similar signal characteristics locally, larger debris bands however would create planar reflections what was not in this case. Reflections from possible debris bands, for example, we encountered in Greenland (Lamsters et al., 2024) with clearly notable planar expression. We do not see any convincing signals that could be attributed exclusively to debris bands in this case due to high water content and scattering created by that primary.

P7. Could you mark the reflection generated by this surface on the GPR profile?

Response: Yes, we have marked the piezometric surface by arrows in Figure 2.

It is not clear to me: if there is a piezometric surface, does this mean that everything below it is saturated? If so, why does the signal scattering due to water content only occur in a limited portion of the subsurface?

Response: Thank you for the comment, yes, that theoretically means that the ice below contains voids filled with water. All water inclusions below generate radar signal scattering characteristic for temperate glaciers, especially Alpine ones as described by Murray et al. (2000), Lamsters et al. (2020b), and we are going to add more references and details in the updated version of the manuscript 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." (L189-196).

We would like not to go in more details as more extended discussion of this issue is already present in literature and also in our previous study from Iceland (Lamsters et al., 2020b).

Signal scattering we are referring to does not occur in the limited portion of subsurface but practically everywhere, as mentioned before, except the very topmost part of some radar images. More intense scattering of vertically stacked hyperbolae in some profiles is referred as crevasses and/or supraglacial streams.