

Response to the RC2 comments on manuscript

(<https://doi.org/10.5194/egusphere-2025-615>) by Akash M. Patil and colleagues entitled “Investigating firn structure and density in the accumulation area of Aletsch Glacier using Ground Penetrating Radar” submitted to The Cryosphere.

23.06.2025 by Patil et al

Response to General Remarks

+ Structure: While the sections of the manuscript are generally well structured, the text would benefit a lot from better structuring, i.e., explaining the more general before going into details. This applies especially to the Abstract, Discussion, and Conclusions, as well as the captions for the Figures and Tables. I strongly recommend rewriting all captions, starting with (i) a short title, followed by (ii) a description of the content, (iii) explanations of axes, acronyms, and labels, (iv) notes or reading examples (if required), and (v) sources.

+ Structure: We appreciate your suggestions regarding the structure of the manuscript. We considered your remarks in revising the manuscript, along with a rewritten caption for all figures as suggested.

+ Readability: In its current form, the paper is hard to read, especially for “fast-food readers” who focus on the abstract, here-we-show-statement, figures, tables, and conclusions. To improve the readability, I encourage the authors to improve the structure and figure captions (see above), reduce the acronyms to the absolute minimum, and review the terminology (see below).

+ Readability: We considered your suggestions on readability of the manuscript, focusing on “fast food reader”, we improved the structure, figures and table captions. We also reduced the number of acronyms; however, we want to retain the most commonly used and essential ones, such as GPR, CMP, CFM, KM, and LIG. We also reviewed and made the necessary changes in terminology.

+ Terminology: The paper is not very consistent in its use of terms. Given the different Swiss place names (cf. Fig. 1), I suggest consistently using “Grosser Aletschgletscher”. Write “Alpine” when referring to the European Alps and “alpine” when referring to more general alpine environments. The (few essential) acronyms (e.g., GPR) should be introduced at their first use (only), and maybe written out in the captions. More questions related to terminology are listed in the specific remarks.

+ Terminology: We agreed to the suggested changes to be consistent with terminology. We made the necessary changes in acronyms in the revised manuscript.

+ Key findings: Better emphasize the novelty and key findings of the study. From the Abstract and Conclusions, it is not clear (to me) which methods or combination thereof are novel and which findings are key for future research. Also, you mention a validation of your results against glaciological stake measurements in the abstract, while Figs. 6, 13, and 14 are instead a comparison.

+ Key findings: We made the suggested changes in presenting our work, primarily with a focus on scientific novelty in our research. Yes, we compared our geophysical results with the stake measurements and also used the stake measurements in scaling the precipitation from Grimsel to Jungfraujoch (section 3.4.2 in the submitted manuscript). We changed the corresponding text while discussing the results of Figs. 6, 13 and 14.

+ Discussion: The discussion of the results remains relatively descriptive, and I see considerable potential for emphasizing the relevance of the findings. As such, you could discuss in more depth (and with the support of meteorological data) the presence (or absence) of seasonal and annual layers. Also, it would be interesting to discuss in more depth the impact of the increasing occurrence of summer melt in these formerly cold zones and the extreme summer 2022, which resulted in a net mass loss and erased an entire snow/firn layer. Finally, the authors mention that their GPR profile was a repeat measurement of the survey by Bannwart et al. (2024) from March 2021. It would be great to compare their GPR profiles (Bannwart et al. 2024, Fig. 5c) and firn cores (Bannwart et al. 2024, Fig. 6) from Ewigschneefeld, especially given the extreme years 2021/22 and 2022/23. Can you link the layers between the studies? Do you see any changes in the snow/firn densification profile? Figure 3 in Machguth et al. (2016) and Figure 4 in Sold et al. (2015) provide good examples of firn core comparisons.

+ Discussion: We appreciate your recommendation to emphasise our findings in detail. We considered your remark and tried to discuss the impact of the extreme summer of 2022. However, our firn core is not deep enough to depict the 2022 summer impact. We also discussed recent studies such as [Sold et al. \(2015\)](#), [Machguth et al \(2016\)](#), and [Bannwart et al. \(2024\)](#) by comparing them with our results in the revised manuscript. Our current manuscript aims at understanding the firn structure in detail using geophysical and glaciological measurements from the 2024 fieldwork, along with basic firn compaction modelling. Therefore, we argue that a quantitative comparison with [Bannwart et al. \(2024\)](#) is reaching too far at this stage. We already have an updated data set from the 2025 fieldwork (repeat measurements of the 2024 fieldwork), which we aim to use for analysing the temporal evolution of firn densification. In this context, we plan to carry out a detailed analysis and comparison of the existing data, including [Bannwart et al. \(2024\)](#) GPR measurements. However, in our revised manuscript, we compared and discussed the glaciological results from [Bannwart et al. \(2024\)](#).

+ Discussion & Figures: Your figures are all placed before the Discussion section. This is fine, but it would help to add (more) labels or reading examples that emphasize the topics discussed. As such, it would be interesting to see your age interpretation of the reflectors (in Figs. 2, 3, 4, 15, 16) and of seasonal/annual layers (Figs. 8, 9, 10, 11, 12).

+ Discussion & Figures: We introduced the labels on Figs. 15 and 16. We would like to keep the CMP dataset as simple as possible to help the reader understand the CMP semblance analysis (Figs. 3 and 4). We iteratively identified annual layers from Figs. 2, 3, 4 and 11 for which the labelling of annual layers makes all the mentioned figures more clumsy and chaotic. The same is applicable to Figs. 8, 9 and 10, in which we observe many ice lenses, and the introduction of ice lens thickness will make the figures unreadable. However,

we provide more figures in the appendix corresponding to the specific figures in the main text. If at all needed, we can move them to the main text.

+ Data availability: I strongly support publishing the dataset in a public repository such as Zenodo (<https://zenodo.org/>) or Pangaea (<https://www.pangaea.de>).

+ Data availability: We are planning to publish the dataset in either Zenodo or Pangaea upon acceptance of the manuscript, as mentioned in the submitted manuscript, line 564.

+ Methods: I note that GPR data processing and Common Mid-Point semblance analysis are outside my expertise. Here, I refer to the other reviewers and suggest considering their feedback on velocities picked from the semblance analysis (<https://doi.org/10.5194/egusphere-2025-615-RC1>).

+ Methods: We responded to the RC1 comments with the updated results as in <https://doi.org/10.5194/egusphere-2025-615-AC1>. Most of the results that depend on the CMP radar velocity picking have been changed, and the corresponding text has also been revised.

Specific remarks

Title

L0: The present title is fine. However, if your firm modelling is a key finding of the study, you might consider reflecting this in the title.

Response: We appreciate your suggestion; however, the present study primarily focuses on field methods to investigate the firm structure and density. Here, we aim to assess how well the firm compaction models represent the observations. We planned our future research on the detailed implementation of firm compaction models along with the geophysical observations. Therefore, we would like to keep the study title as it is now.

Abstract

L1: Consider rewriting to improve structure and to emphasize key findings better.

Response: Agreed to the suggestion. We improved the abstract as in the revised manuscript.

L10: Remove line break in abstract.

Response: Agreed, the line break has been removed.

Introduction

L18: Consider rewriting to improve the structure: background, state of the art, problem, “here we show...”, general aim, and specific approach.

Response: We considered the suggestions; accordingly, the introduction has been revised as in the revised manuscript.

L35: Fix reference style of “Jordan et al., 2008”.

Response: Thank you for pointing it out. We fixed the reference style.

L75<: The study on Findelengletscher, by Sold et al. (2015), was mainly in temperate firn, while your study is located in cold firn. Does this difference matter when it comes to the consideration of radar velocity to get the internal reflection horizons? If so, please clarify.

Response: Yes, the temperate firn is characterised by the presence of liquid water, which can accelerate the densification process ([Wakahama, 1975](#)). Studies like [Bradford et al \(2009\)](#) reveal that even a small volume of liquid water content can alter the radar propagation velocity by more than 15%. Furthermore, because meltwater has a different permittivity than firn and ice, which could scatter electromagnetic waves and attenuate the radar signal deeper within the firn ([Reinardy et al., 2019](#)). Thus, the application of the GPR-based CMP method in temperate firn has a significant influence on radar propagation velocity and the depth estimation of the internal reflection horizons (IRHs).

We discussed it in section 5.4 and also in the conclusion of the revised manuscript.

L77-79: From the formulation of these lines, I would expect that your study will discuss the results compared to Huss (2013), and would not expect a comparison to Sold et al. (2015). I suggest rewriting the motivation part and better formulating the aim of your study.

Response: Agreed to your suggestion. We reformulated the motivation part and rewrote the introduction as in the revised manuscript.

Study area and data acquisition

L94: Concerning the largest glaciers, you might cite Windnagel et al. (2023).

Response: We appreciate your suggestion. We have considered it in the updated manuscript.

L102: Instead of “validate”, I would write “as available from GLAMOS (2024) and WGMS (2024).”

Response: Agreed, we changed it as in the revised manuscript.

L116: Under the title “Glaciological investigations”, I would also expect to find information about the point mass-balance measurements from GLAMOS that you used for validation/comparison to your results.

Response: Agreed, we used winter, summer and annual accumulation stake point measurements to compare our GPR-based CMP results as explained in the “Results” section of the submitted manuscript. We added more information within section 2.2, “Glaciological investigation”, in the revised manuscript.

Methods

L158: Remove space in (Vfirn).

Response: Agreed, we changed it in the revised manuscript.

L166: You have only one sub-section (3.3.1). Consider merging with Section 3.3. This would also reduce redundancies in the current title.

Response: Appreciate your suggestions. We adapted the changes in the revised manuscript.

L177<: Please provide more details on how the “tuning” was done.

Response: We provided details of tuning as here “Parameter tuning was done by iteratively choosing the best coefficient that fits the GPR-derived CMP and glaciological observed density-depth profiles.” We added this detail in the “Firn densification modelling” section 3.3 in the revised manuscript.

L192: Would it be helpful to include these parameters in the sensitivity experiment (Section 5.3)? Also: replace “.” by “.”.

Response: We would not think so. We calibrated the model to fit the geophysical and glaciological observations. Table 3 shows the best-fitted parameter coefficients for the LIG and KM models’ density-depth profiles to the observations. We believe that keeping the tuning parameters within section 3.3 is a better option rather than moving to the “Sensitivity...” section 5.3.

L201: Check and adjust your term (e.g., “seasonal melt factor”, “snowmelt rate value”, “melt factor”) to be consistent within your paper and, ideally, also with related key literature (e.g., Cogley et al., 2011; Hock, 2003). Also, be careful when comparing point with glacier-wide, or seasonal to annual degree-day factors.

Response: Within the updated manuscript, we used the term Degree Day Factor for snow (DDF snow) as mentioned in [Hock \(2003\)](#). We also carefully considered the use of DDF snow in point and glacier-wide applications.

L208<: The scaling of precipitation data includes major assumptions and comes with significant uncertainties. It would be good to add a corresponding discussion, and you might consider including it in your sensitivity experiment.

Response: Agreed to your suggestions. We are working on the sensitivity of precipitation data and its influence on firn densification. We added the related discussion in Section 5.3, “Sensitivity analysis”. Further, we discussed the results in the same section of the revised manuscript.

L224-225: Do you find a similar “trend” and/or “variability”? Please clarify.

Response: Yes, “variability” is the better choice of word. We changed the term “trend” to “variability” within the revised manuscript.

L225: Figure 6 is a “comparison” rather than a “validation”. Please clarify.

Response: Agreed, we changed the term “validation” to “comparison” within the revised manuscript.

L228: Avoid acronyms in the title.

Response: Agreed to the suggestion. We have changed the acronyms in all titles within the revised manuscript.

L229<: Maybe something for the discussion concerning this Method section: How well are annual firn layers (or end of summer horizons) detectable at high-altitude sites? Possibility of complete melting of firn layers in extreme years 2022/23/24? Disturbance & mass transfer through strong melt events?

Response: Agreed, we discussed the suggested part within the “Discussion” section as below.

“At a high altitude site, direct observation, like isotope analysis (Fig. 9), helps to identify the last summer horizon, which can be supported by comparing GPR-based CMP estimated SWE with stake-derived SWE as explained in section 4.4 of the submitted manuscript”. We also added more information regarding the extreme melts and the resulting mass transfer in the revised manuscript.

L262: Consider adding the R²-value (0.88) to Figure 7.

Response: Agreed, we updated Figure 7 with the R-squared values.

Results

L285: “... to a depth of 5 m”. From Figure 9, I would rather say at 4m at Ewigschneefeld (Site 1); for Mönchsloch (Site 2), the fluctuations continue to the end around 5.5 m. Please clarify.

Response: Thank you for noticing a mistake. We corrected the mistake as pointed out and implemented it in the revised manuscript.

L292: “precipitation rate” or “accumulation rate”? Please clarify.

Response: It's an “accumulation rate”.

L295: What is the “effect of elevation difference”? Spatial variability or precipitation-elevation gradient? What about the temperature-elevation gradient?

Response: In this case, we believe that the elevation difference is related to the temperature difference or a lapse rate. We changed the related text in the revised manuscript.

L300<: The difference between the SWE (420 mm vs 740 mm) is mainly due to the different depths of the snow pits, right? It might be better to compare the values at the common maximum depth. Please clarify.

Response: We appreciate your effort in spotting the mistake. Yes, here we compared the difference between the SWE for different depths, which should not be the case. The mistake was rectified, and we updated the text as in the revised manuscript.

L307-309: What influences the maximum depth of the reflection pattern? Mainly density? Please clarify.

Response: We mentioned the reason for the lower penetration depth in the following line within the same paragraph as “ This lower penetration depth can be attributed to the high

melt rate at this location compared to the other two CMP locations". We also discussed the variation in the radar penetration depth in section 5.1, lines 418-423 (submitted manuscript, <https://doi.org/10.5194/egusphere-2025-615>).

L315<: Could the change in density in Fig. 11 also originate from the percolating and refreezing meltwater from the intense summer melt of recent years?

Response: After RC1 comments, we updated most of the results (as in [here](#)), including plots, and the CFM modelling part. We speculate changes in density at certain depths (approximately at 6, 8, 9, 12 and 16 m) due to intense summer melt. The depths of the identified annual layers support the speculation. We added a figure in the appendix explaining the same.

In the revised manuscript, we considered the required improvement in the "Discussion" section.

L319: Instead of "testing and calibration", I would have expected a "calibration and validation" of the firn compaction model. Do you use these terms instead because you do not trust your glaciological and geophysical observations enough? Or is it related to missing uncertainties? Please clarify.

Response: We agreed that the section title should be changed. The choice of the title had nothing to do with our measurements. The revised manuscript has a changed title for this section as " Calibration of firn compaction models".

L340: Does the difference in density between model and glaciological observations indicate a process (e.g., refreezing) not included in the model? Please clarify.

Response: The chosen semi-empirical LIG and KM models do consider the melting and refreezing processes to simulate the firn densification in polar climatic conditions. However, KM and LIG models rely on a constant surface density assumption (in our case, 300 kg m^{-3}) to reflect the density variation in shallow depth, as in glaciological observations. Surface snow density highly depends on temperature, precipitation, and wind speed, making it hard to model the shallow snow density to reflect observations ([Ligtenberg et al., 2011](#)). We considered your suggestions regarding the density difference between the model and glaciological observation and improved the text with further discussion in the same section of the revised manuscript.

L347: Avoid acronyms in titles. Write "accumulation" with a lowercase "a".

Response: Agreed, the updated manuscript has been corrected for similar mistakes.

L350: Is "lowest winter precipitation" correct, or should it be "lowest winter accumulation"?

Response: We appreciate the mistake pointed out here regarding the terminology. It should be "lowest winter accumulation" instead of "lowest winter precipitation". We considered this change in the revised manuscript.

L355: The average of a summer balance that varies between accumulation and ablation might be misleading. Consider rewriting with a focus on the change from summer-accumulation to summer ablation regime.

Response: We agreed to rewrite the sentence as suggested. The revised manuscript has been corrected for the same.

L370: Based on what evidence/observation/indication did you expect some remaining firn at Mönchsjoch and Ewigschneefeld? Please clarify.

Response: We discussed this point in the section “Accumulation history and spatial firn distribution” line 487< as “At the Jungfraufirn location, the 2022 firn layer does not exist (Fig. 13), but this might not be true for Mönchsjoch and Ewigschneefeld. This argument is supported by the radargram obtained from the GPR profile at Ewigschneefeld, demonstrating the strong IRH (at 140 ns in Fig. 2) that persists with a certain thickness in the upper part of the GPR transect and the thickness reduces as the profile reaches lower elevation”.

L386<: Can you provide more details on how you estimated SWE within each layer? Simply by the geometry of layers and density within layers? Does this estimate consider meltwater penetration through layers? Please clarify.

Response: Yes, the SWE estimation is straightforward, as you stated, by multiplying the geometry and density of the layers. We know the density from the CMP measurements and the estimated radar electromagnetic wave velocity within each identified layer. The SWE can be estimated using Eq.6 as in the manuscript. We believe that the estimated SWE within each layer considers the percolated meltwater that has refrozen at a particular depth of identified annual firn layers (Fig. 16 in submitted manuscript). However, we can not rule out the percolation of meltwater, which can not be quantifiable with this approach. We believe that percolated meltwater, which drains out of the glacier system, might not be accounted for within the identified layers.

We discussed these points in the “Discussion” section 5.4 of the revised manuscript.

L388: “suggesting higher precipitation and lower melt”: AND/OR?

Response: The thicker firn layer at the upper part of the GPR profile (higher elevation) could be due to both “higher precipitation AND lower melt”. Similar to the variation in the firn thickness at lower elevation due to “possible lower precipitation AND higher melt”.

L399: “...leading to lower accumulation as elevation decreases.” I think lower annual net accumulation could also result from similar winter accumulation, but a less positive or negative summer balance with lower elevation. Consider and maybe rephrase.

Response: We agree that the rephrasing would help in clarifying the statement. We changed the sentence in the updated manuscript.

L390: I think the uppermost layer (above the last summer horizon) is a special case since you only have winter accumulation but no summer balance (yet).

Response: Yes, it is indeed a special case, as our measurements cover up to the winter of 2024. We added this information in the revised manuscript.

Discussion

L401: What are you referring to with “extreme weather” – “extreme” in a statistical sense, or just “extreme” with respect to “normal” lowland weather conditions? Please clarify.

Response: Extreme in the sense of weather conditions like “intense precipitation and wind gusts”. We changed the terminology within the revised manuscript to improve the clarity.

L431<: This section about firn densification modelling remains a bit fuzzy. Can you be clearer about lessons learned and conclusions for future use?

Response: We try to improve section 5.2 “Firn densification modelling”, which includes the refined conclusion and future use as in the revised manuscript.

L447: From Fig. 12, I would not say that “the offset at shallower depths disappeared”. Instead, I would say the offset could be reduced. Check and consider reformulation.

Response: Agreed, we reformulated as in the revised manuscript.

L457: “...the tested models consider ..., liquid water percolation and refreezing...” What could be learned from the model on the effect of the extreme melt events of the past few years? Please clarify.

Response: Model results illustrate a sharp increase in density at depths such as 7, 9, 13 and 17 m (Fig. 12 c and d) after model calibration. We speculate that the sharp changes in density can be attributed to the extreme summer melts in 2023, 2022, 2021 and 2020 or change in density resulting from intense summer melts. However, LIG and KM models show some kinks at 6-10 m depth, but these fluctuations are not significant at deeper depths. We improved the CFM result interpretation in the revised manuscript after correcting for the RC1 comments, for which the CFM calibration of parameter coefficients is also changed (Table 3 in the revised manuscript).

L456<: The section on “Sensitivity of field results” seems valuable. However, I found your conclusions difficult to understand. Are there any other relevant parameters that should/could be checked? Or do you consider velocity picking to be the main uncertainty?

Response: We believe that the main uncertainty lies with the picking of Vrms velocities from the CMP-based semblance analysis, which is illustrated by [Booth et al. \(2011\)](#).

We also rewrote this section within the revised manuscript after implementing the comments from RC1.

L465: Do you consider a velocity picking uncertainty of 0.005 m ns⁻¹ to be a typical or maximum uncertainty? How does Fig. 14 show the effect of an uncertainty of 1 ns? Please clarify.

Response: The choice of Velocity picking uncertainty of 0.005 m ns⁻¹ is considered suitable because this uncertainty range falls within the 50% semblance contour ([Booth et al. 2011](#)). Uncertainty with 1 ns picking is very small when compared with the picking in Vrms sensitivity. So, the SWE and Density sensitivity bars or error bars (Fig. 14) already include the effect of 1 ns. We improved the text in the revised manuscript.

L473-474: “Therefore, it is practical to use the mean TWT to estimate SWE along the GPR transect for the identification of the annual firn layer.” Is this a justification for your approach or a conclusion for other studies? What is the corresponding state-of-the-art? Please clarify.

Response: We propose the use of mean TWT to estimate SWE within each identified annual layer. It is a practical approach, as observed IRHs are significantly undulated, ranging from 311 to 174 ns. So, the mean TWT consideration for the further analysis is a better option. We updated the related text in the revised manuscript to improve the clarity.

L475<: The section on “Accumulation history and spatial firn distribution” is very interesting, but it is difficult to understand your main findings. As such, it would be interesting to be more specific about the advantage of your approach compared to that of Sold et al. (2015). Can you simulate the difference?

Response: The advantage of our method is to track the accumulation history without deep firn cores (depth > 20 m), and firn densification models to derive permittivity for IRH depth estimation. This can be possible using the GPR-based CMP method, which provides accurate IRH depth estimations (depth >30 m). The combination of CMP and long GPR transect helps to track the spatial accumulation and firn density distribution.

We improved the section “Accumulation history and spatial firn distribution” in the revised manuscript, implementing suggestions to explain the differences between our study and Sold et al. (2015) in detail.

L475<: The comparison to the earlier study (on the same GPR tracks) by Bannwart et al. (2024) is limited to one qualitative statement. It would be great to see a quantitative comparison of the results from both studies if feasible. Is it possible to link the GPR profile from the Ewigschneefeld? Do we see similarities or differences in the density profiles from the firn cores?

Response: We thought about analysing the data from [Bannwart et al. \(2024\)](#); however, it does not suit the current aim of the manuscript. A new manuscript is under preparation focusing on the firn density evolution using temporal GPR data from [Bannwart et al. \(2024\)](#), [Patil et al. \(2025, TC Preprint\)](#) and our recently acquired winter 2025 repeat measurements. However, we compared the glaciological observations and discussed the results in the new subsection titled “Comparing recent studies” in the revised manuscript, which includes the [Sold et al. \(2015\)](#) and [Bannwart et al. \(2024\)](#) results comparison with our study.

L478: “...it should be noted that not all IRHs necessarily represent annual firn layers.” What else could they represent? Please clarify.

Response: According to [Sold et al. \(2015\)](#), a large number of melt-refreezing events can generate high-density or ice layers. Therefore, IRHs can be identified as refrozen layers or ice lenses if there are no continuous GPR transects. Thus, we suggest the requirement of the chronological and iterative method for identifying IRHs as annual layers from GPR CMP-based estimated SWE compared with stake-derived SWE. We discussed further in section 5.4 (submitted manuscript).

L484: “We assessed the role of extreme events...”. How is your modelling affected by these extreme events? Do you expect some misinterpretation? Or do the models open the possibility for a detection & attribution of such events? Please clarify.

Response: Here, we can only speculate on the model interpretation concerning the Alpine climatic conditions, where the extreme events are quite common. Our current work is primarily focused on understanding how well firn compaction models represent field results. The detailed understanding of firn physics in the model results is reserved for our upcoming manuscript. However, we explained the interpretation of model results in Alpine conditions in the revised manuscript.

L491: “providing evidence for the survival of the 2022 firn layer.” What evidence do you have? Please clarify.

Response: The presence of strong IRH (at 140 ns in Fig. 2) on the radargram obtained from the GPR profile at Ewigschneefeld, demonstrating the persistence of some firn layer of approximate accumulation of 500 mm w.e (Fig. 14) in the upper part of the GPR transect and the thickness reduces as the profile reaches lower elevation.”

L510: “The lack of CMP data at the lower part...”. What was the reason of not having CMP data for Site 1?

Response: Weather conditions and time constraints are reasons for not getting the CMP data at the lower part of the GPR transect.

Conclusions

L540<: The Conclusions would benefit from rewriting, providing more structure, and highlighting key findings better: what was done; what are the key results with respect to the accumulation history of Grosser Aletschgletscher, and from a methodological point of view; (what did we learn from comparison to the earlier survey by Bannwart et al., 2024); what are recommendations for future work.

Response: Agreed, we restructured the conclusion part as suggested. You can find the same in the revised manuscript

Appendix

L563, Fig. A1: I do not see a need for an appendix for one single figure and, hence, would rather integrate it into the paper.

Response: We added a few more plots and some details to the appendix which provides additional information along with the main text of the manuscript.

L564<: Data availability: For your own results, I strongly support publishing the dataset in a public repository such as Zenodo (<https://zenodo.org/>) or Pangaea (<https://www.pangaea.de>). For external input data, you can provide corresponding references (e.g., GLAMOS, MeteoSwiss).

Response: Thanks for your suggestions. We are planning to publish our dataset on one of these platforms. We will update regarding the data availability soon after the publication.

L567<: Author contributions: Who was responsible for the Community Firm Model runs and analysis?

Response: Akash Patil ran the Community Firm Models and analysed the model results. However, we forgot to acknowledge Max Steven's assistance in setting up the CFM for us.

Figures and Tables

All: Please improve the structure of the caption (see general comment).

Response: We appreciate your suggestion for improving all captions. The revised manuscript has all improved figures with updated captions.

Table 1: For the readability of the table, I would explain the acronyms (GRP, CMP) in the caption.

Response: We changed the Table caption as suggested.

Fig. 1: Consider adding a note explaining why there is no CMP at Site 1. Regarding the background image, information on the platform, sensor, and date would be more relevant than the format. I suggest complementing Figure 1 with an additional Table summarising the different observations per Site (1, 2, 3, 4), including locations, elevations, and survey dates.

Response: We considered adding comments on "no CMP at site1" within the revised manuscript figure caption. We also added suggested changes in the caption. The revised manuscript now has a table summarising all measurements with respective survey dates and locations.

Fig. 2: Consider adding the temporal interpretation of the internal reflection horizons (red lines). Indicate locations of other measurements (e.g., SP3, CMP3, SP1). It might be helpful to add a comment on the elevation range from left to right of the profile and on prominent features, e.g., the merger of reflection horizons at a Distance of 1500m (melt of layers at lower elevation?) or the interpretation of the reflection horizon at a Time of 125 ns between red lines.

Response: We considered adding comments on elevation change over the radargram profile within the revised manuscript figure caption. We would like to keep the figure as it is now, which helps readers to look at the complete stratigraphy; otherwise, it will be blocked by labels. The depth in the left axis can not be corrected for the estimated interval velocity, which changes the depth of each IRH. It makes unreliable to add the temporal interpretation for this plot. Moreover, we picked IRH using ReflexW software by zooming in on each layer.

Fig. 3: Consider adding the temporal interpretation of the internal reflection horizons (red lines). Add information on the location of this CMP(3?) with a reference to Fig. 1. Consider showing all CMPs in one figure for comparison.

Response: We would like to keep the figure as it is now as an example figure. Because the depth is not corrected for the interval velocity, we can not label the temporal interpretation of the Internal Reflection Horizons identified here.

Fig. 4: Explain all (colored) elements in figure, i.e. red lines (left), blue-green-yellow-red color range (right); xxx (right). Add information on the location of this CMP(3?) with a reference to Fig. 1.

Response: This figure has changed after being corrected for the [RC1 comments](#). The revised manuscript has an updated figure with suggested changes in the figure caption.

Fig. 5: The caption does not seem consistent with the labels in the figures: “summer mass balance” (y-axis) versus “ablation” (caption)? “Degree-day” or “melt” factor? How does the width of the bars correspond to the (calendar or hydrological) years of the x-axis? Interestingly, there seems to be a correlation between summer balance and degree-day factor, maybe due to a feedback mechanism? What is the survey period of the summer balance? Was this consistent for all years?

Response: The revised manuscript has an updated figure caption after considering the consistency in the label and the figure caption. The width of the bars has nothing to do with the years of the x-axis. Bar width has been chosen considering the plot visibility and to adjust the Degree-day factor for snow values on top of each bar. Yes with the increased DDF snow we have increased melting of snow which means more loss of snow in summer (as seen in the plot). Here we could expect the feedback mechanism. The survey period is consistent between 1 May and 31 August.

Fig. 6: Provide information about the two graphs' trend, bias, and correlation.

Response: We agreed to provide the suggested information. The revised manuscript has an updated figure caption.

Fig. 7: Consider adding a note on the density jumps at 3-5m, 12-17m, 23-27m.

Response: We revised this figure after addressing RC1 comments. The updated figure has the suggested note in the caption.

Fig. 8: Consider adding the thickness of the ice lenses as values to the graph (e.g., next to the left y axis). Consider adding a note on the density jump between the end of the red and the start of the brown line.

Response: We noticed many ice lenses close to each other, mainly after 5 m depth. It makes it unreadable if we add thickness values in the plot. However, we provide an alternative figure in the Appendix with ice lens labels. We added a note regarding the density jump in the revised manuscript.

Fig. 9: Consider adding a note on the location of the end-of-summer horizons.

Response: We agreed to your suggestion. We added lines to explain the location of the last summer horizon in the revised manuscript.

Fig. 10: Consider adding the data from Site 1 to this plot, too. Brown and orange horizontal bars are hard to differentiate. Consider adding the thickness of horizontal layers as values to the plot (e.g., right side).

Response: We would like to keep this figure as it is now. We gathered this data between 16-17 May 2024, which has a significant time gap from the Site 1 data set acquired on 29 February 2024. The figure has many ice lenses or stratigraphy, adding a thickness axis makes it unreadable, as most of the ice lenses are close to each other in depth. However, we provided a labeled thickness figure in the appendix corresponding to the specific colour.

Fig. 11: Avoid or explain the use of acronyms in the caption. Consider adding a note on the break in the velocity and density profiles at a depth of about 15 m.

Response: The figure has changed after correcting for [RC1 comments](#). We considered the required suggestions for this figure in the updated manuscript.

Fig. 12: There seems to be an offset between snow pits/core and CMP values at common depths. Consider adding a corresponding note to the caption and/or cover in Discussion.

Response: We discussed the offset in the corresponding discussion section of the revised manuscript.

Fig. 13: Improve readability of the figure by separating bars of summer and annual balance. Add a horizontal line at zero balance. As a source, I would add a reference to GLAMOS (2024), and thank A. Bauder in the Acknowledgements.

Response: We agreed to the suggested changes, and the updated figure in the revised manuscript now has an added reference. Here, all winter and summer seasonal mass balance measurements date changed each year (GLAMOS, 2024). So, the figure has a constant winter, summer and annual survey date of the end of April, July and September, respectively, for better readability.

Fig. 14: Does the x-axis provide calendar or hydrological years? Correct text to “43-72 mm w.e.”. Consider adding a note explaining the lack of markers, e.g. red in 2021/22, red and orange before 2014. Add a horizontal line at zero balance. Consider labeling the years/dates of the estimated reflection horizons in Figs 2,3,4.

Response: The x-axis provides measurement of summer mass balance date (GLAMOS 2024). We updated the figure with suggested changes of adding a note explaining the markers, and also added the line at zero balance. Adding years to Figures 2, 3 and 4 makes them chaotic. We would like to keep them as they are.

Fig. 15: Does the white at the bottom of the graph refer to ice (density >850 kg m⁻³) or the maximum GPR depth? Consider adding year/date labels to the identified firn layers.

Response: White space is because of no layers that can be picked beyond the maximum depth at a particular distance. It does not refer to the ice density. We agreed to add years to each identified firn layer. The figure is updated in the revised manuscript.

Fig. 16: Is “Accumulation” the correct label for the colour legend, or should it be “SWE”?

Response: We thought about it, and we would like to keep it as an accumulation rather than SWE. Because our study aims at tracking spatial accumulation, which is nothing but the product of identified firn layers' thickness and the estimated layer density.

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