Dear reviewer,

we are very grateful for the numerous comments, questions, and suggested corrections. We agree with almost all of the comments and have revised the manuscript accordingly. In particular, the major comments made by both reviewers, which aimed to clarify the research question and the resulting structure of the article, as well as the requested addition of flow field information have been implemented, and the manuscript has been amended/supplemented accordingly.

Detailed responses and comments on the respective reviewer comments can be found below:

Review No. 2:

RC: Dear authors

Dear editor

This paper combines ground-based ERT and satellite-based InSAR surface displacement to relate permafrost properties (of which ERT-derived electrical resistivity is a proxy) to surface deformation (vertical and horizontal displacement, summer-time seasonality) on (periglacial) rock glaciers and (glacial) thrust moraine complexes. The authors show and discuss how the two landform types have dissimilar "fingerprints" (electrical resistivity and displacement pattern) in their two study areas and propose that their combined ground-based geophysical and remote-sensing DInSAR surface displacement provides a framework to distinguish between these two landform types.

*I found the measurements and chosen approach overall convincing and suited for this journal. My concern is only the unclear message or "thrust" of this paper. As reviewer #1 already mentioned, the overall framing is vague. The paper starts as a process-oriented case study (L13: "...aiming to determine *how* subsurface ice content and structure influence recent surface displacements [at the two field sites]") but ends with an outlook on "DInSAR-derived movement patterns can be used to identify ice-rich glacial or periglacial landforms at a larger spatial scale" (L455). These are two different goals, each with their own strengths and limitations. I got the impression that the authors framed the paper towards the first goal of a local process-oriented case study, but the implicit goal was the second one. I briefly discuss both options.*

*If this study is process-oriented *at landform scale*, then its limitations are: First, with only ERT as the sole source of subsurface information, statements about subsurface ice contents are uncertain and inferior to the state of the art using combined ERT-SRT approaches and petrophysical four-phase modelling. (In the Alps, this lack can be to some extent filled by the rich literature knowledge from nearby sites, but it remains a shortcoming). By focusing narrowly on ice content and type, the confounding role of liquid* *water is insufficiently addressed, both its uncontrolled influence on the ERT data and on the DInSAR-derived surface displacement rates. Second, for the novel regression analysis (L306–342), absolute resistivity values are directly interpreted as a proxy solely for ice content and type, and (which is probably worse as already mentioned by reviewer #1), the electrical resistivity values are presented without their structural/spatial context. In Fig. 7bdf, it (misleadingly) looks like as if the electrical resistivity values, grouped by four a-priori "permafrost classes", alone could be used to infer the subsurface composition and thermal state. This is not true at landform scale (aligned with reviewer #1 who additionally pointed out that the depth distribution of the ice, thickness of the debris cover, etc. also controls the surface deformation), and the authors themselves do a much more careful and valid analysis in Sect. 4.1, taking the spatial resistivity *patterns* into account. Third, the correlation between electrical resistivity values and surface deformation pattern is weak to non-existent for rock glaciers due to the complex processes at depth (discussed in L447) and the lateral stress transmission that detracts from the regression analysis (L439). It seems that for rock glaciers, not much new can be learned at process level from this current data set alone (at least judging from what is shown here).*

*If this study is more methodological and targeted towards identifying ice-rich glacial or periglacial landforms, the story looks different: This study then presents a framework to leverage satellite-based DInSAR data to classify and even outline glacial vs. periglacial ice–debris landforms *at landscape scale*, with important potential applications worldwide. The ERT–DInSAR data set and its analysis at the Pipji and Oberferden sites locally validates the DInSAR surface displacement-derived landform interpretation (that aligns with nearby studies by J. Wee et al.). The novel regression analysis, whose results are presented in L306–342 (including Figs. 6,7), could serve as a blueprint on how to tie the surface displacement patterns to landform type, subsurface characteristics, and geomorphic response in other mountain ranges (where it might differ from the Swiss Alps). The weak/no correlation between resistivity and surface displacement on rock glaciers is not a shortcoming here, but one of the distinct fingerprints of this landform that distinguishes it from more sensitive glacial ice–debris landforms. In the "continental European" mountain permafrost community it is consensus that periglacial and glacial landforms have a different geomorphic response (L24; cf. reviewer #1), but diverging views exist (Harrison et al., 2025): Methods to tackle these questions are needed.*

I believe that once deciding on a clear framing (which of course can include both options with a clearly stated transition), this draft becomes an excellent contribution with reasonable additional text editing effort (the figures are already great).

AR: Thank you very much for your helpful comments and suggestions. We restructured the introduction and tried to clarify the actual aims of this study. And yes, indeed, the study consists of geomorphological questions on the one hand and methodological questions on the other. We have tried to shed more light on the methodological questions in the introduction so that the general structure of the article is easier to follow and our results and conclusion at the end also fit the questions posed in the introduction.

Minor comments and questions

RC: The following points concerning the regression analysis might be better discussed or briefly mentioned: Is the max(log(resistivity)) sensitive to outliers, would a, say, 90% percentile be a more robust measure? What is the DInSAR pixel size on the ERT profiles, over which area (or along-profile length) is the electrical resistivity averaged? Is there a depth-cutoff beneath which you ignore the resistivity values because they might have little correlation with the surface deformation on top? What is the scatter from interannual DInSAR displacement variability? Do the seasonal patterns consistently repeat each year (perhaps shifted only by the variable date of snow melt-out), or are they sensitive to the summer weather too (precipitation, water infiltration)?

AR: Thanks for the questions. We checked the robustness of the data before we conducted the analysis and there seems to be no distinct outliers. For sure, using a 90% percentile would be more robust from a statistical point of view, but there might be some masking effects in areas with very high ice contents, especially when these anomalies are comparatively small or thin. Therefore, we decided to use the maximum after carefully checking the general data quality. From a morphological point of view, it would not affect the interpretations in a distinct way.

The DInSAR pixel size on the ERT lines is not regular due to the different orientations of the individual ERT lines compared to the raster orientation of the SAR-data. We calculated the pixel boundary positions in the respective ERT lines and then calculated the max. resistivity in the entire depth column of the pixel, but only below the active layer to exclude outliers caused by air filled voids in the active layer. We did not use a lower boundary to avoid excluding any effects of deeper layers. Maybe it is worth to compare the relationship between surface movement and subsurface properties of different depth layers in the next step. Nevertheless, this is a considerable effort and exceeds the framework of this paper.

RC: The Figures are overall carefully crafted, my compliments.

AR: Thank you very much!

RC: Please check the following words, they are vague, colloquial, and often unnecessary: "real", "striking", "very", "drastically", "especially", "some", "often" (should be used for frequency in time, not in space; an "often high ice content" makes me think of seasonally variable ice contents), "normal", "intense", "recent", "strong". Then: "Alpine" (capital A) vs. "alpine", which conveys a slightly different meaning. The beautiful Valais place names: "Hungerlitälli" instead of "Hungerlitaelli", since German umlauts are also used for "Üssers Barrhorn". Furthermore, I found the formulations "two-dimensional profile" and "quasi *three-dimensional grid" a bit cumbersome: Introduce it once like this for clarity, but it becomes pleonastic afterwards. Simply "profile" and "grid" is then enough.*

AR: We checked the respective words throughout the entire manuscript and replaced all irrelevant or vague formulations. The use of "two-dimensional" and "quasi-three-dimensional" was reduced to the necessary level. Where appropriate, the abbreviations "2D" and "q3D" were used.

RC: L49-51: A high ground ice content is used to argue once for stability (L49) and once for instability (L51). The apparently double role of ground ice is confusing, please clarify. I think the confusion arises partly because it is not distinguished between thermal (L49) and mechanical (L51) instability.

AR: We adjusted the respective paragraph during the general restructuring of the introduction and clarified this.

RC: L93: "Climatic stations in the vicinity…are rare in the area". Please note that for international readers, the density of weather stations in the Swiss Alps might appear almost ridiculously high. In a similar vein is the comment by reviewer #1 on the "dry" and "continental" inner-Alpine climate in the Valais.

AR: That is completely right, we adjusted the formulation accordingly.

RC: L127: In which season/DoY were the ERT profiles/grids acquired, and how was the weather in the weeks prior to the field campaign (wet or dry summer)? Please briefly mention that (possibly in an expanded Sect. 3.1 as requested by reviewer #1).

AR: Respective information was added to the text. All measurements were conducted in the period of late August to early October in the field seasons 2023 and 2024. Weather conditions were rather dry, except for the measurements of the 3D grid in Oberferden, which was measured during moist conditions.

RC: L134: Which DEM was used?

AR: We used the SwissALTI3D DEM, we added the information to the text.

RC: L142 (and elsewhere): Inconsistent use of n-dash (–) and hyphen (-).

AR: Thank you for pointing that out. We checked our use of n-dash and hyphen throughout the article and adjusted if necessary.

RC: L173: Why "apparent", not "inverted" resistivities?

AR: You are right and this was an error in the text. We used the inverted resistivity values for the statistical analysis. We adjusted the text accordingly.

RC: L179: "It was checked that…" is painstakingly honest. It is enough to write something like "The active layer, that can have high-resistive cells due to, e.g., air-filled voids, was excluded from the analysis to ensure that the resistivity values reflect the permafrost."

AR: We changed as suggested.

RC: L299 (and elsewhere, including the label in Fig. 7bdf): Copernicus standard requires "Ranges need an en dash and no spaces between start and end (e.g. 1–10, Jan–Feb)".

AR: Thanks for the reminder, we changed as suggested.

RC: L306: Put the text in L306–342 in its own section 4.3 and consider reversing the order of Figs. 6 and 7 together with the accompanying text. First, the generalized insights, second the slices along the profiles.

AR: We restructured the section as suggested.

RC: Fig. 6: Adding titles on top of the two rows ("Pipji rock glacier" and "Oberferden thrust moraine complex") would add clarity. Consider sharing the same y-axis scaling across the two columns for easier comparison. Notably the very different seasonality in (c) and (d) is masked by the different y scales. Could it be that the variation along the Pipji profiles appears larger compared to Oberferden (where the profile is much shorter, so actually also the x-axis scaling is different)? What is negative seasonality in panel (c)? (Seasonality could be a bit better explained in the methods part).

AR: We added titles to the two columns, thanks for pointing out this. Yes of course, the scalings of the x and y axis are different for the two columns. Due to the distinctly different ranges and the different profile length we had to adjust this. A respective identical scaling would lead to a loss of information for the individual profiles. A generalized scaling of the x-axis would result in an undesired optical imbalance between the two profiles due to the different length. To avoid misunderstandings, we added a note to this to the figure caption and have also adjusted parts of the graphic.

Negative seasonality means that there are stronger movements in the early than in the late summer season. We added some additional information in the methods chapter.

RC: Sect. 5: Please consider introducing more subsections for easier orientation in the (long) text.

AR: We added another subsection in the result chapter to delineate the description of surface movement and the analysis of linkages between the surface movement and the subsurface properties. In the discussion section we added another subchapter for the discussion regarding the delineation of different landforms and the use of the surface displacements as a proxy for climate change.

RC: References

Harrison, S., Racoviteanu, A., Shannon, S., Jones, D., Anderson, K., Glasser, N., Knight, J., Ranger, A., Mandal, A., Vishwakarma, B. D., Kargel, J. S., Shugar, D., Haritashya, U., Li, D., Koutroulis, A., Wyser, K., and Inglis, S.: Will landscape responses reduce glacier sensitivity to climate change in High Mountain Asia?, The Cryosphere, 19, 4113–4124, https://doi.org/10.5194/tc-19-4113-2025, 2025.

Wee. J.: Glacier-permafrost interactions and interrelation: dynamics of Little Ice Age glacier forefields in alpine permafrost environments, PhD thesis, Uni Fribourg, 2025, https://folia.unifr.ch/unifr/documents/333093

AR: We included the suggested literature at relevant positions in the text.

We would like to thank the reviewer for the constructive criticism, which has contributed to a significant improvement of the manuscript. We hope we have been able to answer all questions and clarify any ambiguities.

Many thanks and best regards on behalf of all co-authors,

Julius Kunz