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.

Author Response date: 24.06.2025 by Patil et al

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

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