

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"

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The manuscript presents a multi-method investigation of firn properties in the accumulation area of the Grosser Aletschgletscher (Swiss Alps), combining repeat ground-penetrating radar (CMP and GPR transects), firn cores, snow pits, and geochemical/isotopic analyses. The study aims to characterize spatial and temporal variations in firn density, stratigraphy, and compaction, and to relate these observations to accumulation variability and recent climatic conditions.

A central component of the manuscript is the use of repeated CMP surveys to derive firn density profiles and track changes in internal reflection horizons (IRHs) between 2024 and 2025. These radar-derived observations are compared with firn-core measurements and geochemical proxies to establish stratigraphic interpretations and derive firn compaction rates.

The manuscript presents a valuable and relatively rare multi-proxy dataset from an Alpine firn environment, and the repeat CMP approach has clear potential as an efficient method for monitoring firn evolution. The interdisciplinary combination of geophysical and glaciological observations is also well suited for publication in *The Cryosphere*. However, in its current form, the manuscript would benefit from substantial revisions before publication.

Overall, I found that the main scientific contribution of the study is currently obscured by diffuse structure, insufficient methodological clarity, and an overly broad narrative. In particular, the novelty and implications of the repeat CMP-based compaction analysis are not consistently foregrounded, despite representing the most original aspect of the work. I also found it difficult in several places to distinguish which observations and analyses are genuinely new in this manuscript and which have already been presented or discussed in [Patil et al. \(2025\)](#). A clearer separation between previously published results and the new contributions of the present study would substantially improve transparency and help better position the manuscript within the context of the authors' earlier work. At the same time, several methodological descriptions lack sufficient detail for reproducibility, and some interpretations, particularly regarding climatic controls on densification, appear stronger than what is robustly supported by the available observations. The language in many instances is vague or unclear so it is not always easy to follow the argumentation.

Nevertheless, I believe these issues are largely structural and can be addressed through revision. The dataset is valuable, the repeat CMP approach is promising, and the study has the potential to make a

meaningful contribution to firn monitoring and characterization in Alpine environments if the main findings and methodological advances are more clearly articulated and placed at the centre of the manuscript. I therefore recommend this manuscript for publication after revision.

My review is structured in three general comments, followed by line by line specific comments.

1 General comments

1. 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.

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 somewhat 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.

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.

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 guiding the reader through the methods.

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).

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.

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.

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.

- **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.
- **Line 23:** Suggested rephrasing: “can be found in *accumulation areas of ice caps, ice sheets and glaciers*” ~~accumulation areas~~
- **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.
- **Line 30:** Remove “Similarly,”
- **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.
- **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.
- **Line 60:** “conventional” is unclear—conventional compared to what?
- **Line 69:** Citation missing.
- **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. \(2015, 2016\)](#))
- **Line 96:** What do the *repeat* CMP measurements reveal about temporal changes in the firn pack?
- **Lines 100–102:** The link to limitations from prior work is too general. Please specify which limitations are addressed and how.
- **Lines 117–118:** Redundant sentence.
- **Figure 1 (main map):** It is not always clear where labels (e.g. SP2, FC2) belong. Panel (c) appears geometrically distorted.
- **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.
- **Lines 129–130:** It is unclear how density is derived from the cone — please clarify.
- **Line 145:** Citation missing.
- **Line 146:** “Similar locations” is vague — please specify distances between sites.
- **Section 3.1:** Why was the drilling system changed between FC2 and FC3? Why were isotopes/chemistry only measured for FC3?

- **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?

- **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).
- **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.
- **Lines 167–169:** The CMP conceptual description appears too late in the methods. It would improve clarity if introduced earlier.
- **Lines 172–173:** This step is not clearly explained in Section 4.1.
- **Line 174:** V_{RMS} is not defined until here.
- **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).
- **Line 181:** Same issue: mixing layers, interfaces, and reflectors leads to conceptual inconsistency.
- **Line 186:** V_{firn} is not defined in Eq. 1.
- **Equation 3:** Please clarify indexing 12 (alternatively use $i, i+1$ instead of “12” to generalize IRHs).
- **Lines 192–194:** Clarify workflow: distinction between methods for CMP2/4 vs CMP3 could be made more explicit.
- **Lines 193–194:** Isn’t V_{int} also used in depth conversion, together with TWT?
- **Lines 200–201:** ”until a match is found” is not very clear.
- **Lines 213–214:** This sentence is unclear and does not add essential methodological information; either clarify purpose or remove.
- **Line 215:** Previous method should be explicitly cited.
- **Equation 4:** Clarify that this refers to time between repeated measurements.
- **Lines 235–236:** Replace “direct glaciological observations” with firn cores/snow pit to be more specific.
- **Lines 236–237:** Clearly label firn core FC3 (in addition/instead of 19.4 m long core)
- **Lines 236–240:** Apparent inconsistency in description of pore close off density vs depth.

- **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?
- **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.
- **Line 245:** Replace “small depth” with “shallow depth”.
- **Line 279:** “Similar locations” is vague — specify distances.
- **Figure 7:** Use just one vertical axes (depth instead of TWT) for comparability
- **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?
- **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.

- **Line 395–396:** “Weather patterns” is vague — consider linking explicitly to temperature?
- **Line 418:** you mean “*snowfall* 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.

- **Line 458:** citation missing
- **Line 462:** with distance to what?
- **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.

- **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?
- **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?
- **Lines 493–497:** I think this is an important aspect of the discussion and should be elaborated by comparing these two methods more extensively
- **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).
- **Line 520:** annual layers 0 and 1 representing 2023 and 2021 seems not logic. Perhaps you could mention that the 2022 layer melted away?
- **Line 520:** higher compared to what?
- **Lines 520–521:** lower firn air content is also a result of firn compaction, so this argument is unclear.
- **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.
- **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)
- 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.

References

- Patil, A. M., Mayer, C., Seehaus, T., Groos, A. R., and Bauder, A. (2025). Investigating firn structure and density in the accumulation area of the grosser aletschgletscher using ground-penetrating radar. *The Cryosphere*, 19(11):5547–5577.
- Sold, L., Huss, M., Eichler, A., Schwikowski, M., and Hölzle, M. (2015). Unlocking annual firn layer water equivalents from ground-penetrating radar data on an alpine glacier. *The Cryosphere*, 9(3):1075–1087.
- Sold, L., Huss, M., Machguth, H., Joerg, P. C., Leysinger Vieli, G., Linsbauer, A., Salzmann, N., Zemp, M., and Hoelzle, M. (2016). Mass balance re-analysis of findelengletscher, switzerland; benefits of extensive snow accumulation measurements. *Frontiers in Earth Science*, 4:18.