Author Response to the RC1

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Manuscript: Annual firn pack variations in the context of the climate settings: Example from the Grosser Aletschgletscher using multi-year radar observations
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Response to Specific Comments

Thank you for your positive feedback and review of our manuscript. We appreciate your thorough engagement with our work, constructive feedback and valuable questions. We agreed to implement most of your comments and have gladly integrated the majority of them into our manuscript. RC1 comments on the model tuning procedure, which is not part of our manuscript, so we refrain from responding in detail to related comments. We see a few repeated comments in specific, critical, major, minor and line-by-line comments. So we selectively responded to Specific comments. We addressed all comments and responded to them in the corresponding comment sections.

Specific comments

01. Scope — Does the paper address relevant scientific questions within the scope of The Cryosphere? *Yes. The manuscript directly addresses firn dynamics, glacier mass balance, and cryosphere–climate interactions on an Alpine glacier — all central topics within the scope of The Cryosphere. No concerns regarding scope.*

02. Novelty — Does the paper present novel concepts, ideas, tools, or data? **Yes, with qualification.** The primary novel contribution is the first multi-year, repeat CMP application for temporal firn density and compaction monitoring on a European Alpine glacier. Previous Alpine GPR studies (e.g., Sold et al., 2015, The Cryosphere, 9, 1075–1087, doi:10.5194/tc-9-1075-2015) used fixed-offset profiles for spatial snow water equivalent mapping but did not track year-to-year density evolution at depth. The integration of stable isotope and chemical impurity records for IRH dating adds methodological rigour beyond prior Alpine studies. The quantification of elevation-dependent annual 2 density change (~130 kg m^{-3} a^{-1} at lower-elevation sites vs. smaller values at higher sites) is a novel empirical result. The CMP

methodology itself is not novel (established in polar glaciology: e.g., Brown et al., 2012, doi:10.1029/2011JF002089; Meehan et al., 2021, doi:10.1017/JOG.2020.91), and the observation that compaction rates decrease with depth is consistent with established firn densification theory (Herron & Langway, 1980). The authors should more clearly delineate, in the Introduction, what is genuinely new relative to prior Alpine work.

03. Conclusions — Are substantial conclusions reached? Partially. The conclusions are broadly consistent with the data presented. However, several claims are not adequately supported:

a. The assertion that results constitute “an essential step toward improving firn-densification models and glacier mass-balance estimates” (Abstract, line 19–20) is not substantiated. The firn model tuning is described without specifying the optimisation procedure, objective function, or validation against independent data. Three CMP locations over two years are insufficient to rigorously constrain or validate community firn models.

b. The conclusion that CMP is “a valuable alternative” to invasive core drilling is reasonable but would be strengthened by a quantitative comparison (RMSE, bias) between CMP-derived and core measured density profiles.

c. The observation of “potential effect of glacier dynamics on firn stratigraphy” (Abstract, line 17) is presented as a conclusion but is not quantitatively supported in the manuscript body.

Recommendation: Moderate the impact claims; provide quantitative support for model-tuning claims; reframe the glacier-dynamics observation as a hypothesis for future investigation.

04. Methods — Are scientific methods and assumptions valid and clearly outlined? Insufficiently. This is the most critical deficiency in the manuscript.

What is adequate: The choice of CMP for velocity-based density profiling is scientifically valid and well-established. The use of the Looyenga mixing formula, cross-checked against Kovacs and CRIM, is appropriate. The combination of geophysical and geochemical methods for IRH dating is methodologically sound.

What is missing or unclear:
• CMP acquisition parameters are not reported: antenna frequency (MHz), offset range, number of traces per gather, trace spacing, recording time window, and

number of stacks. These are essential for assessing data quality and for reproducibility.

• Processing workflow is described only in general terms. Specific parameters for bandpass filtering, gain functions, and the velocity analysis algorithm are not given.

• Semblance panels or velocity spectra — the standard quality-assessment output of CMP analysis — are absent. Their inclusion is necessary to demonstrate the reliability of velocity picks and the absence of cycle-skipping or noise-dominated picks.

• IRH tracking procedure is not described: manual vs. semi-automatic, continuity criteria, treatment of gaps, and how IRHs were matched between CMP sites and between years.

We appreciate your feedback. The detailed process for picking IRHs from Semblance analysis is described in our previous work (Patil et al., 2025). The same is cited in the manuscript, line 170. The CMP analysis quality assessment is discussed in our previous papers and provides details, such as the quality of CMP data with depth in the Appendix figures A1 and A2 (Patil et al 2025). We also provided the same in the supplement. We revisited the manuscript and provided missing descriptions on the CMP acquisition parameters, IRH tracking procedure and processing flow. Further, added more in detail in the supplement.

• Firn core analysis methods are incompletely described: which chemical species were measured by ion chromatography, analytical precision, and how seasonal signals were identified and used to define annual layers.
Thanks a lot for noticing. We added a recommended description within the main text.

• Firn model tuning is mentioned but not described: which parameters were adjusted, by what optimisation procedure, and how the tuned model was validated.
We did not mention any Firn model tuning within the submitted manuscript. The manuscript only focused on field observations.

• **Critical unaddressed assumption:** The manuscript assumes that downward displacement of IRHs between repeat surveys equals firn compaction. In a flowing glacier, this displacement also includes a contribution from vertical strain (thinning due to horizontal divergence and longitudinal extension of ice flow). At Grosser Aletschgletscher, surface velocities in the accumulation area reach ~30–60 m a^{-1} (see: Millan et al., 2022, Nature Geoscience, 15, 111–116, doi:10.1038/s41561-021-00885-z), implying non-negligible vertical strain rates. This confound must be either corrected for or explicitly quantified and discussed.
We responded to these comments within the specific comments compiled below.

05. Results — Are the results sufficient to support the interpretations and conclusions? Partially. The qualitative patterns reported (density change decreasing with depth; compaction rate maximum near the surface; elevation-dependent density evolution) are plausible and internally consistent.
However:
• No uncertainty estimates are provided for any quantitative result. Without error bars, it is impossible to assess whether differences between sites, depths, or years are statistically distinguishable. Velocity uncertainty of ±5% from semblance analysis propagates to density uncertainty of ~8–10 kg m^{-3} via the Looyenga formula — non-trivial relative to the reported annual changes (Medley et al., 2015, doi:10.3189/2015AOG70A203).
We appreciate your detailed suggestions on uncertainties. However, we provided the semblance velocity picking (±0.005 m/ns) uncertainty in density estimations in line 591 within the subsection 6.5 “Conservation of mass and Uncertainties”. The same is explained in detail in our previous work (Patil et al 2025) as suggested by Booth et al. (2011). All plots related to density-depth or density-age profiles are plotted considering the semblance velocity uncertainty of 0.005 m/ns (Figures 7, 8, and 9) along with the uncertainty or error bars.

• The CMP-vs-core density comparison is presented qualitatively (“agreement”). No RMSE, mean bias, or correlation coefficient is reported, and no side-by-side figure with error bars is provided.
We appreciate your suggestion. We provided the RMSE and Bias information in Figure 7b in the revised manuscript. We also added a standalone figure of the CMP-vs-core derived density profile with RMSE and Bias information in the supplement.

• Compaction rates are reported as “maximum ~0.3 m a^{-1}” without specifying the depth interval, the year of measurement, or the uncertainty.
Thanks a lot for the feedback. We provided depth and year of measurement information on the maximum compaction rate.

• The firn model tuning is claimed to improve model performance, but no quantitative comparison (pre- vs. post-tuning RMSE) is shown.
We did not use any firn models in this study. The manuscript only focused on field observations.

06. Reproducibility — Is the description of experiments and calculations sufficiently complete to allow reproduction by fellow scientists? No — this is a critical deficiency. As detailed under criterion 04, the CMP acquisition parameters, processing workflow, IRH tracking procedure, and firn model tuning procedure are insufficiently described for independent reproduction. The manuscript should provide either (a) complete parameter tables in the main text, or (b) a comprehensive

supplementary methods section with all parameters and intermediate outputs (semblance panels, velocity-depth functions, IRH depth tables).
We appreciate your feedback. We agreed and provided detailed CMP acquisition parameters and processing workflow within the main text in the form of tables. The IRH tracking procedure is added in the revised manuscript. We provided the velocity-depth, including Vrms and interval velocity profiles, within the supplement, along with the IRH and depth information. The submitted manuscript focuses on field observations and does not provide any information on firn model tuning.

07. Credit and originality — Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Adequately, with minor gaps. The reference list is broad and generally appropriate. The authors cite key relevant works in Alpine firn studies (Sold et al., 2015), polar CMP applications (Brown et al., 2012), and firn densification modelling (Herron & Langway, 1980; Kuipers Munneke et al., 2015). **However:**

• The Introduction does not explicitly state what is novel in this study compared to Sold et al. (2015) and other Alpine GPR work. A direct comparison sentence would help readers assess the incremental advance.
• Stevens et al. (2024, Journal of Glaciology, doi:10.1017/jog.2024.24) — cited in the reference list — reports direct multi-year firn density measurements at Wolverine Glacier, Alaska, and represents a close methodological analogue that should be discussed in the context of the current study's novelty.

• The discussion does not sufficiently compare the reported compaction rates (~0.3 m a^{-1}) with values from polar studies (Medley et al., 2015: ~0.33 m a^{-1} in Antarctica; Kruetzmann et al., 2011, doi:10.5194/TC-5-391-2011: ~0.05–0.25 m a^{-1} near Ross Island) to contextualise whether Alpine rates are anomalously high, low, or consistent with global patterns.
We responded to similar comments within the critical, major, and minor comments compiled below.

08. Title — Does the title clearly reflect the contents of the paper? Adequately, with a suggested refinement. The current title, "Annual firn pack variations in the context of the climate settings: Example from the Grosser Aletschgletscher using multi-year radar observations," is descriptive. **However:**

• "firn pack variations" is somewhat vague; "temporal firn density and compaction" would be more precise.

• "in the context of the climate settings" is awkward phrasing; "in response to Alpine climate forcing" or "under Alpine climate conditions" would be cleaner.

• The colon structure is acceptable but the subtitle “Example from…” slightly undersells the contribution.

Suggested alternative: “Temporal evolution of firn density and compaction at Grosser Aletschgletscher: first multi-year CMP radar and firn-core observations for a European Alpine glacier.”
We appreciate the suggested changes in the title. We changed the title to improve clarity and the scientific reach of our study.

09. Abstract — Does the abstract provide a concise and complete summary? Partially. The abstract adequately covers the objectives, methods, and main results. **However:**

• **Line 14: typographical error —** “CMP measurements are & valuable alternative” (should be “a valuable alternative”).
Thanks a lot for noticing an error. We corrected for this.

• **Line 11: ambiguous phrasing —** “changes in firn density over a year decrease with depth and age” (does “over a year” modify “changes” or “decrease”?). Suggested revision: “annual changes in firn density decrease with depth and age.”
Thanks for noticing possible ambiguity in phrasing. We accepted the suggestions and corrected them accordingly.

• The abstract does not quantify the uncertainty on the key result (~130 kg m^{-3} a^{-1}), which is important for readers to assess the robustness of the finding.
Thank you for the feedback. We added this information in the abstract.

• The claim that results are “an essential step toward improving firn-densification models” (line 19) overstates the contribution relative to the study scope (see criterion 03). This should be moderated.
We appreciate your suggestion. We made the required changes.

• The abstract could be shortened by ~15% without loss of information (currently ~230 words; target ≤200 words for this journal).
We appreciate your suggestions. We consider making the needful changes.

10. Overall presentation — Is the overall presentation well structured and clear? Adequately structured, with important gaps. The manuscript follows a logical IMRaD structure (Introduction, Methods, Results, Discussion, Conclusions) appropriate for The Cryosphere. The writing is generally clear. **However:**

• **Essential figures are absent:** (a) semblance panels or velocity spectra demonstrating CMP data quality; (b) a side-by-side figure correlating GPR radargrams with core stratigraphy (density, $\delta^{18}O$, chemical impurities) with IRHs annotated; (c) a quantitative CMP-vs-core density comparison figure with error bars; (d) a figure showing compaction rate vs. depth with uncertainty bounds and comparison to model predictions or polar observations.
We respond to these comments within the specific comments compiled below in detail. Our current study does not focus on any firn model; therefore, we can not provide any figures related to model comparison with polar observations.

• A summary table of key results (density change rate, compaction rate, accumulation rate) by site and year, with uncertainties, would substantially improve clarity.
We respond to these comments within the comments compiled below.

• A conceptual schematic of the study area, CMP geometry, and firn layer structure would aid non-specialist readers.
We appreciate your suggestions. However, we refrain from providing a conceptual schematic of the study area as we already have a real-world aerial image in Figure 1. We added other basic information on CMP geometry within the supplement.

• **The Results section lacks a clear narrative hierarchy:** key findings are not distinguished from secondary observations.
Thanks a lot for the suggestion. We revisited the manuscript and made changes wherever necessary.

11. Language — Is the language fluent and precise? Generally yes, with minor issues. The English is fluent and technical terminology is used correctly. **Specific issues:**
• "Or" capitalised mid-sentence (Introduction, line 25): "survived at least one summer Or melt season."
We appreciate such detailed suggestions. We corrected the grammatical mistake.

• **"& valuable alternative" (Abstract, line 14):** typographical error. • Inconsistent hyphenation: "firn-core" vs. "firn core" used interchangeably throughout.
Thanks a lot for noticing the typographical errors. We made the required changes to be consistent with the hyphenation in the manuscript.

• Several long sentences in the abstract and methods could be split for clarity.
We appreciate your suggestions. We made changes in splitting long sentences wherever necessary.

• The phrase “functionally related to temporal changes” (Abstract, line 6) is vague; consider “controlled by” or “directly linked to.”
Thanks a lot for the detailed commenting. We changed the phrasing as suggested.

12. Mathematical formulae, symbols, abbreviations, and units — Are these correctly defined and used? Mostly, with gaps. The Looyenga mixing formula and its variables are cited but not written out explicitly in the manuscript. Given that this formula is central to converting CMP velocities to density, it should be stated explicitly with all variables defined. The abbreviations CMP, GPR, IRH, and CRIM are defined on first use. Units are generally correct, but axis labels on several figures are missing units — this must be corrected systematically. The decimal separator usage appears consistent (period), in line with journal style.
We respond to these comments within the specific comments compiled below.

13. Clarification, reduction, or elimination of parts — Should any parts be clarified, reduced, combined, or eliminated? **Several suggestions:**

• **Clarify:** The firn model tuning section should either be expanded with full methodological detail or removed if it cannot be adequately described and validated within the scope of this paper.

• **Clarify:** The section on glacier dynamics effects on stratigraphy (comparison of 2024 and 2025 GPR profiles) should be clearly labelled as preliminary/observational, with a quantitative estimate of the expected strain effect or an explicit statement that this is a hypothesis for future work.
We appreciate the much-needed suggestion. Yes, this interpretation is preliminary; we need additional observations to support our interpretation. We added the suggested statement explicitly.

• **Reduce:** The introduction contains some general background on firn processes that could be condensed, as it is well known to the target readership of The Cryosphere.
Thanks for the suggestions. We agreed and reduced or condensed some parts of the introduction related to firn processes.

• **Add:** A dedicated Limitations subsection in the Discussion would strengthen the manuscript by explicitly acknowledging: (i) single-site scope; (ii) two-year temporal baseline; (iii) three CMP locations; (iv) absence of ice velocity data; (v) lack of independent compaction validation.
We appreciate your suggestion of adding a dedicated limitation section in the discussion. We agreed and added suggested information within subsection 6.5.

14. References — Are the number and quality of references appropriate? **Largely yes, with minor issues.** The reference list (~90 entries) is appropriate in scope and covers key literature in firn physics, GPR methodology, Alpine glaciology, and firn modelling. **However:**

• Williamson et al. (2020) appears twice with apparently identical DOI (lines 915–919 of the manuscript). One entry should be removed, or the two entries should be differentiated if they are genuinely distinct publications.

• **DOI formatting errors:** Multiple DOIs use "https:lldoi.org" (capital "I" instead of lowercase "l"), which will generate broken links. All DOIs must be standardised to "https://doi.org/…".
We responded to these comments in the line-by-line comment section compiled below.

• **Missing comparison reference:** Stevens et al. (2024, Journal of Glaciology, doi:10.1017/jog.2024.24) is cited in the reference list but not discussed in the text despite being a highly relevant analogue study (direct multi-year firn density measurements at an Alpine-type glacier).
We appreciate your suggestions. Yes, we mentioned Stevens et al (2024) study on direct measurements to derive firn density measurements. We consider comparing our study with Stevens et al (2024) results and discuss in section 6.3.

• **Suggested addition:** Vandecrux et al. (2020, The Cryosphere, 14, 3785–3810, doi:10.5194/tc-14 3785-2020) — the RetMIP firn model intercomparison — should be cited when discussing firn model tuning, as it provides the community benchmark for model evaluation.
Thanks for the suggestion. However, our current study does not focus on any firn model tuning. Therefore, we do not discuss Vandecrux et al. (2020) in the submitted manuscript.

15. Supplementary material — Is the amount and quality of supplementary material appropriate? Supplementary material is not mentioned in the manuscript. Given the extent of methodological detail that is currently missing from the main text (acquisition parameters, processing workflow, intermediate outputs), the authors may consider preparing a comprehensive supplementary methods section to include such items as:

• CMP acquisition parameter table (per site and campaign).
• Processing workflow diagram with parameter values.
• Representative semblance panels for each CMP site and survey year.
• Velocity-depth functions derived from semblance analysis, with uncertainty bounds.
• IRH depth tables for each site and year.
• Firn core data tables (density, $\delta^{18}O$, chemical impurity profiles).

• Firn model tuning procedure and parameter values.

We appreciate the suggestion to prepare a comprehensive supplementary material, including the above-mentioned points in detail. We agreed and provided the supplementary material consisting of CMP acquisition parameters, processing workflow, representative semblance panel for each test site, velocity depth profiles with Vrms and interval velocity with uncertainty in semblance picking of ±0.005 m/ns, and IRH-depth table for all three CMP test sites. We provide firn core data, including all CMP and GPR profile data, once the manuscript is accepted for publication by creating a DOI for the dataset. Our current study does not discuss or provide any results related to the firn model tuning procedure.

Placing the raw data in a publicly accessible repository, accompanied by a data availability statement—a practice recommended by The Cryosphere, to the best of my knowledge—can also be carried out.
Thanks for the suggestions. We already provided the statement within the data availability section in the manuscript, lines 667-669.

Here is an ordered summary of specific comments compiled from above

First, the critical ones:

1. Uncertainty quantification: Provide full uncertainty propagation for all key results (CMP velocity → density → compaction rate → accumulation). Present all results with error bars or ± values. State the dominant uncertainty sources explicitly.
We appreciate your detailed suggestions on quantifying uncertainties. However, we provided the semblance velocity picking (±0.005 m/ns) uncertainty in density estimations in line 591 within the subsection 6.5 “Conservation of mass and Uncertainties” and also mentioned the error sources explicitly. The same is explained in detail in our previous work (Patil et al 2025) as suggested by Booth et al. (2011). All the plots related to density-depth or density-age profiles are plotted with error bars considering the semblance velocity uncertainty of ±0.005 m/ns (Figures 7, 8, and 9), except for the compaction rate vs age figure 10 in the submitted manuscript. We revisited all figures and added missing information as suggested.

2. Methodological completeness and reproducibility: Add a comprehensive methods description or supplementary methods section including: CMP acquisition parameters, processing workflow with parameter values, semblance panels, IRH tracking procedure, firn core analysis details, and firn model tuning procedure.
We appreciate the suggestion of preparing a comprehensive supplementary material, including the above-mentioned points in detail. We agreed and provided the supplementary material consisting of CMP acquisition parameters, processing workflow, representative semblance panel for each test site, velocity depth profiles

with Vrms and interval velocity with uncertainty in semblance picking of 0.005 m/ns. and IRH-depth table for all three CMP test sites. We will provide firn core data, including all CMP and GPR profile data, once the manuscript is accepted for publication by creating a DOI for the dataset. Our current study does not discuss or provide any results related to the firn model tuning procedure; therefore, we refrain from describing the same in the manuscript.

3. Strain vs. compaction separation: Either (a) obtain and present ice velocity data to estimate and correct for vertical strain, or (b) include a quantitative estimate of the strain contribution to IRH displacement and discuss its implications for the reported "compaction rates." A worked example: "Assuming a vertical strain rate of $\dot{\varepsilon} \approx X\ a^{-1}$ (consistent with surface velocities of ~Y m a^{-1} and a depth of Z m), the strain contribution to annual IRH displacement is estimated at ~W m a^{-1}, compared to the observed total displacement of V m a^{-1}."
Thanks for bringing the importance of strain rate on compaction rate. We appreciate your suggestions. We looked at S. Leinss and P. Bernhard (2021) velocity map, which suggests a negligible velocity gradient at our measurement locations. For this reason, we did not provide the strain rate impact on compaction rate in our submitted manuscript. However, we agreed to estimate the vertical strain rate using Millan's 2022 dataset. We also provide a quantitative estimate of the strain contribution to IRH displacement when the estimated values are significant; otherwise, we provide the details in the supplement.

4. IRH-core correlation figure: Provide a figure showing side-by-side GPR radargrams and core profiles (density, $\delta^{18}O$, SO_4^{2-}) with IRHs explicitly annotated and matched to core annual layers. Quantify core dating uncertainty.
We appreciate your suggestions. We agreed and provided a figure showing the GPR radargram and core profiles similar to Figure 5, with an annotated GPR radargram in the supplement. We also added information on quantifying the core dating uncertainty.

5. Quantitative CMP-vs-core comparison: Report RMSE, mean bias, and correlation coefficient for the CMP-vs-core density comparison. Provide a side-by-side profile figure with error bars.
Thanks for the suggestion. We added RMSE and mean bias information in Figure 7b and provided the standalone figure in the supplement.

<u>Then some major comments:</u>

6. Moderate conclusions: Replace "essential step toward improving firn-densification models" with language reflecting the case-study scope: "These observations provide site-specific constraints that can inform future calibration of firn-densification models for Alpine accumulation areas." Reframe the glacier-dynamics observation as a hypothesis.

We appreciate detailed feedback. We agreed and made changes in phrasing and reframed the suggested conclusion parts.

7. Related with meteorological context: Present air temperature and precipitation data for the study period (from Jungfraujoch station or ERA5 reanalysis) and compare to the 1991–2020 climatological mean.
Thanks for the suggestions. We already provided the present and past climatological air temperature in lines 113-114 and 526-528 from the nearby weather station. Jungfraujoch research station does not record precipitation data; we provided the annual and seasonal accumulation as in Figure A1. We also added the comparison of mean annaul air temperature and accumulation over a suggested climatological period with our measurement period.

8. Dedicated Limitations subsection: Add an explicit Limitations section acknowledging: single-site scope, two-year temporal baseline, three CMP locations, absence of ice velocity data, and lack of independent compaction validation.
We appreciate your suggestion. We already have an uncertainty subsection; we added suggested information within the same section.

9. Missing figures: Add semblance panels, IRH-core correlation figure, compaction-rate-vs-depth figure with uncertainty and model comparison, and a summary results table.
We appreciate your feedback. We agreed to provide semblance panels for the remaining CMP datasets, similar to Figure 2 in the supplement. We also provide a compaction rate vs depth figure with uncertainty in the supplement. We do not have any model results to compare in this study. The summary results table is also agreed to be added to the supplement.

10. Data availability statement: Add a data availability statement and deposit raw data in a public repository (PANGAEA or equivalent). Or not…
Thanks for the suggestions. We already provided the statement within the data availability section in the manuscript, lines 667-669.

Finally, some minor recommendations:

11. Title revision: Consider: “Temporal evolution of firn density and compaction at Grosser Aletschgletscher: first multi-year CMP radar and firn-core observations for a European Alpine glacier.”
Thanks for suggesting changes in the title to improve the scientific readability of this research and for a cleaner and more precise title. We agreed and made a change in the title as suggested.

12. Abstract corrections: Fix typographical error (“& → a”); resolve ambiguous phrasing (line 11); reduce length to ≤200 words; moderate impact claim. 13.

Language corrections: Fix capitalised “Or” (Introduction, line 25); standardise hyphenation (“firn core” throughout); split long sentences.
We appreciate detailed comments on typographical and language corrections. We agreed and corrected the suggested changes to the revised manuscript.

14. Mathematical notation: Write out the Looyenga formula explicitly; add missing axis units to all figures.
We appreciate your suggestions. However, the Looyenga formula is well-known within the community, and explicit mention of the formula in this manuscript does not add valuable information to existing knowledge. Most of the figures presented have axis units in the submitted manuscript, except for Figure 11 (depth-age). We reviewed all figures and made the required changes.

15. Reference corrections: Remove duplicate Williamson et al. entry; fix all DOI formatting errors; add in-text discussion of Stevens et al. (2024) and Vandecrux et al. (2020).
Thanks for the suggestion. We removed duplicate citations and fixed the DOI formatting wherever necessary. We also agreed to discuss Stevens et al (2024) as suggested in line-by-line remarks. However, our study only focuses on observation-based firn densification and compaction; therefore, discussing Vandecrux et al (2020) is not suitable for the scope of this manuscript.

<u>And lastly —typos, tiny comments, and specific line-by-line remarks</u>

Abstract line 11: “changes in firn density over a year decrease with depth and age” — ambiguous scope of “over a year”
Agreed and changed the phrasing as suggested.

Abstract line 14: “CMP measurements are & valuable alternative” — typographical error : “CMP measurements are a valuable alternative”
Thanks for noticing. We corrected a typographical error.

Abstract line 19: “an essential step toward improving firn-densification models” —overstated: “a contribution toward future calibration of firn densification models”
We appreciate your suggestions. We made changes accordingly.

Introduction line 25: “survived at least one summer Or melt season” — erroneous capital: “survived at least one summer or melt season”
Corrected
Introduction general: Novel contribution vs. Sold et al. (2015) is not explicitly stated. Add one sentence: “Unlike Sold et al. (2015), who used fixed-offset GPR for spatial SWE mapping, the present study employs repeat CMP surveys to track year-to year changes in firn density and compaction at depth.”

We appreciate your feedback and suggested changes. We added the suggested correction and made the corresponding changes in the manuscript.

Methods: CMP antenna frequency not stated. Add: “CMP surveys were conducted using [X] MHz antennae with an offset range of [Y–Z] m and a trace spacing of [W] m.”
Thanks for the suggestions. However, we already added this information in the submitted manuscript, lines 147-148. We consider adding details of the CMP acquisition parameters in the table format.

Methods: Looyenga formula not written out: State formula explicitly: ε^(1/3) = φ_ice · ε_ice^(1/3) + (1–φ_ice) · ε_air^(1/3), with all variables defined.
We appreciate your suggestions. However, the Looyenga formula is well-known within the community, and explicit mention of the formula in the main text of this manuscript does not add valuable information to the existing knowledge. We provided this information within the supplement.

Methods: IRH tracking procedure not described. Add: “IRHs were tracked [manually/semi automatically] using [software name], with a continuity threshold of [X] m. Gaps of more than [Y] traces were [interpolated/excluded].”
Thank you for this suggestion. We added this information to the revised manuscript.

Results: “maximum ~0.3 m a^{-1}” — depth interval and year not specified. Add: “~0.3 m a^{-1} at [X–Y] m depth during [year]”
We appreciate your suggestion to add depth and year. We agreed to add this information to the revised manuscript.

Results: CMP-vs-core comparison presented qualitatively only. Add: RMSE and mean bias; provide quantitative figure.
Thank you for pointing it out. We added RMSE and mean bias information in Figure 7b and provided the standalone figure in the supplement.

Discussion: “potential effect of glacier dynamics” — unquantified claim. Either quantify or reframe as: “We hypothesise that glacier dynamics may influence firn stratigraphy; quantification requires future ice velocity measurements.”
Thanks a lot for the feedback. Agreed and reframed the sentence as suggested.

Discussion: No comparison of compaction rates with polar analogues. Add comparison: Medley et al. (2015) ~0.33 m a^{-1} in Antarctica; Kruetzmann et al. (2011) ~0.05–0.25 m a^{-1} near Ross Island.
We appreciate your comments on comparing the polar studies. As the study is focused on Alpine climatic conditions, we thought comparison with polar results is not suitable for the intended focus of the manuscript. Nevertheless, we added the suggested compaction rate values to the revised manuscript.

References: Williamson et al. (2020) duplicated (lines 915–919). Remove duplicates; verify if two distinct papers.
Thanks for noticing our mistake. We rectified accordingly by removing the duplicate citation from the bibliography.

References: Multiple DOIs: “https:lldoi.org” (capital I). Standardise to “https://doi.org/…” throughout.
Agreed and corrected

Figure captions: Axis units missing on several figures. Add units to all axes; conduct systematic check.
Thanks for the suggestions. However, most of the figures have axis units in the submitted manuscript, except for Figure 11 (depth-age (years)). We reviewed all figures and made required changes wherever necessary.

Throughout: Inconsistent hyphenation: “firn-core” vs. “firn core”. Adopt “firn core” (noun phrase) and “firn-core” (attributive adjective) consistently.
We appreciate such detailed feedback and comments. We corrected for the consistent hyphenation, as suggested.

Throughout: Inconsistent citation format (some with DOI, some without). Standardise: include DOI for all references where available.
Thanks a lot for noticing the inconsistent citation format. We could not find DOIs for some of the papers that we cited in the manuscript. We have revisited the manuscript, corrected for the suggested consistency by adding missing DOIs or the last access date.

References:

Patil, A. M., Mayer, C., Seehaus, T., Groos, A. R., and Bauder, A.: Investigating firn structure and density in the accumulation area of the Grosser Aletschgletscher using ground-penetrating radar, The Cryosphere, 19, 5547–5577, https://doi.org/10.5194/tc-19-5547-2025. 2025b

Booth, A. D., Clark, R. A., and Murray, T.: Influences on the resolution of GPR velocity analyses and a Monte Carlo simulation for establishing velocity precision, Near Surface Geophysics, 9, 399–411, https://doi.org/10.3997/1873-0604.2011019, 2011

S. Leinss and P. Bernhard, "TanDEM-X:Deriving InSAR Height Changes and Velocity Dynamics of Great Aletsch Glacier," in *IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing*, vol. 14, pp. 4798-4815, 2021, doi: 10.1109/JSTARS.2021.3078084.

Author Response to the RC2

Review of Patil et al., 2026 on ”Annual firn pack variations in the context of the climate settings: Example from the Grosser Aletschgletscher using multi-year radar observations”

RC2 comment date: June 8, 2026

Thank you for the thorough and helpful review of our manuscript. We are glad that our manuscript was overall well-received and appreciate your constructive feedback.
We agreed to integrate most of the comments and suggestions regarding the overall structure, methodological clarity and interpretation of results and discussion into our revised manuscript. Here we respond to each of RC2, three general comments, followed by line-by-line comments.

1 General comments

Overall structure, and narrative focus
The manuscript presents a valuable dataset and several interesting observations, but the overall narrative is at times difficult to follow and can read more as a collection of parallel analyses than a focused argument centred on a clearly defined contribution.

In my view, the main novelty lies in the use of repeat CMP/GPR measurements to quantify temporal firn density evolution and compaction. This contribution is partly obscured by extensive analyses of firn cores and snow pits and by detailed descriptions of results that are similar to what has already been shown by Patil et al. (2025).

The introduction provides a comprehensive background but is relatively slow to identify the specific knowledge gap addressed by the study. Novelty claims occasionally appear somewhat overstated, particularly regarding the uniqueness of Alpine firn studies. In contrast, the genuinely novel aspect the repeat CMP-based monitoring approach- should be highlighted much earlier.

We appreciate the acknowledgement of our valuable dataset. The aim was to provide a detailed, comprehensive introduction to the firn process, including densification and compaction, before we introduce the related knowledge gap. We

agreed to the suggested structural changes and condensed the introduction as needed, and provided the CMP-based study novelty earlier in the introduction. We restructured the manuscript, focused prominently on study novelty by curtailing detailed discussion of glaciological methods, which is similar to our previous work, wherever necessary.

I would recommend focusing the results and discussion more strongly on the changes observed between the two survey years, while keeping the presentation of the second-year glaciological and radar observations more concise. Establishing a clearer hierarchy between the core contribution and supporting analyses would improve readability and strengthen the overall narrative. At present, firn core and snow-pit observations are presented in considerable detail before the GPR results, which feels some what misaligned with the main objective. Conversely, the repeat CMP-derived compaction analysis receives relatively little discussion regarding its accuracy, efficiency, limitations, and potential for future applications or comparison with other methods such as ApRES.

Thanks a lot for your feedback on strengthening the research clarity. We revisited the manuscript and made changes in the results and discussion sections, focusing on temporal changes observed in two measurement years. We also reduced the descriptive analysis of glaciological results.
We have rare deep firn core results after Oeschger et al (1977) firn core analysis at the nearby location. We also have a detailed analysis of isotope and chemical impurities, which we used as direct validation for CMP3-derived density profiles and mainly to trace annual layers. For this reason, we described glaciological results from the 2025 dataset (firn cores) in detail.

However, we agree to shorten the snowpit and shallow firn cores analysis in detail, which are already shown in Patil et al. (2025). We shortened the details of the result section and discussion related to glaciological observation, considering the repeated analysis from our previous publication (Patil et al 2025). Further, we extended the discussion part related to accuracy, efficiency, limitations, and potential future applications of the CMP method by comparing it with other methods as suggested.

Finally, the manuscript would benefit from a clearer distinction between newly acquired results, previously published datasets, and established methods. At present, it is not always clear which findings are novel and which build directly on Patil et al. (2025). For example, spatial density differences between sites were already documented in Patil et al. (2025) using firn core and snow-pit data, yet some of these patterns are revisited here in considerable detail without always clarifying the additional insight gained. In such cases, the manuscript could be streamlined either by referring more directly to previous work or by explicitly highlighting the new contribution of the present analysis. Clarifying this distinction already in the abstract

and introduction would help readers more readily identify the study's novel contributions.

We thank the reviewer for a valuable suggestion to distinguish new results from our previous work. We rewrote the relevant sections and subsections of the manuscript, incorporating suggestions to streamline it by referring to our previous work wherever appropriate, and highlighted distinctive findings from Patil et al. (202
5). We also curtailed the repeated findings and corresponding discussion documented in Pati et al (2025) and kept the study's novelty at the centre. We also clarified the distinction between our new study (submitted manuscript) and Patil et al (2025) study in the abstract and introduction, as suggested.

2. Methodological clarity and conceptual consistency

Several parts of the methodology are currently under-specified and difficult to reproduce, particularly regarding GPR acquisition, processing, IRH interpretation, and the derivation of density and compaction from repeat CMP measurements.

For example, several important GPR acquisition and processing details required for reproducibility are currently missing or insufficiently described (e.g. stacking parameters, time windows, antenna configuration, trace spacing, profile acquisition procedure, processing settings, and uncertainty estimation).

The manuscript would benefit from a clearer overview of the full methodological workflow early in the methods section. In particular, the relationship between IRH identification, annual layer dating, density inversion, SWE matching, and compaction-rate estimation is not always clearly explained, and the rationale for several processing steps remains implicit. Introducing a short sentence in each section explaining why you are doing certain analysis steps would help guide the reader through the methods.

We appreciate the reviewer's insights into the methodological clarity and conceptual consistency. We extensively discussed the method to interpret IRH as an annual layer in Patil. et al. (2025) study. Patil et al (2025) study also provides details of methodological workflow explaining IRH identification, annual layer dating, density inversion and SWE matching in sections such as 3.2 and 3.5 in Patil et al (2025). Therefore, we kept the methodological workflow to a minimum by referring to our earlier work.
However, we noticed that our current manuscript is missing the GPR and CMP acquisition and processing details from our 2025 survey year. We made changes in the revised manuscript by considering the suggestions carefully. We provided a CMP and GPR acquisition parameter setting in separate tables within the main text. We also added details of certain analysis steps wherever necessary. Further, we

provided the rest of the methodological details in the supplement to avoid repetitive explanations as in our previous work.

A conceptual issue I struggled with throughout the manuscript is the use of the terms IRH, reflector, and layer. These concepts are sometimes used interchangeably, although they represent physically distinct entities. Since radar reflectors correspond to dielectric contrasts at interfaces between layers, it is not physically consistent to assign volumetric properties such as density “within” an IRH or reflector. Density and compaction should instead be associated with firn layers bounded by reflectors.

I encourage the authors to introduce a clearer conceptual framework distinguishing between what is directly measured (radar travel times and reflected wavefields), what is interpreted (reflectors/IRHs as interfaces), and what is subsequently derived (annual layer thickness, density, compaction).

We appreciate the reviewer’s suggestions on improving the conceptuality, mainly the terminology. We agreed and made corresponding changes in the revised manuscript. We also introduced a clearer conceptual framework as suggested.

3. Interpretation and discussion

In several places, interpretations appear stronger than what appears to be robustly supported by the available data. This is particularly true for climatic attribution statements linking differences in densification directly to temperature or “climate settings” despite limited supporting meteorological observations or longer temporal context. Given that the study is based primarily on two field campaigns separated by one year, I encourage the authors to frame such interpretations more cautiously and distinguish clearly between observed variability and inferred climatic drivers. In the specific comments below, I’ve highlighted a few instances where I think more supporting evidence is necessary, e.g. by referring to the firn core dataset.

We thank the reviewer for pointing out the overstatement of our interpretation. We revisited the manuscript and reframed such interpretations by carefully considering the climatic drivers and available supporting results. We addressed all specific comments in the revised manuscript.

I would also encourage a more explicit discussion of the advantages and limitations of the CMP approach relative to other methods (e.g. firn cores and ApRES), particularly regarding resolution, uncertainties, and assumptions. In addition, it would be helpful to clarify the degree to which CMP-derived density and compaction estimates depend on auxiliary information such as nearby firn cores, i.e. how independent the method is and under which conditions it remains robust. At present, the manuscript demonstrates the potential of the CMP method, but provides relatively limited discussion of its uncertainties, advantages, limitations, and broader

applicability. In my view, this could become one of the most valuable aspects of the paper if developed further.

We appreciate suggestions and feedback on improving the discussion. We agreed and provided an explicit discussion on the advantages and limitations of the CMP approach relative to other methods in the revised manuscript. We already provided the details on the resolution and uncertainties of the CMP method in section 6.5. Patil et al.'s (2025) study discusses uncertainties and resolution-based ambiguities in detail, and also provides related figures such as A1, A2 and A6-A9 in the appendix following Booth et al. (2011). We also cited the study in the uncertainty subsection. However, we added an extended discussion in the uncertainty subsection, considering advantages, limitations and broader applicability as suggested. We also clarified the dependency of CMP-derived density and compaction results on auxiliary information.

2 Line-by-line comments

- **Title:** The phrase “in the context of the climate settings” is too vague and does not clearly specify which climatic aspects are addressed.
  Thanks for the feedback. We made the needful changes.

- **Lines 4–6:** I think this novelty claim is too strong. There is considerable amount of literature on alpine firn already. I would try to be more specific and put more emphasis on the repeat GPR datasets to monitor change over a year.
  We appreciate comments and suggestions. We agree and rephrased the sentence accordingly.

- **Line 23:** Suggested rephrasing: “can be found in accumulation areas of ice caps, ice sheets and glaciers” accumulation areas.
  Thanks a lot for the suggestion. We rephrased the sentence as suggested.

- **Line 26:** It would be helpful to clearly define what you mean with “firn processes” early on (e.g. densification, meltwater percolation/refreezing, snow metamorphism, compaction), as the term remains vague throughout the introduction.
  Agreed, we made the necessary changes in the revised manuscript.

- **Line 30:** Remove “Similarly,”
  Removed.

- **Lines 50–52:** The distinction between firn densification and firn compaction is not clearly defined and may incorrectly suggest two separate processes. They

are more appropriately understood as coupled expressions of the same system (density change vs. vertical strain). This should be clarified early in the introduction.
Agreed, suggested changes have been made.

- **Lines 52–55:** This sentence conflates several ideas (compaction measurements, polar bias, and geodetic mass balance corrections), making the logic difficult to follow. It would be clearer to separate (i) why compaction matters for volume-to-mass conversion, (ii) why it has been mainly studied in polar regions, and (iii) why Alpine settings remain important.
  We appreciate the reviewer's suggestions. We changed and rephrased the sentence accordingly.

- **Line 60:** "conventional" is unclear—conventional compared to what?
  Agreed, we changed the sentence accordingly.

- **Line 69:** Citation missing.
  Thanks for noticing, citation added

- **Line 90:** I think the claim that no studies have addressed Alpine firn processes or multi-year geophysical/glaciological monitoring is overstated (e.g. Sold et al. (2015, 2016))**.**
  Agreed, we rephrased the sentence by considering the novelty of our study, "multi-year CMP measurements…".

- **Line 96:** What do the repeat CMP measurements reveal about temporal changes in the firn pack?
  Thanks for noticing the missing word "repeat or multi-year". Corrected.

- **Lines 100–102:** The link to limitations from prior work is too general. Please specify which limitations are addressed and how.
  Thanks for the suggestions. We made the necessary changes.

- **Lines 117–118:** Redundant sentence.
  Thanks, removed.

- **Figure 1 (main map):** It is not always clear where labels (e.g. SP2, FC2) belong. Panel (c) appears geometrically distorted.
  Thanks a lot. We changed the position of labels on the map, considering the clarity and no overlap. We adjusted the distorted panel (C) in Figure 1.

- **Figure 1 (caption):** The 2024 dataset should be explicitly cited. The Swiss Federal Office of Topography map background should also be properly cited and/or linked.

Thanks for the suggestions. We made changes as suggested by adding citations and links wherever necessary within the figure caption.

- **Lines 129–130:** It is unclear how density is derived from the cone — please clarify.
  Thanks, corrected for the required clarification.

- **Line 145:** Citation missing.
  Thanks for noticing, citation added.

- **Line 146:** “Similar locations” is vague — please specify distances between sites.
  Thanks for the suggestion. The correction has been made accordingly.

- **Section 3.1:** Why was the drilling system changed between FC2 and FC3? Why were isotopes/chemistry only measured for FC3?
  Because of time constraints and equipment availability, we used a Kovacs for the Shallow firn core (FC2) and an electromechanical drill for the deep firn core. Isotope and chemical impurity analyses were dedicated to the deep firn core due to funding availability and focus of our study; expecting more melt at the CMP4 location did not favour the idea of isotope and impurity analysis.

- **Lines 149–154:** Key GPR acquisition parameters (time window, stacking, sampling rate, antenna configuration) are missing. CMP concept could be briefly explained for non-specialists. Was antenna spacing increased in the direction parallel to the GPR profile/flow direction or perpendicular? Also for the GPR-profiles additional field deployment details (towing method, spacing of sampling, walking speed) are needed for reproducibility. Why did you choose to deviate from the previous year’s GPR profile?
  Thanks for the suggestions. We added missing CMP acquisition parameters in a table, along with the suggested GPR profile field deployment details. The CMP method is well known within the community; we provided the basic CMP concept in the supplement. CMP measurements were carried out perpendicular to the flow direction, as was the spacing. We added this information in the main text. The GPR long profile was measured from the lower elevation, and due to time and weather constraints, we could not follow the same transect. We thought it would be ideal to have a long GPR profile along the flow line, unlike the 2024 measurements.

- **Section 4.1:** The GPR processing workflow is insufficiently detailed and not fully reproducible. A clearer, structured description of all processing steps and parameters is needed (e.g. filter cutoff frequencies, number of stacking).
  Thanks for the suggestions. We added the CMP workflow in the table along with the suggested parameters.

- **Line 167:** the layers do not look perfectly horizontal in the GPR profile (Fig. 3), particularly at CMP 3. It would be worth mentioning in the discussion how this potentially impacts the results.
  We appreciate the suggestion. The CMP measurements were perpendicular to the GPR profile. Also, the total CMP spacing is 20 m compared to the longer GPR transect. It is also worth noting that the CMP provides point information. We discussed these points and how they affect the results as suggested.

- **Lines 167–169:** The CMP conceptual description appears too late in the methods. It would improve clarity if introduced earlier.
  Thanks for the suggestions. The description in the manuscript follows the direct (glaciological) and indirect (GPR) measurements. We revisited the manuscript and made the changes accordingly.

- **Lines 172–173:** This step is not clearly explained in Section 4.1.
  Thanks for the suggestions. We made the suggested changes in Section 4.1

- **Line 174:** VRMS is not defined until here.
  Thanks, corrected for the suggested changes.

- **Lines 177 and related:** “Within each IRH” is conceptually unclear. IRHs are interfaces, not volumes, so assigning density to them is physically inconsistent. Please distinguish clearly between IRHs (interfaces) and firn layers (volumes). (see also my general comment).
  Thanks for the valuable feedback. We corrected the terminology throughout the manuscript as suggested.

- **Line 181:** Same issue: mixing layers, interfaces, and reflectors leads to conceptual inconsistency.
  We appreciate noticing the conceptual inconsistency. We made the required correction.

- **Line 186:** Vfirn is not defined in Eq. 1.
  Thanks for noticing that, in Equation 1, Vint = Vfirn, we added that information.

- **Equation 3:** Please clarify indexing 12 (alternatively use i, i+1 instead of “12” to generalize IRHs).
  We appreciate the suggestion. We changed the indexing accordingly.

- **Lines 192–194:** Clarify workflow: distinction between methods for CMP2/4 vs CMP3 could be made more explicit.

Thanks for the suggestion. We added the explicit workflow for CMP2/4 vs CMP3 methods.

- **Lines 193–194:** Isn't Vint also used in depth conversion, together with TWT?
  Yes, Vint is used for the depth conversion along with TWT.

- **Lines 200–201:** "until a match is found" is not very clear.
  Thanks, corrected.

- **Lines 213–214:** This sentence is unclear and does not add essential methodological information; either clarify purpose or remove.
  Thanks for the suggestion. We removed the sentence.

- **Line 215:** Previous method should be explicitly cited.
  Thanks, citation added.

- **Equation 4:** Clarify that this refers to time between repeated measurements.
  Thanks for the suggestion. We added the suggested information to clarify Equation 4, which refers to the time between two repeat measurements.

- **Lines 235–236:** Replace "direct glaciological observations" with firn cores/snow pit to be more specific.
  We appreciate your suggestion. We replaced the "direct glaciological observations" with firn cores/snow pits.

- **Lines 236–237:** Clearly label firn core FC3 (in addition/instead of 19.4 m long core)
  Thanks, we corrected the suggested changes.

- **Lines 236–240:** Apparent inconsistency in description of pore close off density vs depth.
  Thanks for noticing. We corrected for the same.

- **Figure 3:** Appears misplaced in Methods; likely belongs in Results? It would be useful to show which part of the profiles are shown in panel b and c. Why did you not pick the same horizons in panel c as in panel b?
  We appreciate your feedback. We can see that Figure 3 is in the result section between lines 241-243. If it appears before the results section, it looks like a classic LaTeX mistake. However, it can be addressed during typesetting. We explained in the caption what exactly panels b and c show. The annotated picking in figures 3b and c is an example of picking shown for readers. The same horizons can be picked, but affects the appearance of the radargram for the reader. If needed, we can provide an alternative figure in the supplement indicating the suggested picking of the same horizons.

- **Figure 4:** You could consider adding the vertical colored bars in the legend to see at first sight what they represent. Why are horizontal bars not of equal length? Here and in other figures like this showing discrete measurements: You could consider using a step plot instead of interpolated between measurement sections to better represent the 20 cm sections you used for density measurements.
  We appreciate the suggestions. However, we believe vertical bars represent the theoretical values, so we prefer explaining them in the caption rather than adding them in the legend. Horizontal bars can be extended fully, but the idea is that the shortened horizontal bars represent the corresponding extent of density profiles. We also provided the suggested plot in the supplements. Yes, we can consider the step plot, but to keep figures 4 and 5 (a) the same, we preferred the interpolated plot.

- **Line 245:** Replace “small depth” with “shallow depth”.
  Thanks, replaced as suggested.

- **Line 279:** “Similar locations” is vague — specify distances.
  Thanks for the suggestion. We corrected for the suggested changes.

- **Figure 7:** Use just one vertical axes (depth instead of TWT) for comparability
  We appreciate the feedback. However, each CMP density-depth profile changes with changing interval velocities; that is, for the same TWT, the estimated depth varies at three locations. The figure represents the different density-depth profiles showing variability in TWT and depth, estimated using interval velocity and TWT. The figure also includes a glaciologically derived density-depth profile, which is more suited when plotted in depth scale (m).

- **Lines 316-317:** ”At the far right end of the radargram, it is difficult to distinguish between IRHs, which can be interpreted as ice”. I find this statement misleading. It is not generally true that ice doesn’t show IRH and therefore you can interpret this section as ice. From Fig. 1 it looks like you’re getting into a zone of crevasses which could be the reason why the wave energy is more scattered and you don’t see reflections?
  Thanks a lot for the feedback. Yes, we agree that the statement is misleading; the statement has been changed accordingly. We discussed the reason for our interpretation of the spatial firn to ice transition (accumulation to ablation area) in section 6.2, lines 448-450. The GLAMOS 2025 data suggest that ELA is at an approximate elevation of 3200 m a.s.l. This is further evident from our observation during the field measurement and aerial images from swisstopo (Figure 1), that at a distance of ~3500 m onwards (Figure 3a), we reach an ablation area indicating no evidence of firn below an elevation of 3200 m a.s.l. The same is also evident from the radargram Figure 3 (a).

- **Lines 387-389:** Do you mean: ”We observed a slight decrease in slightly lower surface snow density at the FC3 (3450 m.a.s.l.) compared with the FC2 (3360 m.a.s.l.) ...” I think that the difference in elevation is a plausible explanation for density differences, but is perhaps not the only possibility? What about wind patterns or differences in accumulation due to local topographies? At the moment, your conclusion is not very well supported without directly linking it to other data, e.g. firn cores, temperature records etc.
  Thanks a lot for pointing out other possible explanations. Yes, wind-drifted snow and variable precipitation can also contribute to the change in surface snow density; these effects are hard to quantify from the field measurements. We added the suggested information considering firn cores and temperature records.

- **Line 395–396:** “Weather patterns” is vague — consider linking explicitly to temperature?
  Thanks for the suggestions. We made changes accordingly.

- **Line 418:** you mean “snowfall intense” ?
  Yes, intense winter means “snowfall is intense”.

- **Line 457–470:** This part is hard to follow; there are multiple hypotheses without clear hierarchy and pro/cons that indicate which might be the most likely explanation of this reflector. It’s unclear what you mean with ”resulted from glacier dynamic” and with ”dividing into cold and temperate zone”. Which zone would be temperate according to your interpretation? I think in the scope of your work, this is a secondary observation, and doesn’t need to be discussed in such length if you are not sure what it is/or that it is relevant in the context of your results.
  We appreciate your feedback. Yes, these are our secondary observations. We do not have any supporting evidence to attribute the visible strong IRH depth below 250 ns between 0-2400 m from the upper to the lower part of Ewigschneefeld (Fig 3a) to glacier dynamics. Our interpretation is more hypothetical at this point. We shortened the discussion and made the necessary changes to the revised manuscript.

- **Line 458:** citation missing
  Thanks, citation added.

- **Line 462:** with distance to what?
  Thanks for noticing this; it's the distance to the GRP profile. That is along the GPR profile in Figure 3a. We made the required changes.

- **Lines 489–490:** I believe this assumption is only true under steady state conditions, e.g. constant temperature and accumulation rate conditions. I don't think these steady state conditions apply here though, so this might be too simplified. The profiles look similar, but there are also clear offsets, e.g. at 20 m depth, so it is unclear to me what you want to conclude from this.
  Thanks for noticing the unclear statement on Sorge's law and its validation under steady state conditions. We changed the related discussion part according to the suggestions and improved the conclusion on Soge's law applicability at the CMP3 location.

- **Lines 480–482:** I find the beginning of the second sentence a bit confusing: " We attribute variations in densification at this location ..." what do you mean with the variations in densification when the density profiles keeps its shape, i.e the densification is uniform with depth?
  Thanks a lot for the feedback. It is supposed to be "potential processes leading to density changes…". We made necessary changes by rephrasing the sentence.

- **Line 483:** Potential typo: isn't Layer 5 the most dense in Fig. 9? Do you have an explanation for the higher density at this depth?
  Layer 5 has an initial high-density layer. The discussion is about the change in density. In layers 3 and 4, the change in density is greater over a year compared to other older layers. The temporal change in density within identified annual layers from Layer 5 to 10 is relatively lower compared to the layers 3 and 4. High-density layer 5 could be due to the high summer melt and low winter accumulation (Figure A1), along with the added overburden pressure from accumulated five-year snow.

- **Lines 493–497:** I think this is an important aspect of the discussion and should be elaborated by comparing these two methods more extensively
  We appreciate your suggestions. Yes, we agree that it needs an elaborate discussion. We added the extended discussion to the revised manuscript.

- **Lines 498–502:** you claim that the higher densification over the years in CMP 4 compared to CMP 3 is due to higher temperatures, but the argument that " this is evident from the significantly higher densification rate" is circular. This statement needs to be more carefully linked to other records indicating different temperatures (e.g. the melt layers).
  Thanks a lot for your feedback and suggestions. We added further discussion by linking it to the suggested temperature and accumulation by comparing records over climatological and measurement periods.

- **Line 520:** annual layers 0 and 1 representing 2023 and 2021 seems not logic. Perhaps you could mention that the 2022 layer melted away?

We appreciate noticing our mistake. We added suggested information on the 2022 melt layer, which is completely ablated at the CMP4 location.

- **Line 520:** higher compared to what?
  Thanks for the suggestion. We corrected for the same. “Higher compaction compared to subsequent layers”.

- **Lines 520–521:** lower firn air content is also a result of firn compaction, so this argument is unclear.
  Yes, agreed, we reframed the sentence accordingly.

- **Line 523:** it’s not so clear from the phrasing that the 2024 accumulation was highest in the last 20 years and that this is not referring to exceptionally high melting and refreezing.
  We appreciate your feedback. We rephrased the sentence considering the feedback.

- **Line 608:** Suggested rephrasing: a novel geophysical approach using repeat GPR-based CMP measurements (because CMP itself and its application to firn is not new)
  Thanks for the suggestion. We made the changes accordingly.

- **lines 587–589:** I think this is an important point and deserves more discussion. What are the advantages of CMP compared to ApRES or firn cores? You are presenting a new method for deriving firn compaction rates with CMP but there is very little discussion of how it compares with other methods.
  We appreciate your feedback. Yes, we agree that it deserves more discussion. We discussed extensively within the same paragraph.

References

Booth, A. D., Clark, R. A., and Murray, T.: Influences on the resolution of GPR velocity analyses and a Monte Carlo simulation for estab lishing velocity precision, Near Surface Geophysics, 9, 399–411, https://doi.org/10.3997/1873-0604.2011019, 2011

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