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The Cryosphere
Dr. Teddi Herring

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Author‘s response (egusphere-2024-893)

Dear Dr. Teddi Herring,

Thank you for your positive feedback on our revised manuscript (egusphere-2024-893) entitled “*Pressurised water flow in fractured permafrost rocks revealed by borehole temperature, electrical resistivity tomography, and piezometer pressure”,* which requires only minor revisions.
We have carefully answered all the comments from the two reviewers and slightly changed the manuscript. Please find attached our detailed point-by-point responses and our suggested changes.

We look forward to hearing from you.

Kind regards,

Maike Offer (on behalf of all authors)

Reply to Referee #3

Dear Referee #3,
Thank you for taking the time to review our revised manuscript entitled *"Pressurised water flow in fractured permafrost rocks revealed by borehole temperature, electrical resistivity tomography, and piezometric pressure" (egusphere-2024-893)* and acknowledging the difficult field work conditions in high-alpine environments. We greatly appreciate your positive feedback with only minor revisions. Our detailed point-by-point responses are given below, highlighted in blue, with proposed changes indicated in bold. Please note that the lines mentioned in our replies correspond to the second version of the revised manuscript.

Sincerely,
Maike Offer (on behalf of all authors)

The manuscript I have received was a revised version, accompanied by a detailed explanation of changes. This revision has been done carefully, and it is clear that the authors respond to the concerns of the reviewers. The study is after my opinion a timely and interesting case study demonstrating water flow in fractured bedrock affected by permafrost in a high-alpine setting. As far as I know, these data are novel and unique, and extremely difficult to obtain. I see the several shortcomings addressed by the reviewers related to different dates, instruments and inversion procedures, which would probably kill a study under very simple and controlled conditions. Here, we have a high-alpine environment, where such shortcomings have to be accepted.
So, in summary, I would recommend publication of the paper, after some minor revisions (see below). The revised manuscript reads well and is well structured. The figures are illustrative, and I think the study advances our knowledge on high-mountain permafrost processes in a time of global climate change. I have gathered some comments below, which you many address:

Abstract: The abstract is quite long and detailed and would benefit from making it a bit shorter.
(A1) We made the abstract a bit shorter and more readable.

Introduction: Consider deleting (or move somewhere else) the last sentences in the Introduction (from line 76), this is a sort of conclusion.
(A2) The last sentences were adapted and added in the first round of revisions in response to a comment from Referee#1 for a clearer articulation of the specific objectivities of our research (see our previous response, (A4)). To address the concerns of Referee#1, we propose keeping these sentences as they currently appear in the Introduction.

Chapter 3.2. This is a detailed and wordy description of the ERT, and the inversion process. Just give fundamentals and cite relevant literature. If you think this much important information, consider making a paragraph in the appendix.

(A3) To ensure that readers with varying backgrounds in ERT can benefit from our research, we initially included a detailed description of the ERT basics and inversion process. However, we agree that this level of detail might detract from the focus of the manuscript. Following your suggestion, we have moved this paragraph in the appendix.

Chapter 4.1. You may consider to provide some of data in the first paragraph of the chapter in a table. Much easier to understand.
(A4) We agree that the first paragraph of subchapter 4.1 contains many numerical details that might be challenging to follow. Rather than providing a new table, which would duplicate information already in Fig. 4, we have modified Fig. 4 to include the additional details from this paragraph (active layer thickness, depth of zero annual amplitude, thermal offset, and depth of 0 °C on the days of ERT measurements in 2023).

l. 258: This sentence should come later? ERT results not yet introduced here.
(A5) Thanks for your comment. We have retained the first part of the sentence in subchapter *4.1 Borehole temperature and thermal anomalies* ("All abrupt changes and irregularities in the borehole temperature occurred between late May and September.") but moved the context of the second part to subchapter *4.2 Seasonal variations in ERT* ("The most pronounced seasonal variation in electrical resistivity occurs in summer (Fig. B1), **coinciding with all abrupt changes and irregularities in borehole temperature (Fig. 7).**")

What is a "quasi-sinusoidal"
(A6) The term "quasi-sinusoidal" describes a pattern that closely resembles, but does not strictly conform to, a true sinusoidal curve. In the context of borehole temperature data, this term often refers to a repeating wave-like temperature pattern that approximates seasonal temperature cycles. As the term is already used in several studies, e.g. Hauck (2002), we decided not to further describe the term in the manuscript.

Hauck, C., Frozen ground monitoring using DC resistivity tomography, Geophys. Res. Lett., 29(21), 2016, doi:10.1029/2002GL014995, 2002.

Figure 5: make sure that the text in the figure is readable. Now for me just with a magnifying glass 😊
(A7) We have increased the size of the text in Fig. 5.

Chapter 4.3.: Is quite misplaced after the ERT results description. This calibration could be part of the Method-section under the ERT explanation, this is a procedure you do to get your results interpreted. Alternative, before the ERT result description.
(A8) In the first version of the submitted manuscript, the chapter of the laboratory calibration was presented as the first subchapter in both the methods and results. However, in the first round of revisions, Referee#2 suggested, "*I would have first presented the field before the laboratory calibration approach as the latter completes the former*" (see our previous response, (A6)). To address this concern, we restructured the subchapters to begin with

borehole temperature observations, followed by the field ERT measurements and concluding with laboratory calibrations. To maintain alignment with the comments of Referee#2, we suggest to retaining the current order of the subchapters.

Discussion: The chapter 5.2. about limitation should start the discussion. Now you discuss the results, then you say there are limitation, then you discuss further the results. So, move this to the start of the discussion. Check also for redundancies, a bit wordy the whole paragraph. The limitation is longer than the rest of the discussion, show clearly that the limitations do not hamper the value of the results.
(A9) Thank you for your suggestion. We believe that it is important to be transparent about limitations, but we think that starting with the study results will better capture the interest of the reader, especially since our focus is on the novel observation of pressurised water flow in permafrost rocks. We agree that the limitations section is detailed because of the range of measurement techniques we used – borehole temperature, electrical resistivity tomography, laboratory calibration, and piezometric pressure – all of which have uncertainties, especially in high-alpine environments. This section has been expanded in response to previous comments from reviewers and the editor to address uncertainties (e.g., different instruments and measurement years, atmospheric conditions, high contact resistance) in the measurements. To the best of our knowledge, we have highlighted all relevant limitations and explained why their impact on our results is minimal or why certain datasets (ERT 2013) have been excluded from the interpretation.

Conclusions: Consider deleting the last paragraph, does not give any new information.
(A10) We agree that the last paragraph of the conclusion does introduce new information; it was intended to outline future research steps in rockwall hydrology and to serve as a bridge to an upcoming manuscript. Therefore, we would like to keep this paragraph.

l. 319: change “paper” with “study”
(A11) →changed as suggested.

Fig. 8 caption: What do you mean with “this moving mean air temperature”, who “moving”, moving average? Over what period?
(A12) By “moving mean”, we indeed refer to a “moving average”, which calculates the mean over a sliding window of data points, moving incrementally over the dataset. We here used the definition of MATLAB.
In our case, as indicated in the caption of Fig. 8, we calculated the moving mean air temperature with a 2-hour sliding window. To further clarify, the air temperature data from the weather station was recorded at 10-minute intervals (as now included in subchapter 4.4: “The rapid rise in piezometric pressure correlates with days when the mean air temperature was above 0 °C, as measured at the weather station **in 10-minute intervals** on the nearby Gletscher Plateau (2.940 m asl).”

l. 334: why “inconcistent with Archie’s law”?

(A13) The explanation why the massive decline in electrical resistivity from June to September is inconsistent with Archie’s Law, unless pressurised water flow is assumed, is described from Line 325-330. We have slightly revised one sentence for clarity: “In thawed conditions, resistivity decreases for various rock types at a rate of ∼ 2.9 ± 0.3 %/°C (Krautblatter, 2009), and according to our laboratory calibrations, by 4.5 ± 0.3 %/°C (Fig. 3, Table D1). Thus, **and considering Archie’s Law,** a temperature warming from July to September (Fig. 9) in already fully saturated rock with constant porosity would not cause a significant further and rapid electrical resistivity decline. This can only occur if pressurised water flow contributes to additional hydraulic opening of fractures within days to weeks.”

Reply to Referee #2

Dear Referee #2,

Thank you for reviewing of our revised manuscript *"Pressurised water flow in fractured permafrost rocks revealed by borehole temperature, electrical resistivity tomography, and piezometric pressure" (egusphere-2024-893)* and for the positive feedback indicating only minor revisions. Below, we provide detailed responses to your comments, highlighted in blue, with proposed changes in the revised manuscript indicated in bold. Please note that the lines mentioned in our replies correspond to the second version of the revised manuscript.

With kind regards,
Maike Offer (on behalf of all authors)

This second version of the paper now entitled "Pressurised water flow in fractured permafrost rocks revealed by borehole temperature, electrical resistivity tomography, and piezometric pressure" has adequately addressed the review's comment. Adding the piezometric data provides additional evidence and makes the paper more comprehensive. In its current form, it presents an interesting and unique combination of data showcasing a significant effort in integrating them despite their relative limitations. Additionally, the quality of the graphs is highly satisfactory. I now recommend this paper for publication after minor revisions. Minor comments:

- I would suggest adding the depth at which the "abrupt temperature changes are observed" in the abstract

(A1) Changed to: "[…] They further show abrupt temperature changes (~ 0.2-0.7 °C) **at 2, 3, and 5 m depth** during periods with enhanced water flow and temperature regime changes between 2016-2019 and 2020-2022 **at 10 and 15 m depth** […]"

- L42-43: this reference might also be interesting to complete the state of the art: Hugentobler, M., Loew, S., Aaron, J., Roques, C., and Oestreicher, N.: Borehole monitoring of thermo-hydro-mechanical rock slope processes adjacent to an actively retreating glacier, Geomorphology, 362, 107190, https://doi.org/10.1016/j.geomorph.2020.107190, 2020.

(A2) Hugentobler et al. (2020) provide valuable insights into thermo-hydro-mechanical processes on a rock slope affected by glacial retreat, monitored through deformation, groundwater pressure and temperature in three boreholes. However, the borehole measurements in this study indicate the absence of permafrost in the investigated slope. In the revised manuscript, especially in L39-44, we have focused on observations of piezometric pressures specifically in permafrost-affected regions. Therefore, we suggest not including the suggested reference in the introduction, but to clarify the knowledge gap: "[…] piezometric pressures have only been observed **in permafrost regions** on one rock glacier (Phillips et al., 2023; Bast et al., 2024) and in one open crack at shallow depth (Draebing et al., 2017). Direct observations of piezometric pressures in deep depths (> 10 m) have not yet been measured in permafrost-affected rockwalls, but remain crucial for understanding hydrological processes and thus for the prospective prediction of rock slope failures."

- L94: did you mean "installed in 2012"?

(A3) Thanks for identifying this mistake. We have corrected it to: **"[…] installed in 2012**".

- L241: this sentence is a little bit confusing: did you interpolate borehole temperature from ERT measurements?? Or did you mean temperature measurements during ERT measurements.

(A4) We here refer to borehole temperature on the days when ERT measurements were taken. To clarify, we have revised the sentence to: "**Interpolated borehole temperature recorded on the days of the ERT measurements in 2023** […]".

- L260: figure 7 is cited before figure 6

(A5) As we moved the corresponding sentences to subchapter 4.2 (see our response (A4) to Referee #3), Fig. 6 is now cited before Fig. 7.

- L269: I would rather say "pluriannual changes" rather than "long term" as it is < 5-year trend

(A6) Changed as suggested.

- L275, Fig. 6: the paper should be checked by a native speaker. Writing "in" 10 m depth sounds not appropriate to me. Wouldn't it be "at"?

(A7) We modified the caption of Fig. 6 and L264 to: "[…] **at** 10 m depth", and "[…] in July at 10 m depth […] and in September **at** 15 m depth […]".

Finally, I would recommend thoroughly reviewing the references, as some are not used appropriately: e.g. L247, Gruber et al., 2004 is not the paper that defines the DZAA; L340, Noetzli et al., 2007 doesn't address non-conductive heat transfers; L359: Magnin and Josnin, 2021, doesn't use ERT measurements. Those are only a few examples, but all references should be thoroughly reviewed.

(A8) We acknowledge that Gruber et al. (2004) refers to the depth of zero annual amplitude but does not define it. Since this term is commonly used within the permafrost community, we have removed the citation.

Noetzli et al. (2007) was cited to support that rapid changes in rock temperature cannot be explained solely by diffusive heat exchange (L330-332). This study demonstrates that temperature signals based solely on heat conduction take considerable time to penetrate deeper into a ridge with a north-south orientation, like the Kitzsteinhorn. Therefore, the rapid rock temperature changes cannot be explained by the models presented in Noetzli et al. (2007).

Magnin and Josnin (2021) was incorrectly cited twice in the same sentence. We corrected this to: "[…] as already shown by geophysical measurements (Krautblatter and Hauck, 2007; Krautblatter et al., 2010; Keuschnig et al., 2017) and by numerical approaches (Magnin and Josnin, 2021) […]".

We carefully reviewed all other references but could not find further mistakes.