Author comment:

Dear Victor Pozsgay,

Thank you very much for your community comment on our manuscript titled *"Pressurised water flow in fractured permafrost rocks revealed by joint electrical resistivity monitoring and borehole temperature analysis" (egusphere-2024-893).* We sincerely appreciate your constructive and positive feedback. We have provided general responses to your community comments in blue. However, we encourage you to also review our detailed responses to the reviewer comments, as some of your concerns are addressed there more thoroughly.

With kind regards,

Maike Offer, Samuel Weber, Michael Krautblatter, Ingo Hartmeyer, and Markus Keuschnig

I am a postdoctoral fellow with growing expertise in numerical simulations of permafrost ground in mountain areas and slope failures. I have a background in theoretical physics and am relatively new to the field hence why this comment focuses mainly on scientific methods and data selection, and should be taken with a pinch of salt. It has been a pleasure to read this manuscript and I hope that sharing the following comments will be useful.

The abstract and introduction convey well-written and well-referenced information allowing the reader to understand the context and the interest of the study. However, I believe that the overall scientific methodology could be improved. For instance, conclusions are reached about the timing of the infiltration relative to snow melt and the absence of precipitation in the 'days' preceding measurements but no attempts to consistently measure snow cover or precipitation were made. The influence of the air temperature on snowmelt and on the whole infiltration process is also essential, but once again, the reader does not have access to it. Towards the end, the authors briefly assert that piezometric measurements were made and supported their hypothesis, but neither the method nor the results are reported. In my opinion, the manuscript would be stronger if more supporting evidence was presented to the reader.

Thanks again for your feedback. In the revised manuscript we will provide a weather analysis during the observation period from 2013-2023 (see response A4 for Referee 2). In addition, we will demonstrate the piezometric measurements and describe its methodology and results in detail. Please consider our detailed response to Referee 1 (A2) and 2 (A13) about the piezometric observations.

Beyond this, the major issue that I have with this manuscript lies in the ERT dataset selection. Due to some lightning strikes, most ERT measurements between June and September 2013 were corrupted and the authors decided to fill the gap with data from 2023, 10 years after the original measurements. The authors are comparing monthly ERT measurements coming from two sets of measurements spaced by 10 years, and do not address the issues created by such a significant temporal gap. As they correctly put it in their introduction, the rise of temperatures and the permafrost degradation have accelerated in the last decade, and there is little reason to believe that the study site has not been affected too. In fact, it is clear from Figure 4 that the resistivity of the bedrock along the survey line has changed tremendously between 2013 and 2023 during the months of June and September (the only months measured both in 2013 and 2023). Visually, the most impressive difference comes from September where the resistivity of the whole cross section is about 2 orders of magnitude smaller in 2023 than in 2013. Finally, when looking at the measurement dates in Table B1, I find it surprising that most are taken within the last week of the month but for some unknown reason (which could be technical, but it is not communicated), the October 2013 data was measured on the 8th, which is not

consistent with other data points. Given these comments, it is hard to justify treating the 2013 and 2023 months on an equal basis which is why I believe that the authors could improve the overall readability by sharing their reasoning behind choosing this particular dataset. It would be interesting to know if they are aiming at studying inter-annual or solely seasonal variability, in which case they would probably need to justify why they look at data taken 10 years apart. However, having such data could still be a strength if more was said about the evolution of some metrics over this decade.

The selection of the ERT dataset will be explained more explicit in the revised manuscript. As this was also addressed by Referee 2, we include here our response (A12):

Since the novelty of our manuscript is to reveal pressurised water flow during the thawing period, we refrained from a detail interpretation of the decadal permafrost change. Our initial motivation to include the data from 2013 was to cover all seasons, including the frost period, which was logistically not feasible in 2023, but would probably have yielded comparable results. We will include a short paragraph in the discussion section 5.3 to clarify our choice of ERT data and suggest to refraining from a detailed interpretation of the decadal permafrost change, since we did not present ERT measurements from the same seasonal periods nor a decadal recording of borehole temperature or piezometric data (Figure R1):

(Line 324): *"[…] Consequently, we refrained from detailed interpretation of the inverted tomograms from February to May 2013 and September to December* ***2013 but included the ERT data to cover all season. We assume that a repeat of the ERT measurements in 2023 during the frost period would have yielded comparable results, as borehole temperature show frozen subsurface conditions from January to June and from October to December (Figure 4), during which no thermal anomalies or irregularities were observed (Figure 6).***"

The differences in the ERT results from September in 2013 and 2023 is likely driven by varying atmospheric conditions before the respective measurements. This was also already responded thoroughly in our response to Referee 2 (A12):

Line 320): ***"Atmospheric conditions vary slightly between years, affecting the timing of snow melt and hence the change in resistivity regime. The ERT results from June 2013 exhibit higher resistivity values in the upper part of the profile (>64 kΩm) compared to the ERT measurement in 2023 (Figure 4), probably due to colder rock and atmospheric conditions prior to the measurement. However, the trend towards lower resistivity values, particularly in the lower part of the profile, is also clearly visible in the tomogram from June 2013.*** *The penetration depth of the current flow into the subsurface depends on the characteristics of the top layer and may vary seasonally. Dry and frozen conditions can impede current flow, while water-saturated conditions might trap current flow, resulting in an attenuated current flow into deeper layers (Loke, 2022). Poor electrode coupling is often associated with frozen conditions and ice-filled fractures and cracks from autumn to spring, which cause noisy data and can lead to inversion artifacts represented by a high RMS error.* ***This phenomenon was observed, for example, in September 2013 vs. 2023, where cold air temperatures likely caused freezing of the rock surface layer prior to the ERT measurement, resulting in high contact resistance and an inability to resolve the long-lasting summer thermal signal in greater depths."***

The decision to include the ERT measurement from October 8th was based on technical reasons. However, we believe that ERT observations at the end of October would have yield comparable results. This assumption based on the marginal differences observed in the ERT tomograms from September to December 2013 (Figure 4). Since we refrained from a detailed interpretation of the ERT measurements during winter due to the potentially reduced data quality (see Line 157-160), we did not further scrutinize the slight variation in observation dates during the frozen season.

Finally, the strength of combining the ERT measurements with borehole temperature data is precisely to be able to produce a plot like Figure 7b, providing some elements of proof of the presence of pressurized water flow. To me, this is the main message of the paper, and I believe it goes slightly unnoticed in the current layout. I would suggest emphasizing this result and providing more explanation of the processes at hand and the reasoning underpinning the conclusion.

The primary message of our manuscript is the detection of pressurised fluid flow during the thawing season. To enhance this point, we will incorporate new piezometric measurements, which will underpin our findings of the borehole temperature and ERT analysis. Additionally, we will slightly modify the conclusion (see A15 in the response to Referee 2) to underscore our main message of the paper.

Overall comments on Figures and Tables:

- The axis labels are not centered, and not capitalized.
  Since the journal does not specify guidelines for axis label formatting, we have chosen our own layout.

- The Tables include some repetitions in the units, some confusing symbols, and some labels not previously introduced.
  We have carefully reviewed the Tables and will remove the repetition of the unit in Table B1 (500**V**) and explain the parameter of the bilinear relation in Table A1 (**a+by**). However, we could not identify confusing symbols as we used only common declarations of parameters.

- Some text should accompany the Figures and Tables of the Appendix.
  The Figures and Tables in the Appendix are either explained in detail within the corresponding paragraphs of the main text or have their findings described directly in the Appendix.

- Not all Figures are referenced, and the order of the Figures in the Appendix does not represent their reference order from the main text.
  We will include the reference to Figure B3 in the main text and modify the order of the Figures in the Appendix to place Table B1 before Table A1.

Some extra comments:

- Line 100: Is there a particular reason behind this choice of diameter? Could you comment on how the relation could potentially change with a different diameter?
  The choice of diameter was based on technical constrains imposed by the drill bits used for extracting the core samples. However, we assume that using a different diameter of the core samples for the laboratory calibrations would not have yielded to significant different temperature-resistivity relationships.

- Line 143: What about the weather conditions in 2013? I believe it would be interesting to present some weather data in a table, say more about the air temperature, talk about precipitation, snow etc.
  See comment above.

- Line 163: The ERT doesn't give any information at depth below x = 0m, so could you please clarify why you decided to place B1 at the beginning of the survey line? A short sentence motivating the geometry of the survey would be interesting for the reader.

  The initial placement of boreholes was primarily intended for monitoring rock temperatures and was determined independently from the ERT monitoring. However, since both boreholes were drilled along the profile, we used them to describe the temperature regime of the investigated rock wall and, specifically, borehole B2 to directly link the resistivity values with rock temperature.

- Line 191 / Paragraph 4.2: In relation to previous comments, it might be interesting and even needed to add a paragraph studying the inter-annual variations.
  See comment above.

- Line 232: From Fig. 6, it seems to me that thermal anomalies are identified with thermal rate of change as low as 10^{-3} °C/10min. This corresponds to a difference of 1.2x10^{-2} °C over an averaging window of 2h, which is an order of magnitude less than the claimed threshold of ~0.2°C over that same period above which heat transfer becomes non-conductive. Could you please provide more information here and clarify the agreement between the Figure and these statements?
  We will modify the sentence to: *"[…] which exhibit a temperature rise of* ***up to 0.7 °C in less than 2 hours".***

- Figure 6 / B1 / 15m: It is mentioned that there are 'notable changes in the quasi-sinusoidal pattern since 2020' but I believe the reader would benefit from an explanation of the underlying cause for such a change.
  Please consider the detailed response to Referee 2 (A13).

- Line 273: It is surprising to read this sentence about the piezoemetric measurements without context. I would kindly suggest that the authors add some context and most importantly, present some data.
  See comment above and consider our detailed response to Referee 1 (A2) and 2 (A13) about the piezometric observations.