Reply to the comments of Referee #2

This manuscript investigates the contributions of hydrostatic and cryostatic processes to the observed electrical resistivity signal in high-alpine permafrost rockwalls using a comprehensive monitoring dataset. It is well structured and clearly written, with well-defined objectives and a meaningful contribution to the understanding of seasonal processes in permafrost rock slopes. The dataset and its analysis are comprehensive, thoroughly explained, and well-illustrated. I found the figures particularly informative and well annotated.

Understanding the drivers of slope instability in permafrost rockwalls is an important research topic, and this study provides a novel and valuable contribution to the field. Overall, the manuscript is of high quality and suitable for publication. I have only a few minor comments and corrections for the authors to consider.

Specific comments

- Perhaps the word "*preparing*" in reference to permafrost rock instabilities could be replaced by "*conditioning*" or "*priming*", as it sounds awkward in places (e.g lines 5, 32, 375)

  We replaced "*preparing*" with *"conditioning"* in all the relevant places (Line 5, 32, 376).

- How was the active layer measured? If it was defined as the depth of the 0°-degree isotherm which seems to be the case, this should be explicitly stated in the methods section

  The active layer thickness was defined as the depth of the 0 °C isotherm. We have now stated this explicitly in section 2.4: "*The active layer thickness was defined as the maximum depth of the 0 °C isotherm, derived from a spline interpolation of the borehole temperatures.*"

- Section 3.1 Data pre-processing: It would be helpful to report the contact resistance values (e.g., as a range, seasonal averages, or similar statistics), as it provides a useful reference for other winter ERT studies

  Thank you for the suggestion. We fully agree that contact resistance values would be helpful, especially for year-round measurements. Unfortunately, in our automated setup the contact resistance could not be recorded before each measurement due to technical constraints: if the values were too high, the system would abort the measurement routine, so we had to disable this electrode check to keep the monitoring running continuously. We therefore do not have contact resistance values for the monitoring period. However, we already reported values from earlier surveys at the same profile in section 5.3*: "While technical constraints in our system prevented electrode contact resistance checks before each measurement, earlier ERT surveys at the investigated rockwall indicated acceptable, low contact resistances (< 190 kΩ) in summer (Offer et al., 2025)."*

- I am a bit unclear about the choice of the **b parameter** in the error model. I find it a great strength of this study that reciprocal measurements were acquired, which is not always possible for permafrost ERT measurements. Considering that reciprocal analysis was performed, could the authors clarify why b = 0.08 was selected for all inversions? Based on Figure 2d, most values fall between 0 and 0.03, with only two outliers approaching 0.04-0.06. Would it be more appropriate to scale b based on measurement ID (perhaps binned into phases or seasons), as nicely illustrated in Figure 2d, rather than applying a single value to all measurements?

Thank you for pointing out the value of the reciprocal measurements (which are quite time-consuming). You are right that the fitted error parameter b_{rn} mostly falls between 0 and 0.03, with higher values during the freezing period (Fig. 2d). We chose a single fixed value of b_{rn}=8% for two reasons. First, previous ERT studies in permafrost environments have reported higher data uncertainties (e.g. Krautblatter et al., 2010; Bast et al., 2024), so we set b_{rn} to a conservative level that stays above our empirical errors across the whole time series and accounts for the sensitivity to high-resistivity conditions (Line 209-212). Second, we preferred a constant error over one scaled per seasonal phase: a time-varying error would apply different degrees of smoothing to the individual datasets, which could introduce or suppress resistivity contrasts between the seasonal phases. Since comparing resistivity changes across these seasonal phases is the main aim of the study, we wanted to avoid that. We added a sentence to section 3.2 to make this clearer: *"We preferred a constant error level rather than scaling b_{rn} per seasonal phase, to avoid applying different degrees of smoothing to the individual datasets, which could introduce or suppress resistivity contrasts between the seasonal phases."*

- Section 3.2 Inversion routines: This section could be expanded with a few more details on the inversion procedure. For example, it would be helpful to briefly describe the inversion method, the data and model norms used, and the choice of regularization.

  The time-lapse difference inversion following LaBrecque and Yang (2001) was already described, so we added the details that were missing on the inversion method, the norm, and the data treatment: "*The inversion follows a regularised, smoothness-constrained (Occam-type) least-squares scheme, minimising an objective function that combines the data misfit with a model roughness term, both evaluated in the L2 norm. The data were log-transformed and weighted according to the reciprocal error model (Eq. 1).*"

- Lines 296-297: The authors write: "*Almost the entire ERT model exhibited values below 10 kΩm, except for a localised zone near the surface (~5 m depth) in the upper part of the profile (x = 0-18 m)*". I am curious about another localized zone of higher resistivities, around the borehole location. Since this phase corresponds to the deepest active layer, I would have expected the active layer to be more clearly resolved in the inverted tomogram, particularly around the borehole location. Do the authors have any thoughts on why this is not more apparent, and the shallow near-surface region shows higher resistivities instead?

  Thank you for this observation; actually, it touches on one of the central findings of the study. Around borehole B2 specifically, the electrodes are likely placed on a disintegrated rock outcrop that dries and freezes faster than the surrounding rock. This is already visible in phase III but becomes strongest during superficial cooling, which is why we explain it in the phase IV discussion (lines 359–364): "*The discrepancy in phase IV is likely due to electrode placement on a disintegrated rock outcrop (Fig. 3), where near-surface rock is subject to more rapid drying and freezing. This interpretation is supported by a comparison of apparent resistivity values involving electrodes E22/E23 near the borehole and equivalent electrode spacing at the profile centre (Fig. 7a), which shows substantial deviations during superficial cooling (phase IV). Thus, near-surface resistivity appears to be more strongly influenced by rapid changes in conditions (e.g. saturation, drying, or freezing) associated with local rock surface heterogeneity, while rock temperature responses are delayed and moderated due to thermal damping.*"

We would normally expect a conductive active layer sitting on a resistive frozen permafrost body, so a low-over-high resistivity structure. Around B2 we see the opposite: the near-surface disintegrated outcrop dries and freezes quickly and therefore appears comparatively resistive, while at greater depth the permafrost body does not stay resistive, because liquid water percolates into deep-reaching fractures and is pressurised into a wide-reaching discontinuity network, keeping this zone conductive even where borehole temperatures still indicate frozen conditions. We summarise both effects in the caption of Fig. 6: "*deviations near the surface are attributed to likely high electrode contact resistance caused by surface drying or cooling of a disintegrated rock block, whereas high discrepancies at greater depths (2.5–4.5 m) coincide with periods of pressurised water flow*." We would like to emphasise that this high-over-low structure is a local phenomenon confined to the immediate vicinity of borehole B2. Across the rest of the profile, the widespread low resistivity described for phase III dominates.

- **Figure 4** provides interesting historical anchor load data, and I understand that it is included to show how typical the observed seasonal pattern is. However, since the analysis and discussion focus mainly on the 2024–2025 observations, I wonder whether **Figure 5b** might be sufficient for the main text, and **Figure 4** could be moved to the Appendix

  Thank you for the suggestion. It is right that the main analysis focuses on the 2024-2025 observations. Therefore, as suggested, we moved Figure 4 to the appendix (now Fig. A3) and kept a reference to it in the main text, so the multi-year context still shows that the observed seasonal anchor-load pattern is representative of previous years.

- In the lower right corner of **Figure 6**, some of the colours used to depict the depth sections are repeated in the circular phase plot. This may be slightly confusing for the readers, as the same colours represent different information. Have the authors considered simplifying the colouring of the depth sections? In my view, a single colour or graduated shading would be sufficient, especially as the depths are already labelled next to the corresponding boxes.

  We agree that using the same colours for the depth sections and the phase plot (legend) is confusing. We changed the depth sections (now in Fig. 5) to a graduated brown shading.

Technical corrections

- Lines 10-12: The authors write: "*The ERT data set was combined with observations of the rockwall's hydro-mechanical response derived from load cells of two 25 m-long anchors and from piezometric measurements at 16.85 m depth*". To me, this wording gives the impression that the load cell and piezometric measurements are somehow directly integrated into the ERT inversion. Since these measurements are only used to help interpret the resistivity response, I would suggest rewording this statement to make that distinction clear.

  We agree that the sentence could imply a direct integration into the ERT inversion. We have replaced "*combined with*" by "*complemented by*" to clarify that the anchor load and piezometric measurements are independent observations (and only used to support the ERT interpretation).

- Lines 29-33: The authors write: "*Despite the control exerted by the geological structure (Stead and Wolter, 2015), frost weathering- and erosion-driven fracture propagation (Matsuoka and Murton, 2008; Mayer et al., 2024), long-term seismic loading (Gischig et* *al., 2016), glacial changes (Hartmeyer et al., 2020a; Pfluger et al., 2025) and permafrost degradation (Gruber and Haeberli, 2007; Savi et al., 2021), hydrostatic and cryostatic pressure play a crucial role in preparing planes of weakness."* I am not sure that despite is the right word here. In my view, hydrostatic and cryostatic pressures are additional factors contributing to slope destabilization, rather than contrasting ones to the controls listed above. Perhaps the sentence could be reworded to reflect this more clearly?

We agree that "*despite*" implies a contrast that is not intended here. We replaced it with "*In addition to*".

- Line 69: in the sentence starting with "*Therefore, we here conducted daily…*" I suggest to either remove "here" altogether, or move it before the "we"

  We deleted "*here*".

- Lines 101-103: The authors write: "*Numerous open fractures, also observed along the ERT profil (Keuschnig et al., 2017), are filled with fine-grained material that promotes water infiltration and thus influences the hydrostatic and thermal dynamics of the rock mass*". A brief explanation of this influence would be helpful, rather than just stating that an influence exists.

  We have reworded the sentence: "*Numerous open fractures, also observed along the ERT profile (Keuschnig et al., 2017), act as preferential pathways for water infiltration and are partly filled with fine-grained material that retains water within the discontinuities.*" Since this sentence belongs to the geological setting (section 2.1), we want to keep it descriptive here and discuss its effects on the hydrostatic and thermal dynamics later in the discussion (section 5.1).

- Line 102: typo in the word "*profile*", missing "*e*" at the end

  Changed.

- Lines 116 - 119: Consider adding a reference for the statement of Wenner configuration being associated with the maximal signal strength

  We have added the reference Hauck and Vonder Mühll (2003) to support the statement.

  Hauck, C. and Mühll, D. V.: Inversion and interpretation of two–dimensional geoelectrical measurements for detecting permafrost in mountainous regions, Permafrost and Periglac. Process., 14, 305–318, https://doi.org/10.1002/ppp.462, 2003.

- Line 124: Consider adding an "*and*": friction-locked and grouted

  Added an "*and*".

- Line 188: I would remove the word "*technical*" before "*filtered data sets*"

  Changed.

- Line 189: Suggests, instead of suggest

  Changed.

- Line 226: Is the statement that "*fractures were predominantly ice-filled*" based on the interpretation of the ground temperature data? Perhaps a brief clarification could be helpful here, as some readers may interpret this as a more direct measurement of unfrozen water content.

The statement is based on the borehole temperature data, which stayed below -0.5 °C throughout this period. We reworded the sentence to *"suggesting that fractures were predominantly ice-filled"* so it reads as an interpretation of the rock temperature data rather than a direct measurement.

- Line 316: The authors write: "*...During phase I (stable freezing), high resistivity values (> 19 kΩm, decisive for frozen conditions (Offer et al., 2025))* ...". I find the wording slightly awkward here. Perhaps *indicative* of frozen conditions (or *characteristic of, associated with*, etc.) would be more appropriate than *decisive* for frozen conditions.

  As suggested, we replaced "*decisive for frozen conditions"* with *"indicative for frozen conditions"*.

- Line 375: Similarly, here, the phrase "*decisive in preparing*" reads slightly awkward to me. Perhaps consider rephrasing it using an alternative.

  We rephrased "*decisive in preparing"* to *"which contribute to the conditioning of"*.

- Lines 386-387: The authors write: "*the pattern of enhanced subsurface electrical conductivity during snowmelt is also reported in other automated monitoring studies (Hilbich et al., 2011; Kneisel et al., 2014)*". I would add "*in similar settings*" or an alternative, as "*other monitoring studies*" is rather broad

  We added "*in similar settings"*.

We *thank Referee #2 for the thorough and thoughtful review. The comments were very helpful and improved the clarity of the manuscript.*