Answer to Editor

Dear Editor,

Please find attached the revised version of our manuscript, together with a detailed point-by-point response to the reviewers' comments, as requested. We have addressed all points raised during the review process and have revised the manuscript accordingly. We believe that these revisions have substantially strengthened and improved the quality, clarity, and overall presentation of the manuscript.

In the accompanying response document, we provide detailed explanations of the changes made and indicate where the corresponding modifications can be found in the revised manuscript. We hope that the revised version satisfactorily addresses all concerns and will now be suitable for publication.

Thank you for your consideration and for the opportunity to revise our work.

Kind regards,
Riccardo Scandroglio
on behalf of all authors

Answer to Reviewer #1

Dear Reviewer #1,

Thank you for taking the time to review our manuscript and for your insightful feedback. We appreciate your detailed comments, which have helped to improve the quality of our work. Below, we address each of your points in turn. We believe these revisions enhance the clarity and quality of the manuscript, and we hope the changes meet your expectations. We look forward to hearing your thoughts.

Sincerely,

Riccardo Scandroglio on behalf of all authors

Answer to comments of Reviewer #1:

Line 49: I think you mean automated.
Line 52: As above.

Both have been corrected.

Line 62: Shallow borehole data has been used to calibrate automated ERT datasets, please see Cimpoiasu et al. (2024, 2025)

Thanks for the suggestions. We added a sentence with these references.

Line 79: m of more or less

Corrected.

Line 95: “m asl” acronym was used above, please be consistent.

Corrected throughout the whole text.

Figure 1: b) attach color scale please; labels of main and side tunnels need to be slightly more readable
d) – caption: “temperature loggers”
d) Is that meant to represent data retention post filtering? it is not explained very well in text and caption. Please add more detail.

The figure has been improved as required.

Subfigure d) represents the number of recorded quadrupoles. Caption has been improved.

Line 132: I am assuming you are doing a timelapse inversion here, but you have not specified.

We chose not to use timelapse inversions, but independent inversions. The idea behind it is to reduce any temporal constrains (e.g. the previous inversion, or the first inversion) that could influence the results. We believe that this increases the value of our results, showing consistent trends even without time constraints.

Line 172: “Side tunnel”.

Corrected.

Line 196: The authors would agree that resistivity exhibits a **different relationship with temperature above and below the freezing point**, at which the value of 29 kΩm was measured. However, it seems like inferences about the freezing point are mostly reliant on a more thorough analysis discussed in another paper: Krautblatter et al. (2010). For clarity, I would encourage the authors to either a) explain in more detail the lab measurements they make references to or b) **summarize the findings of Krautblatter et al. (2010).**

We agree with the reviewer and added a detailed paragraph to the discussion part (Line 330).

Figure 5: delete space in "E 53" / b) I don’t understand the color scheme / should be subplot or subfigure c) d)

Figure improved as requested.

Line 209: I am not sure average resistivity a representative value for the rock/matrix type. Is this the average across all records?

We agree and removed these numbers – they are also not relevant for the key message.

Figure 6: “the lower image represents the subsection of b outlined by a black rectangle”

Corrected

Figure 7: There is no X axis, so the text Line 238-245 is very hard to follow. Replace “following figure” with “figure 8”.

> Figure 8a) has been moved to Figure 7b) so that the X and Y axes are now visible and the interpretation is easier for readers.

3.3: You don’t report any metrics regarding the %RMS of your inversions.

> Metrics for the RMS have been included: a graph in the supplementary material (Figure S9) and a paragraph in the results (Line 376).

Section 3: With such a long record of measurements and having experienced some problems which lead to faulty datasets, isn’t there some scope to add a section about “lessons learned” in reference to the monitoring setup. I am sure it will make an interesting read and addition to this work.

> We added one chapter in the discussion.

Figure 8: Is the max % coverage of frozen areas also changing?

> The max % of frozen area is indeed also changing, as confirmed by Fig 7.

There is no color scale for resistivity here.

> Added

Line 255: I think you need a justification for choosing that specific number of clusters.

> The number of clusters was determined using the elbow method, as suggested by Cimpoiasu et al (2025).

Line 269: 39,000.

> Corrected

Figure 11: What is MMAT? What is mm12m/mm36m?

> Abbreviations are now explained in the caption.

Section 4: You talk a bit about future scenarios in which a critical point of low permafrost might be reached, generating geohazards, such as rock/landslides. Given that you have an automated set-up and you have the potential to track the evolution of temperature and permafrost coverage in near real time, would it be scope to discuss some parameters one could be looking at in order to evaluate the state of emergency and perhaps form the basis of an early warning system?

> The current setup was not fully automated; instead, the system was installed individually for each campaign and dismantled afterward. Consequently, it does not enable real-time monitoring.
>
> The potential of automated ERT has been assessed in Keuschnig et al. (2017). The calibration and error estimates in this paper contribute to a more robust implication of ERT in monitoring and early warning systems around critical infrastructure i.e. mountain huts and at cable car stations, where less calibrated approaches are currently tested.

Recently, fixed and automated ERT monitoring systems have been implemented on steep permafrost slopes (e.g., studies by Magnin and Offer). In principle, such systems could allow continuous resistivity measurements and provide near–real-time insights into permafrost conditions. However, to our knowledge, these approaches have not yet reached a level of maturity that enables reliable operational use for early warning purposes. Achieving this would require developing robust automated workflows for both data acquisition and processing, and establishing clearly defined early warning criteria, as suggested by Reviewer #1. Potential criteria could include thresholds for active-layer thickness or for resistivity values approaching the thawing point.

Importantly, ERT-derived information alone is insufficient for early warning applications. It must be complemented by detailed geological characterization, including surface fracture mapping and, ideally, subsurface information obtained from boreholes or tunnels to identify critical discontinuities and preferential ways for water flow. Furthermore, reliable quantitative interpretation of resistivity data requires field calibration over at least one complete freeze–thaw cycle and across multiple depths, as demonstrated in this study.

This answer was partially added to the discussion (Line 457).

Section 4: Are there any future plans for this site? One thing to consider would be to have an increased measurement frequency around the shoulder/warm season? (not sure if the battery power would be an issue). This way you can better capture the active layer evolution and heat transport.

Monthly measurements have continued uninterrupted in recent years as part of another project, and the data collected will support future analyses.

An increased measurement frequency at this site was tested in 2017 for one season, but yielded poor results due to cable failures. Figure 9 shows that the active layer typically thaws within two to four months, with the exception of 2023, when this process occurred within a single month. Increasing the measurement frequency from April to July could provide valuable insights into the delayed thermal response of deeper rock layers; however, this would require greater human effort or the implementation of an automated system, which would in turn be exposed to potential lightning hazards at the site.

Among all the sites we have instrumented, this is the most exposed to severe lightning, with sometimes daily return periods in the summer months. We have tested different approaches to achieve more continuous monitoring since 2009, but we have not found an approach that can bypass severe overvoltage other than disconnecting the overvoltage. This will also apply to other highly exposed summits that are not shielded by infrastructure.

One sentence on this point was added to the discussion.

Answer to Reviewer #2

Dear Reviewer #2,

Thank you very much for taking the time to review our manuscript. We genuinely appreciate your detailed and constructive feedback. We are confident that addressing your comments significantly improved the quality of the manuscript.

Here you find our answer to the major comments, and further down, the answers to comments in the text.

Sincerely,

Riccardo Scandroglio on behalf of all authors

Answer to major comments of Reviewer #2:

1. Lack of ERT Data Processing Details: For a manuscript whose primary findings rely almost entirely on ERT measurements, the description of the data processing is extremely brief. The authors state only that data were filtered using the variance of resistance to remove systematic errors. There is no information provided regarding data exclusion thresholds, contact resistance issues, or the percentage of data retained per dataset. These details are critical for assessing the quality of the raw data before inversion.

1) In the first version, we intentionally kept the description of data processing concise, as the applied procedures largely follow those presented in the previous study by Krautblatter et al. (2010). However, we agree with Reviewer #2 and the Editor that including these details improves the clarity and completeness of the manuscript. Accordingly, we have significantly expanded the methodological description in Sections 2.3 (“Monitoring setup”) and 2.4 (“Data analysis”), addressing the points raised by Reviewer #2 here and providing the required additional relevant details.

2. Missing Time-Lapse Inversion Strategy: The authors note that an updated version of CRTomo was used, but they completely omit their time-lapse inversion strategy. Time-lapse ERT is highly sensitive to the chosen regularization approach (e.g., independent inversions, difference inversion, or fully 4D spatio-temporal regularization). Without knowing how the temporal constraints were applied, it is impossible to determine whether the reported 25% decrease in resistivity is a robust physical trend or an artifact of the inversion process.

2) This is an important point, as the chosen time-lapse strategy can significantly influence the results, as demonstrated by Scandroglio et al. (2021). In line with the approach of Krautblatter et al. (2010), we applied independent inversions for each dataset. This strategy aims to minimize the influence of temporal constraints - such as dependencies on the previous or the initial inversion models - that could bias the outcomes. We argue that this approach strengthens the robustness of our results, as consistent trends are observed even in the absence of imposed temporal constraints. To clarify this methodological choice, we have added a dedicated paragraph in Section 2.4 (“Data analysis”).

3. Ambiguity in the Temperature-Resistivity (T-rho) Comparison: *The methodology linking ERT-derived resistivities to measured borehole temperatures is unclear. The text states: "Values from the side tunnel (Transect A-A') were used for the field calibration together with values of rho extracted from raw data, as normally done in laboratory calibrations". Does "raw data" refer to apparent resistivity or inverted cell resistivity? If inverted data were used, how were the cells extracted relative to the temperature sensors? Was a nearest-neighbour approach used, or an average of surrounding cells? The spatial scaling between a 1D point sensor and a 2D ERT grid should be explicitly defined.*

3) The caption of Figure 4 and Section 2.4 (“Data analysis”) have been revised for improved clarity. Specifically, the description now reads: “The T–ρ field calibration (Figs. 3 and 4) was conducted by combining these temperature values with single apparent resistivity values from the closest quadrupole to each temperature logger (within a 0.3 m radius).”

4. K-means Clustering and Missing Uncertainty bounds: *The authors apply a k-means clustering algorithm to segment the tomograms. However, there is no justification for why k-means was selected over other clustering methods. Furthermore, the authors acknowledge that "clustering is not unique, since multiple solutions are possible". Despite this non-uniqueness, the clustered results are used to generate highly specific, deterministic volume estimates (e.g., estimating 40,000 to 60,000 m³ of frozen rock, and projecting the loss of 39,000 m³ in the next decade). I find deriving these volumetric metrics without assigning any confidence intervals or uncertainty bounds quite dangerous.*

4) The main purpose of this approach is to highlight domains or potential zones of interest in an independent manner, not influenced by expert-based a priori knowledge. Further on, the aim of this paper is not to discuss clustering strategies and different algorithms. This has already been presented by Delforge et al (2021), who compare the three clustering algorithms k-means, hierarchical agglomerative clustering (HAC), and Gaussian Mixture Model (GMM), and include the question of the optimal choice of cluster number. Further literature:
   - Cimpoiasu et al. (2025), also used the k-means and elbow approach.
   - Watlet et al. (2023), used the Hierarchical Agglomerative Clustering (HAC)

   Actually, “multiple solutions are possible” is not precise; therefore has been substituted with: “This approach converges to a local minimum, though lower minima may exist. In practice, the global minimum is approximated by running multiple replicates with random starting points.“

   Confidence intervals are now included in the graph.

5. Overemphasis on the Error Model: One of the main focal points of the paper is the application of an updated error model. While proper error weighting is important for image resolution, it distracts from the physical findings of the paper. A range of existing literature has already demonstrated the impact of error models on inversion results. Elevating this to a core research question fragments the narrative, especially given the lack of detail on the broader inversion strategy. This discussion would be better suited as a methodological footnote or moved to the supplementary material.

5) The 17-year dataset provides a unique opportunity to test whether error models require a constant shift or should rather be kept at the original settings which is a longstanding debate in the history of quantitative ERT interpretation. We think that next to the climate-related permafrost signal revealed by the ERT this is a major outcome for data series spanning more than a decade and has a wide implication for other long-term datasets.

We therefore consider the error model to be a central pillar of this article, as it enables a more robust qualitative analysis and reveals details that were not discernible with the previous model. For this reason, we believe it should remain in the main text rather than being moved elsewhere. Please also refer to our response below, where we further elaborate on the paper's overall concept.
In addition, we have substantially revised and expanded the methodology section as requested, and we believe that the previous lack of detail has now been adequately addressed.

6. Unclear Manuscript Goals: The paper lacks a clear "red line." The introduction lists four distinct research questions, but the manuscript struggles to synthesize them into a unified conclusion. The authors should decide what the primary message of this paper is, is it a methodological paper about field calibration and error models, or is it a geomorphological paper about climate-driven permafrost degradation? Streamlining the narrative will greatly improve the manuscript's impact.

6) The core objective of this paper remains the detection and quantification of permafrost degradation. In addition, we provide insights into field calibration and error estimation - two essential components of any monitoring approach that significantly enhance data analysis and interpretation, as we demonstrate here. As a follow-up to Krautblatter et al. (2010), the manuscript maintains a similar structure, which we believe makes it accessible to a broad audience while emphasizing the importance of robust, accurate methodology in geomorphological research.
We have worked intensively on the manuscript to provide a more synthetic discussion and conclusion that unify and integrate the major outcomes. We think that the overall concept is now clear, and, with the suggested revisions, the central narrative ("red line") has become more coherent and visible. We sincerely thank both reviewers for their constructive feedback.

Answers to the comments in the attachment of Reviewer #2

Line 11: "degradation rated up to −4.2 kΩmy−1." This seems quite site specific, and I'm not sure that this should be stated in the abstract.

This article focuses on quantifying changes, so it is important for us to provide figures, even if they are site-specific. These values, along with others, can be used for comparison purposes.

Line 26: Doesn't this bring you back to the dependence on the ice content?

Thanks, we have incorporated this specification into the sentence for added clarity.

Line 72: I fully agree that this is important. In the end this goes back to the importance of the error model on the inversion itself, since you are translating inverted resistivity into temperature. Now the impact of the error model on the resistivity inversion has been well studied (see, e.g., Tso et al, 2017). **So is there really a need to make this point so prominent?**

Tso, C.-H. M., Kuras, O., Wilkinson, P. B., Uhlemann, S., Chambers, J. E., Meldrum, P. I., Graham, J., Sherlock, E. F., & Binley, A. (2017). Improved characterisation and modelling of measurement errors in electrical resistivity tomography (ERT) surveys. Journal of Applied Geophysics, 146, 103–119. https://doi.org/10.1016/j.jappgeo.2017.09.009

Yes, because in our study the error model does not only influence the resistivity inversion itself, but also directly affects the subsequent translation of resistivity into temperature, which is the key step for interpreting permafrost conditions. While the impact of error models on resistivity inversion has indeed been discussed in the literature (e.g., Tso et al., 2017), their implications for the derived thermal interpretation remain less explicitly addressed. In our case, even small differences in the inversion propagate into the temperature estimates and can significantly alter the qualitative and quantitative assessment of permafrost degradation. Emphasizing this aspect is therefore necessary to demonstrate how methodological choices at the inversion stage influence the final geocryological interpretation, which is central to this paper's objectives.

Line 79: *"its summit is composed of 600–800 m more or less uniform layers of Triassic Wetterstein"* – more or less is a very vague description.

Improved in the text.

Line 110: I find the structure of this section confusing. Why do you need to start with "Bedrock electrical resistivity:" and then follow with "Bedrock temperature:". This seems unnecessary.

Improved in the text.

Figure 1: Where is north here? There is no color scale here. I assume that you show the temperature here? Or is it resistivity? What is this lower graph showing? Is this the amount of collected data? Or the data that remains after filtering? There is no axis label.

North arrow and color bar (resistivity) have been added.

The lower graph represents the number of recorded quadrupoles. Caption has been improved.

Table 1: You list the precision here, but what is the accuracy? Have they been calibrated? Especially for the iButton sensors, their reading can fluctuate a lot in my experience. So I would have thought that if you want to measure "reliably" in the -2 - 1° range, you would need to calibrate them.

All loggers displayed in this study have been calibrated.
The ST, AT, and RST loggers (Geoprecision) were carefully calibrated at 0 °C by the producer using a high-precision calibrator with a temperature stability of ±0.005 °C. The exact zero-degree reference point was determined using a water triple-point cell. This procedure ensures highly accurate, reproducible calibration of the measurement system.
The iButton loggers were calibrated by the author in an ice bath multiple times during this study to correct for drift.

Table 1: What does distance refer to here? Is this the spacing between sensors?

This is the spacing between loggers. Improved.

Line 126: Please be careful in the order you reference figures. So far, you have only referred to Fig. 1, so this should be Figure 2.

Reference removed.

Line 126: I'm confused by the choice of words here and before. Is logger equal to a sensor? To me a logger is just the device that records data. But this can be separate from the actual sensor.

We improved the text with clarifying sentence.
"For clarification, the term "logger" in this article denotes the entire instrument, comprising both the recording unit and the measuring sensor."

Line 139: I would be more clear here...the error can be modelled by a linear function.

Thanks for the suggestion. Improved.

Line 141: From this, it is completely unclear which approach you took here.

This is clearly stated in Line 220: "For the following analysis, we will use the error estimation from Slater et al. (2000)". The choice of which model to use is a result of our analysis and, therefore, is included in the Results part.

Line 148: Also here, from this description I don't understand how you clustered the data. How many clusters did you actually use, and did you cluster each acquisition separately, or did you cluster the timelapse results, like in Delforge et al. (2021)

Delforge, D., Watlet, A., Kaufmann, O., van Camp, M., & Vanclooster, M. (2021). Time-series clustering approaches for subsurface zonation and hydrofacies detection using a real time-lapse electrical resistivity dataset. Journal of Applied Geophysics, 184, 104203. https://doi.org/10.1016/j.jappgeo.2020.104203

We added clarity to this part with the following sentence.

We used five cluster classes, based on the elbow method (Cimpoiasu et al. 2025), which evaluates the sum of squared distances between samples and their nearest cluster center as a function of the number of clusters. The optimal number is identified at the elbow of the resulting curve.

We clustered all the results together, as suggested by Delforge et al (2011). This reference was included in the paper

Chapter 3.1: This section needs restructuring. What information do you want to convey here? There is a lot of information here without much detail and it is unclear what is actually important.

We have significantly restructured chapter 3.1, improving the chapter's order, reducing the amount of information, and moving the calibration/validation into a new chapter.

Line 156: I believe in climate change in thawing permafrost, but from this timeseries, I'm not convinced that you see that here. From 2019 to 2021, you have a clear decrease in ALT, while in 2022 and 2023 there is an increase. But what this timeseries shows to me is "just" a strong variability in ALT that may have a strong connection to snow thickness and/or air temperature.

Rock permafrost results are strongly dependent on local variations in snow regime and solar radiation. The importance of the Zugspitze is that it provides a unique representation of the northern Alps with high precipitation (2000 mm/a) and an intense snow regime, as opposed to the conditions in the inner alpine dry zone (600-1000 mm/a), and its value stems from the comparison with the more frequent observations there. The aim of this chapter is not to prove global climate change with 5 years of data, but simply to present temperature results. The reviewer correctly interpreted our message: there was a clear decrease in ALT, with some strong variability. This is only a little part of the whole message, and a complete interpretation is offered in the discussion, where other contributions help to answer our research questions.

Line 162: Why is so much information placed in the supplementary information?

Because of the large amount of information available in this long-term study, we had to move the secondary information to the supplementary material. This allows the general reader to focus on the paper's main messages while providing detailed information, secondary analysis, and proofs of the results for the interested reader. E.g., the parameters used to measure are not decisive for understanding permafrost dynamics, but they are definitely important for those who want to repeat the study.

Line 162: This is an analysis that would be great to show in the main part of the paper.

As suggested, Figure S3 has been moved from the supplementary material to the main paper.

Line 173: Do the two measurements correspond to the same elevation. From Fig. 1 this is difficult to say.

This information is presented in the introduction (Line 65) and in section 2.2 (Line 96); for clarity and reader convenience, we have repeated it here. We sincerely thank the reviewer for this helpful and constructive suggestion.

Figure 3: Resistivity variations derived from what and where? Is this just the mean/median resistivity from the inverted resistivity models, or mean apparent resistivity?

Removed “variation”. We present here single-quadrupole measurements prior to inversion.

The caption and part of the chapter “data analysis” have been improved for clarity: “The T-rho field calibration (Fig.3 and 4) was conducted using the same temperature values, along with single rho values extracted from the raw data, selecting the closest quadrupole to the temperature logger…”

Line 187: Without looking at the figure, it is unclear what you mean here. I assume that you mean variations along the profile that lead to a large spread?

We added a sentence for clarification. Resistivity varies from 30 to 120 kΩm for ST-10, from 15 to 25 kΩm for ST-15, and from 40 to 130 kΩm for ST-20.

Line 188: How did you extract the resistivity from the resistivity model? Is it a single cell at the location of the temperature sensor, or a set of cells? If so, in which radius around the sensor?

Single-quadrupole measurements prior to inversion at the location of the sensor, within a < 0.5 m radius.

Chapter 3.2: What error model is used in the previous analysis? I would assume the updated analysis. In this case, this section should be placed before the previous.

> The data presented here are raw, prior to any inversion, and as such, no error model has been applied. We believe, therefore, that the current chapter order follows a logical progression and should not be altered.

Line 239: Rather than showing them in Fig. 8, I would perhaps show them in Fig 7. Also make clear you indicate again in the caption which feature the letters correspond to.

> The Subfigure was moved to Figure 7. The feature explanations have been added.

Line 255: What is the rational for clustering here? Coming back to a comment I posted earlier, how are you clustering? Is this clustering of each single tomogram, or all of them as a time-series?

> Line 149 of the preprint: "The function analyzed all tomograms from 2014 to 2023"
>
> We added the methodology: *"We used five cluster classes, based on the elbow method \citep{Cimpoiasu2025}, which evaluates the sum of squared distances between samples and their nearest cluster center as a function of the number of clusters. The optimal number is identified at the elbow of the resulting curve."*

Line 265: I would like to see a better uncertainty analysis of the estimated volumes here. Simple K-means doesn't allow for that, but you could use Fuzzy C-means clustering (or a whole range of other approaches) to get a probability to each cluster assignment. That would help you to not just provide a value, but also an uncertainty of these values.

> This question strongly relates to major question nr. 4. Please refer to that answer to avoid repetitions.

Line 292: "*Newly available data, improvements in methods, and updated analyses have significantly enhanced the quantification of both short-term permafrost dynamics and long-term degradation."* If this is the goal of the paper, that should be made much clearer.

> We agree that the overall goal of the paper must be stated as clearly as possible. In our view, the abstract already explicitly communicates this objective, in particular in the final sentence: *"With these results, we demonstrate that ERT monitoring can yield high-quality **quantitative insights into long-term permafrost evolution** and **effectively track bedrock permafrost degradation across both decadal and seasonal timescales**".* Further on this is one of our research questions. We therefore believe that the central message is already clearly conveyed. See also the comment on major point 6.

Line 310: One point that I feel you completely neglect here is that most lab analysis focuses on fully saturated samples. How would a change in saturation change the resistivity, especially if you are not only considering electrolytic conduction, but also surface conduction?

And on a second note, when you take rock samples in particular, you take them out of their natural stress field, which may lead to small fracturing or fracture opening, which could also reduce the resistivity of the rock sample.

> The conditions we observe at the Zugspitze are always fully saturated (>90%), except for the outer 8-10 cm of the rock surface (Sass, 2005). In the specific setup where we

measure from an always moist gallery meters to mostly decameters inside the rock wall, surface variations do not play and important role (in comparison to measurements with electrodes on the surface).

Thanks for this very interesting comment. We added one sentence in Section 4.1.

Line 325: But you can automate this...

Certainly, data analysis can be automated, but the acquisition time still remains longer. In our case, this would require extending each field campaign by 2-3 hours. Due to logistical constraints this would necessitate the team staying overnight on the mountain. The phrase "during data acquisition" has been added to the sentence for clarity.

Line 327: This is not true. Koestel et al. 2008 used a time-varying error model.

Thank you for the correction. The sentence has been updated.

Line 329: What you don't report here is how the updated error model impacts the data misfit. This should be added.

We added a paragraph on that at the end of chapter 4.2.

Line 362: But is this really surprising? In Fig. 8 you are looking a binary change from frozen to unfrozen, while here you look at the change in resistivity.

Sentence has been rephrased for clarity.
"This confirms that non-linearly degrading processes are occurring in the active layer and that sudden degradation might occur in the next few years."

Line 372: This I don't follow. You have electrodes along the side tunnel, and you interpret transect A-A' as ALT to permafrost core. But really your resolution is primarily a function of distance to the electrodes, which is not changing here. So of course you have a better resolution to the permafrost core, but why a reduced accuracy to the shallow active layer?

You are right, the sentence has been rephrased for more clarity.
"The configuration employed here significantly enhances accuracy along the main tunnel (deep bedrock) and the side tunnel (permafrost core), but results in reduced resolution on the remaining sections of the outer bedrock slope."

Line 380: If you would link your resistivity observations to snowmelt and precipitation, maybe you could validate that?

Unfortunately, the snowmelt measurements were collected on the south slope, and Scandroglio et al. (2025) demonstrated significant differences that make extrapolation to the north side challenging. However, we still have documentation of the ice presence in the tunnel (Supplementary material).

Line 410: Exactly. Could you also show this here? You provide a detailed analysis of the rho-T relationship, but stop with that. Why not aiming to translate the resistivity into temperature? If you do that, you should also be clear on the limitations.

Krautblatter et al. (2010) provided insight into ERT-derived temperatures, but we observe deviations where higher degrees of fracturing occur in the rock. Therefore, we did not include it here.

<u>Literature: please refer to the list in the article.</u>