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 supplemented accordingly.

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

Review No. 1:

RC: The paper deals with the current structure and kinematic behavior of landforms resulting from the interaction between advancing glaciers and frozen debris (permafrost) during the Little Ice Age in two areas in the Swiss Alps. It is based on the combination of two techniques, namely electrical resistivity tomography (ERT) to assess the internal structure of the investigated landforms, and DInSAR, satellite-borne radar interferometry, to determine the surface motion of those landforms, the observed movement resulting from the varying combination of the downslope movement of the mass and subsidence induced by the melt of embedding glacier ice.

Whereas the data and results are of good quality, partly innovative, properly presented and discussed, making this paper worth of being published, the latter suffers from a number of weaknesses. Major improvements are required before any further consideration for publication.

I don't grasp the actual goal of the paper: it looks that it is more focusing on the combined application of both methodologies than on solving the question(s) they are applied for (as the title announces). I would suggest to reverse the importance, first clearly stating the question(s), then evaluating the contribution of the applied methodologies to solve them.

AR: Thank you very much for your comments. We tried to clarify this in the introduction and restructured the paragraphs accordingly. In addition, we highlighted the applicability of the methodological approach to delineate subunits of individual landforms as an additional objective of this study. Since the question was addressed in the discussion and the results anyway, but was not raised as a question in the introduction, we hope that adding it will give the article a more stringent form and a clearer central theme.

RC: I would also strongly suggest to the authors to make use of the Swissimage (orthoimages) made available by Swisstopo for about the last 60 years to :

- analyze the development of the investigated landforms over the last decades (e.g. development of thermokarst depressions),

- determine the current (e.g. 2017-2023) flow fields (direction and velocity) of the investigated landforms by feature tracking; it could be done for single boulders or by applying GIS tools as IMCORR or others.

AR: We included the results obtained from the IMCORR approach into the manuscript to clarify the open questions. They were included as additional tiles in Fig. 5. We referred to them at relevant positions, but the main focus of the manuscript remains on the combined methodological approach of DInSAR and ERT. We hope that the added information on the local flow fields helped to clarify the open questions and enhanced the understanding of the local morphodynamics.

In addition, we used the Swisstopo data to map the thermokarst depressions, which developed during the last decades. Nevertheless, these are not in the central focus of this article since it focused mainly on recent surface movements and their relationship to subsurface properties. The link to former thermokarst processes is useful to understand the general geomorphology of the study sites but not provides important information about the morphodynamics during the study period.

RC: It would also be helpful to propose a clear labelling (e.g. numbering or naming) for the various investigated landforms. Otherwise, it is sometimes difficult to follow what it is spoken about.

AR: We added some labels for the individual landforms. We highlighted their outlines in Figure 1, included the labels and used the same labels/abbreviations in the text. We used the following abbreviations for the individual landforms:

OF_TM: Oberferden thrust moraine complex
OF_RG: Oberferden rock glacier
BW_TM: Barrwang thrust moraine complex
PI_RG: Pipji rock glacier

RC: The paper should gain in concision and consistency. It could also be shortened. There are issues with the meaning of sentences or terminologies (I only went into details in the very first part of the paper, but this has to be applied all along). Also a part of the discussion could be moved in the methodological section.

My comments - after a first detailed section - are only limited to some important points (and suggestions). It might be that I misunderstood some aspects, meaning that some of my comments could be disregarded. I would be glad to proceed to a full review of the paper after it has been reworked.

AR: Thanks for your numerous comments regarding the terminologies. We tried to adapt your recommendations throughout the entire article and clarified the respective paragraphs. We also restructured the article and moved some sections of the discussion to the methods chapter. We also tried to shorten the manuscript, but since both reviewers

suggested to include more information in several sections there was no real shortening of the manuscript possible.

Detailed comments:

RC: The title sounds a bit weird to me. I would suggest something as : "Current morphodynamics and subsurface structure of thrust moraines and rock glaciers connected to Little Ice Age glacier forefields : case studies from the Swiss Alps". Another option could be to emphasize on the applied techniques, e.g. : ERT and DInSAR for investigating the current…

AR: Thanks for pointing this out. We changed the title to "*Current morphodynamics and subsurface structure of thrust moraines and rock glaciers connected to three Little Ice Age glacier forefields in the Swiss Alps"*

RC: l.9-11 – "Glacier-permafrost interactions in Alpine environments significantly influence geomorphological processes, making it essential to understand the relationship between subsurface structures and surface morphodynamics for predicting landscape evolution under climate change ». Such a sentence has no clear meaning to me. Glacier-permafrost interactions are not specific to Alpine environments. In LIA glacier forefields, there are often not occurring anymore since decades (one should use the past). Subsurface structures and morphodynamics of what ? (the sentence is talking about interactions). Why do we need to predict the landscape evolution there ? Why is it "essential to understand" those relationships between subsurface structures and morphodynamics ? Make all that much clearer.

AC: Thank you for pointing out the unclear meaning! We completely restructured the first part of the abstract to clarify, what we intend to say.

RC: l. 11 – "direct correlation" – what does it mean? Could it be indirect?

AR: The entire sentence was replaced during restructuring of the paragraph.

RC: l.11-12 – "…correlation between subsurface ice distribution and surface movement for thrust moraine complexes and rock glaciers… poorly understood". I don't grasp the actual meaning of this sentence. We would like to know the kinematic behavior (morphodynamics, if I understand right) of the thrust moraines + rgs on one hand, and their internal structure (i.e. ground ice characteristics and distribution) on the other hand, in order to be then able to infer on their dynamics (i.e. the process driving their motion, if any) in the context of connection to a LIA glacier forefield. No ?

AR: See comments above.

RC: l.12-14 – " This study investigates the internal structure and morphodynamics of both landform types … aiming to determine how subsurface ice content and structure influence recent surface displacements ». Agreed, but I would suggest to write such a

sentence in a more efficient way, avoiding repetition (the second half of the sentence must be merged with the first one).

AC: The sentence was adjusted.

RC: l. 13 – "…both landform types in two Swiss glacier forefields…" : apart altering the formulation, I would suggest to split the Pipji area in two glacier forefield (see comment later) -> both landform types in connection to three LIA glacier forefields in the Swiss Alps.

AC: We adapted the sentence accordingly.

RC: l. 14-15 – "We combined electrical resistivity tomography (ERT) with Differential Interferometric Synthetic Aperture Radar (DInSAR) to assess subsurface resistivity and surface displacement patterns" – I would suggest to clearly associate the method and its objective, namely ERT for assessing subsurface resistivity and DInAR for surface displacement patterns.

AC: The section was adapted.

RC: l. 16 – "on two sites in the Valais region (Swiss Alps)," – almost already said three lines above

AC: The description was changed to "The study focuses on three glacier forefields in two valleys in the Valais region (Swiss Alps)…."

RC: l. 21 – "intense surface dynamics" – what is meant ?

AR: "intense surface dynamics" was replaced by "surface movement rates" to clarify.

RC: l. 21 – "moraine complexes" – thrust moraines complexes ?

AR: We added "thrust" at this position but also at all other positions to clarify.

RC: l. 21 – « logarithm of maximum resisitivity » - what is the maximum resistivity ? why is it talked about its logarithm ? I don't understand the sense of the latter. Maximum resistivity seems to be sufficient.

AR: The "maximum resistivity" is the highest electrical resistivity value detected in the subsurface column below a respective remote sensing pixel. In the statistical analysis, logarithm of this value was used to minimize negative effects of outliers. Probably it is better to talk about maximum resistivity in the abstract. We changed the sentence to "…with the maximum electrical resistivity of the subsurface correlating with absolute horizontal displacement (R^2 = 0.75) and elevation change (R^2 = 0.76)."

RC: l. 24-25 – "These findings underscore the importance of distinguishing between these landform types in permafrost studies and climate change assessments" – Again, I don't understand the meaning. There are very probably some shortcuts in what the authors would like to say.

AR: We added additional information to the sentence to clarify our statement: “These findings underscore the importance of distinguishing between these landform types in permafrost studies and climate change assessments, since the different landform types might react morphologically differently to changing climate conditions.”

RC: l. 27 – “Glaciers and permafrost … occur in close vicinity…” – Note that this paper is not talking about glaciers and permafrost in general, but about a specific interaction which occurred between glaciers at their snout (and/or in their ablation area) and frozen debris during the Little Ice Age, and their consequence it has on the present-day morphodynamics in the related glacier forefield.

AR: Thank you for the comment. We deleted the sentence since it is not relevant for the topic of the paper.

RC: l. 28 – “changed precipitation patterns” – What is meant?

AR: Climate change is altering precipitation patterns in terms of both space and, above all, time/seasonality, resulting in less of the total precipitation falling as snow and more as rain. The lack of fresh snow and, in some cases, warm summer precipitation are affecting the mass balance of glaciers. We have added additional information.

RC: l. 30 – Permafrost degradation in the European Alps is not related to any change in the precipitation patterns (depending however how the latter is defined, what we here don’t know).

AR: We have adjusted the sentence accordingly.

RC: l. 30-32 – “These changes are accompanied by morphological processes in the corresponding areas, which can have a wide variety of consequences such as changing creep rates of rock glaciers…” : The meaning is unclear to me. If I understand the sentence right, it gives that changing creep rates of rock glaciers (what are these changes ?) are consequences of morphological processes (but which ones ? is rock glacier creep not itself a morphological process ?) accompanying (what does it mean ?) permafrost warming and degradation (what is the difference ?) due to higher atmospheric temperatures (by how much (large or tiny) ?) and changed precipitation patterns (what are they ?) ? So, plenty of questions.

AR: Thank you for pointing out that this sentence might be misleading. We have adjusted the respective sentence to clarify the respective statements in the text and adjusted misleading formulations as follows: “These climatological changes lead to morphological adjustments in the corresponding areas, such as increasing or decreasing creep rates of rock glaciers (e.g., Fleischer et al., 2021; Kellerer-Pirklbauer et al., 2024; Marcer et al., 2019), higher frequencies of rockfall events (e.g. Hartmeyer and Otto, 2024; Pfluger et al., 2025) or thermokarst phenomena (e.g. Cusicanqui et al., 2023).”

You are right, rock glacier creep is a morphological process itself, rock falls and thermokarst as well. Changes in rock glacier velocities do not occur uniformly in the context of climate change. Depending on the setting, some areas accelerate, while others thaw or dry out and slow down. Therefore, we can see both acceleration and deceleration. Further information on this can be found in the literature listed in the text.

There are differing statements in the literature regarding temperature increase. However, the degree of warming is not the subject of this discussion and is irrelevant to the statement made. Warming of permafrost means a positive temperature change in permafrost temperature (so still below 0°C). A degradation of permafrost means warming of subsurface temperatures from negative to positive temperatures and therefore the loss of permafrost. In our point of view the wording in the text is clear and readers of this journal are probably aware of the difference.

RC: l. 34 – "Especially in areas where permafrost and glaciers have interacted in the past, high amounts of ground ice are incorporated in moraines…". I guess it is meant incorporation of glacier ice (not ground ice).

AR: Yes, you are right. Glacier ice was incorporated and still exist as buried ground ice. We replaced "ground" by "sedimentary".

RC: l. 35 – "… and other glacial and periglacial landforms ». What are they ?

AR: Other glacial and periglacial landforms are hummocky moraines or rock glaciers for example, but there are several other moraine types as well. We included the two mentioned examples in the text.

RC: l. 35 – "…which are now affected by changing climatic conditions". Not only "now", but already since glacier retreat started, many decades ago. However, as the glacier retreat was considered as possibly favoring ground cooling and permafrost aggradation for decades, the climate evolution (dramatic temperature rise by about 2°C) for the last three decades has more than counterbalanced this presumed earlier trend.

AR: Thanks for the comment. We deleted this section during restructuring of the entire paragraph.

RC: l. 36 – "The last intense interactions…" - It must be stated that the sentence is geographically limited to the Alpine context, and to many other mountain ranges, but is not ubiquitous as interactions are still occurring in some other regions in the Andes or Asian high mountain ranges for instance.

AR: That's true, thank you for the comment. We added "In the European Alps," to the beginning of the sentence.

RC: l. 38 – "… local permafrost base" - What is the local permafrost base ? The permafrost base is the depth to which the ground is frozen. It is obviously used here in another sense (regional lower limit in elevation ?).

AR: That's true, the wording was not correct. We changed "permafrost base" to "lower altitudinal limit of permafrost".

RC: l. 38 – "…real interactions" – There are also apparent ones ?

AR: No, you are right. There are no interactions. We deleted "real".

RC: l. 39 – "only" or mostly ?

AR: "only" was replaced by "mostly".

RC: l. 39-40 – ".. enabled transmission of glacial stress into the proglacial …" – The "proglacial area" means "debris masses overridden by the glacier or in contact at its front ?

AR: Yes. We adopted the suggested wording.

RC: l. 40-41 – "… and resulted in large-scale morphological overprinting in the proglacial areas". What is this specific large-scale morphological overprinting ? Is not any advancing glacier morphologically overprinting the area on which it develops ? Beside the repetition in the sentence, why are proglacial areas now plural, whereas they weren't before.

AR: Yes, every glacier overprints the area on which it develops, but only in the case of interaction between glacier and permafrost the overprinting significantly exceeds the maximum ice extent due to the transferred stress field. However, this part of the sentence was not really neccessary and was deleted during shortening of the article.

RC: l. 44 – "only few studies focus on thrust moraines in alpine environments" - It could be worth of referring to some of them at least (e.g the earlier works by Evin (Fabre and Assier) in the French Alps in the 1980s, then other works in various regions around 2000 and later (e.g. Kneisel, Reynard et al., Lugon et al., Monnier et al.) as examples)

Evin, M. & Assier, A. 1983. Glaciers et glaciers rocheux dans le Haut-vallon du Loup (Haute-Ubaye, Alpes du Sud, France). Zeitschr. für Gletscherkunde und Glazialgeologie 19: 27–41.

Evin, M. 1992. Une moraine de refoulement au Viso (Italie). Zeitschr. für Gletscherkunde und Glazialgeologie 27/28:11–24.

It should not be so complicated to translate these two first papers. Moraine de refoulement = push moraine = thrust moraine

Reynard, E., Delaloye, R., Baron, L., Chapellier, D., Devaud, G., Lambiel, C., Marescot, L. & Monnet, R. (2003). Glacier/permafrost relationships in recently deglaciated forefields of small alpine glaciers, Penninic Alps, Valais, Western Switzerland. Proceedings of the 8th International Conference on Permafrost, Zurich 2003, Vo. 1, 947-952

Lugon, R., Delaloye, R., Serrano, E., Reynard, E. and Lambiel, C. (2004). Permafrost and Little Ice Age Glaciers Relationships: a Case Study in the Posets Massif, Central Pyrenees, Spain. Perm. Perigl. Process. Vol 15/3, 207-220

Ribolini, A. et al. (2010). The internal structure of rock glaciers and recently deglaciated slopes as revealed by geoelectrical tomography: insights on permafrost and recent glacial evolution in the Central and Western Alps (Italy-France). Quat. Sci. Rev., 29, 507–521 (doi: 10.1016/j.quascirev.2009.10.008)

Monnier, S., Camerlynck, C., Rejiba, F., Kinnard, C., Feuillet, T., Dhemaied, A. (2011). Structure and genesis of the Thabor rock glacier (Northern French Alps) determined from morphological and ground-penetrating radar surveys. Geomorphology 134 (3–4), 269-279. https://doi.org/10.1016/j.geomorph.2011.07.004.

Capt, M., Bosson, J.-B., Fischer, M., Micheletti, N., Lambiel, C. (2016). Decadal evolution of a very small heavily debris-covered glacier in an Alpine permafrost environment. Journal of Glaciology, 62 (233) 535–551. doi: 10.1017/jog.2016.56

AR: That is right, thank you for pointing this out. Thanks for your suggestions, we included some of them in the text.

RC: l. 44-45 – "… although in mountain areas like the European Alps, these structures can be particularly large compared to the size of the advancing glacier" – I would change "although" by "even", because thrust moraines complexes can be much larger, also compared to the size of the advancing glaciers, in other mountain ranges.

AR: We changed to "even though". Only "even" does not make sense from our point of view.

RC: l. 46 – "…incorporation of massive sedimentary ice" – see also Kääb et al. (1997), Gärtner-Roer et al. (2022) and some of – if not all - the references provided in one of my previous comment.

Kääb, A., Haeberli, W., and Gudmundsson, G.H.: Analysing the creep of mountain permafrost using high precision aerial photogrammetry: 25 years of monitoring Gruben rock glacier, Swiss Alps, Permafrost and Periglacial Processes, 8, 4, 409–426, 1997

AR: We added some of the references. However, it is not possible or at least not necessary to include all studies in which this phenomenon is described. A corresponding list would simply be too long and would not match the aim of the article.

RC: l. 48 – "…and often very high ice content in the interior of these landforms have favoured a transition to periglacial conditions". I don't see the point (why a very high ice content favors the transition), and the actual meaning of periglacial conditions.

AR: We rephrased the paragraph in order to clarify.

RC: l. 50-51 – "…some of these landforms also show recent surface changes". This is just because they have only been investigated recently. They are presumed (or known) as having been subject to large(r) surface changes (what is actually the meaning of this ?) in earlier time after glacier interaction ceased.

AR: That is absolutely correct, but our statement does not deny these past changes, which may have been even more significant. It is merely a reference to current surface movements. However, the entire paragraph was restructured and edited and therefore we clarified this.

RC: l. 51 – "…could react sensitively in the context of global warming due to their high ice content.". What kind of reaction is it talked about ? Except for the subsidence or the development of thermokarst features, I don't see the point related to the high ice content.

AR: These are precisely the reactions meant. In our point of view these two options are quite important consequences, which should not be neglected. We have now specifically mentioned them in the text to clarify this.

RC: l. 54 – "… and confirmed some direct relationships between morphodynamics and the distribution and type of ground ice." What are these relationships ?

AR: The fundamental relationship is that the presence of ground ice is directly linked to specific surface morphodynamics through processes such as creep or subsidence/degradation. Different ice types in the subsurface can result in different dominating processes and therefore in different movement characteristics. We added the following paragraph to clarify: "The presence of ground ice enables processes like permafrost creep, subsidence due to ice degradation, or internal deformation in massive ice. The extent to which the dominant process is influenced by the type and distribution of ice, and the extent to which it in turn affects the magnitude and characteristics of the resulting surface movement, is not yet clear.."

RC: l. 55 - "correlations" – Note that correlation is a statistical term, which doesn't imply any causal effect. It is just expressing the similarity in the development of two variables.

AR: That is correct and was originally intended here based on the statistical analysis below. Nevertheless, it might be better to speak of "causal relationships" here, as the statistical analysis has not yet been mentioned.

RC: l. 56. – "..information on correlations between the recent displacement rates and the existing subsurface structures as well as ice contents and ice types are still scarce". Unclear meaning. If I am right, it is talked about the possible influence (driving) of either the "existing subsurface structures" (what does it mean ?), or the "ice contents" (why plural ?), or the "ice types" (what is meant ?" on the "recent displacement rates" (meaning permafrost creep rate ?). But it has never been talked about displacement (and the related motion mechanism) until then and we don't know if we are talking about rock glaciers in

general, rock glaciers connected to a glacier forefield, back-creeping push-moraines or whatever else.

AR: We adjusted the sentence as follows: "However, more detailed information on causal relationships between the recent surface movement and the existing subsurface properties (ice content and ice type) are still scarce for thrust moraine complexes, but also for glacier-forefield connected rock glaciers." In this case, we are only concerned with the general surface change and not yet with the individual processes that result from it. When we look at surface movements, in such complex environments these usually result from various processes. What we detect on the surface in this case may therefore be a combination of permafrost creep, subsidence, and back-creep. However, this cannot be distinguished from the surface in terms of processes, as we only see the end result, i.e., the resulting movement from all processes. And that is precisely why we are only talking about surface movements here and not about detailed processes. We added additional information to specify the open research questions after this sentence.

RC: l. 57-58 – "this study builds on a combined approach of geophysical surveying and remote-sensing-based surface displacement analysis using Differential Interferometric Synthetic Aperture Radar (DInSAR)...". It must be explained why having chosen this approach and these techniques. It means that the research questions and their motivation should come before the selection/presentation of the approach and not after (l. 71-74).

AR: That is right. We adjusted the paragraph accordingly and explained our research questions first, before describing the methodological approach.

RC: l. 63 – "...DInSAR – an increasingly common method to study alpine periglacial geomorphology and morphodynamics" – DInSAR is not just one method. It is therefore used since more than 25 years to detect and monitor slope movements in mountain areas, and rock glaciers in particular.

Rott, H., Scheuchl, B., Siegel, A. & Grasemann, B. 1999. Monitoring very slow slope movements by means of SAR interferometry: A case study from a mass waste above a reservoir in the Ötztal Alps, Austria. Geophysical Research Letters, 26(11): 1629-1632.

Kenyi, L. & Kaufmann, V. 2003. Measuring rock glacier surface deformation using SAR Interferometry. Proceedings of the Eighth International Conference on Permafrost, Zürich, Switzerland, June 2003. Balkema, 1: 537-541. rticular (e.g. Rott et al. 1999, Kenyi and Kaufmann 2003 for some of the earliest publications)

AR: We adjusted the sentence to clarify our statement and included the mentioned references. Thank you! We are aware of the fact, that the method is used since several years. Nevertheless, the method was not frequently applied or used for detailed spatio-temporal analysis before the launch of the Sentinel-1 satellite constellation.

RC: l. 68-68 – "the use of satellite-based SAR data now enables a higher temporal resolution and a large spatial coverage so that also seasonal changes can be explored more systematically". But there are also plenty of limitations, which would be worth of mentioning already here.

AR: Thank you for pointing out that mentioning these limitations right here in the introduction. We restructured the sentence as follows to mention the limitations of DInSAR, but also highlight its advantages, that are the reason why we chose an approach using satellite interferometry over other established approaches such as Feature Tracking: "While satellite-based DInSAR is only capable to detect movement in east-west and vertical direction during the snow-free summer season, it provides data at high temporal resolution and large spatial coverage so that also seasonal changes can be explored more systematically (Buchelt et al., 2023, 2024), whereas UAV-based feature tracking approaches (Blöthe et al., 2021; Vivero et al., 2022) or direct kinematic measurements (Cicoira et al., 2022; Kenner et al., 2017) are spatio-temporally restricted and often time-consuming."

RC: l. 72-73 – "What is the current internal structure of alpine thrust moraine complexes and are there differences to neighbouring rock glaciers?" The study is limited to two or three sites only. The objective cannot be generalized to alpine thrust moraine complexes from there only. Therefore, it must be stated that the neighbouring rock glaciers are specific ones only, namely glacier forefield-connected landforms that you morphologically identify as rock glaciers. Basically the latter move outward of the area presumably covered by the glacier during its LIA advance(s).

AR: We adjusted the statement accordingly and restricted it to "selected thrust moraine complexes in the Swiss Alps".

RC: "What kind of surface morphodynamics takes place within the respective glacier forefields?". So, the study is not restricted to the thrust moraine complexes and adjacent rock glaciers, right ?

AR: We adjusted the research question to "What kind of surface morphodynamics takes place in the area of the respective landforms?".

RC: "Are there any linkages between subsurface structure and recent morphodynamics, and are there any differences in this relationship considering the different landform types?" Be more precise. There are mainly three types of movement to be expected, sometimes combined, which could make their detection/disentangling challenging : permafrost (rock glacier) creep (which could also affect thrust moraines) / subsidence (caused by the melting of embedded glacier ice, mostly occurring at shallow depth during the warm/snow free season) / landslide (generic term for mass movement, which could be restricted to debris/ice masses or impacts bedrock as well.

AR: We tried to clarify.

RC: l. 76. " ... an inner-alpine dry region ". To be clear, what is the meaning of dry ? continental ? We are here talking of 1000 mm/year at Pipji and 1500 mm/year at Oberferden...

AR: Yes the meaning of "dry" relates to the continental climate – as stated in the same sentence. Of course, we are talking about 1000 or 1500 mm/year (as mentioned further down in the study site description), but compared to other regions in similar altitude in the European alps, the range seems to be low and the Valais is generally recognized as an inner-Alpine dry region. However, we deleted this not necessary part of the sentence to avoid confusion.

RC: l. 82. Pipji glacier forefield. As explained in the description, there are two distinct glacier forefields at play in the Pipji valley. The Pipji glacier forefield on the orographic right side and an unnamed (Barrwang ?) glacier to the left. Both were only touching at a very tiny and insignificant uppermost section and not influencing each other. It would make much easier for the paper to distinguish them from the beginning and talk about two distinct sites, because the morphologies in both glacier forefields are fully differing one from the other.

AR: That is correct and probably easier to distinguish in the text. Therefore, we decided to adjust the text accordingly.

RC: l. 87. "previous activities". Is it meant former Holocene glacier advances ?

AR: Yes, we adjusted the text accordingly.

RC: l. 87. "...which provided the basis for rock glacier formation in the area of the former main glacier." What is it meant ? This is unclear to me.

AR: The moraines of the Pipji glacier provided material/debris for the rock glacier formation. We adjusted the text.

RC: l. 88. "Due to several glacial advances and successive phases of rock glacier formation, the rock glacier consists of three different units (Units I - III, see Fig. 2a), which are characterized by different ages". What are these ages ? It is important to know (if possible).

AR: To our knowledge, there was no age dating at these landforms so far. There are different ages (Yunger Dryas, Holocene) mentioned in the literature but there are no reliable data so far (at least to our knowledge). Therefore, we decided to give no absolute statement at this point. For the recent study the recent state and the related morphodynamics are more important than the actual age of the landform, which is why we would accept the lag of information at this point. However, as part of the recommended shortening, we have deleted the subclause.

RC: l. 104. Why are you not talking about the part having overridden the rock ridge to the north-east and has developed down to 2580-2600 m a.s.l. ?

AR: We now have mentioned it in the text, but we have no data from this area, which is why we focus on the upper moraine complex and the rock glacier.

RC: l. 108-109. What is meant by a former glacier advance ? Before the last one of the LIA, but still during the LIA ? Something older ? Or you don't know ?

AR: We do not know the age of the glacial advance. From local morphology it seems like it was older than the last major advance during the LIA. The extent of the latter is visible quite well in the field due to debris adjustment and reached only the uppermost end of the rock glacier.

RC: A look at the orographic right side obviously gives the impression that the so-called rock glacier overrode the bottom part of the talus, without clearly deforming them. I presume this is mostly just a glacier deposit. The coarse material at its surface is resulting from the glacial deposit and the erosion of rock walls upstream of the "rock glacier" tongue, but is not derived from the talus slopes adjacent to it. The present-day movement (comparison of Swissimage 2017-2023, not provided in the paper) gives the impression to be in the continuation of the talus slopes only. If so, the glacial deposit is just a deposit currently embedded in the terminal part of a talus-connected rock glacier development.

AR: From a morphological point of view the investigated landform has to be classified as such. If we look on the flow field of the former glacier (which is visible due to material alignment) and also on the surface structures of the rock glacier, it is visible that the material of the orographic right half of the rock glacier nearly entirely originates from the north and northwest-facing rock walls adjacent to the rock glacier root zone. The material of the orographic left side of the rock glacier seems to be transported by the glacier from higher elevations in the glacier forefield. There might be also glacial transport between the rock walls and the orographic right half of the rock glacier in the past, but due to the short distance of transportation we have distinctly differing substrate properties. And at least in the last 3 decades, there seems to be distinct permafrost creep in the north facing scree slope which is located in the rock glacier rot zone. The scree is not derived from the scree slope adjacent to the lateral margin seems to be self-evident from a morphological point of view. We adjusted to description of the locality to "talus slopes between Schwarzhorn and Mandlischfurgga" so that is clear that the debris did not originate from Restigrat itself.

RC: l. 115 – Fig. 1 – a-b) Please, legend the images (draw oultlines); d) the LIA glacier extent in its terminal margins is only presumed; it could have been significantly larger. e) the thermokarst depressions which have developed over the last decades must be mapped; legend) the layout between glacial-gravitational and glacial-periglacial is not possible to be distinguished on the maps.

AR: (a & b): We added some labels and outlines of the most important landforms. (d): Yes, it's true that the outline is only presumed, but debris alignment which is visible on small-spatial scales in the field suggest a similar extent than the given outline. There are several

morphological indicators in the field which confirm that the area of the forefield-connected rock glacier was not glaciated during the last main advance of the LIA. (e): We added the thermokarst depressions to the map and adjusted the color coding of the mentioned classes.

RC: l. 155 – "The final output was a 6-day Line-of-Sight (LOS)" – Could it not be an issue when also using 12-18 days interferograms ?

AR: Thank you for pointing that out. Surface displacement values of longer temporal baselines were rescaled to 6-day displacement values to assure a consistent time series without gaps for the consecutive analysis. Besides, such longer temporal baselines were only necessary in very few cases, usually 0-1 per study site and snow-free summer period/year.

RC: l.160 – Multiplying by three the values of the stacked DInSAR times series to expand the "summer" observation to the entire year is to me an issue, because the downslope movement is susceptible to change over the year and the subsidence due to ice melt is probably restricted to the summer time.

AR: It is correct, that such an extrapolation does not represent the temporal change in velocities over the year, as also mentioned in the discussion section. As DInSAR is only capable to provide reliable information during the snow-free summer months we are limited to this time period. However, as the here used time reference for our velocity is ma-1, we decided to use the result multiplied by 3 in order to be consistent. Further, a systematic comparison of DInSAR movement data with annual kinematic data from FT (see Buchelt et al. 2023) has shown that this multiplication by a factor of 3 is at least valid for horizontal E-W movement. In order to be consistent, we this multiplication is also incorporated in the vertical component, even though it might be more affected by deceleration during the snow-covered period.

RC: l. 164 – The correction "for slope-induced effects by local inclination" presumes that the downslope component of any mass movement is following the local inclination of the surface topography, right ? This must be clearly stated.

AR: Thank you for highlighting this. You are absolutely right that using the surface topography might not be the reality. In fact, bedrock topography would be much better suited to correct for this. However, as we do not have any information about the latter, we have to make a compromise and, therefore, use the surface topography. We additionally mention in the text that we use the surface topography.

RC: l. 165 – "seasonal variations" - So, that's what is called later the seasonality? But I cannot figure out what it is. I tried to read several times the sentence, but it didn't help me. Please make this explanation clearer, and use here the term "seasonality".

AR: We have to apologize for the inconsistency in naming here. The sentence was rephrased to clarify this in the following way:

“For determining the seasonal dynamics (i.e. movement variations between early and late summer), the following parameter was derived from the 6-day LOS displacement time series (Buchelt et al., 2023): Absolute seasonality is defined as the difference between median movement rates in June/July (early snow-free summer) and September (late snow-free summer) in mm/6d (6d = the observation window of one interferogram).”

RC: l. 184-186 - The limits are arbitrary. Permafrost could occur at much lower resistivities than 10 kohm.m. The "ice-poor" and "ice-rich" terminologies have to be left aside as many other factors affect the resistivity (e.g. lithology, temperature, water content) and there are so many different uses and meaning of these two terms that it is better to avoid them. Just talk about very low, low, high, very high resistivity. It would be also very good later to clearly mark these limits on the ERT plots, because just using a degraded color scale is making this delimitation impossible for the reader.

AR: These boundaries are frequently used in the literature, especially for distinguishing between frozen/unfrozen and permafrost/sedimentary ice. However, the boundary value for frozen/unfrozen seems to be reasonably well suited for these sites, which could be confirmed using additional data (borehole temperatures, GPR). We are well aware that resistivity is not determined solely by ice content, which is why the distinction between ice-poor and ice-rich is rather vague. It should be noted at this point that this is a first attempt to classify general zones and to consider their spatial changes separately from each other. Also, our resistivity scales are non-linear, so that the ice-rich class integrates resistivities that are several orders of magnitude higher than the ice-poor class. In this regard, the common logarithm (base 10) provides an intuitive way to form and compare classes. As mentioned above, the class boundaries can vary from site to site, even within an individual glacier forefield, so a certain degree of generalization is necessary in order to implement such an approach. We will therefore retain the classes accordingly, well aware that they are not necessarily accurate everywhere. For this very reason, however, we have refrained from drawing or highlighting boundaries in the tomograms, as these sharp boundaries may not exist in the subsurface in this form and are presumably more fluid transitions.

RC: l. 191 - Please provide larger and clearer geomorphological maps of the investigated areas. Investigate the past development (e.g. thermokarst lakes at Pipji). Map/determine the flow fields.

AR: We added the thermokarst lakes and depressions to the geomorphological Maps in Fig. 1. The information about the flow fields was added to Fig. 5 for better comparison with the DInSAR results.

RC: l. 208 – Note that the massive ice core was visible when e.g. both thermokarst lakes/depressions have developed over the last decades.

AR: We added a note about these observations in the text.

RC: l. 215 - Is Unit I actually a rock glacier ?

a) b) It could be worth of providing a detailed view on the investigated area

c) What is the orientation of the profile ?

AR: This is a good question! Since some of the surface structures have collapsed and the landform in the central area has subsided significantly, this is not easy to assess. It could also be a moraine complex from this point of view. In the literature, the landform is always referred to as a rock glacier, and we have no measurement results that can either confirm or refute this. Since this would be a separate discussion that would go far beyond the scope and purpose of this article, we have adopted the existing nomenclature and have not discussed this further.

Since an additional image would require another figure that would otherwise not add much value, we decided against including an additional image.

The profile in (c) is oriented like shown in the image. The profile start is in the north, the end is on the southern site of the moraine complex. The positions of the letters in the image always mark the start position of the profiles.

RC: l. 245-248 and related figures – "Resistivities of more than 1 MΩm indicate the presence of sedimentary ice within the moraine complex. This detected structure fits well with observations made in the field in late, where massive sedimentary ice could be observed in a thermo-erosional cave in the distal flank of the moraine ». No, it is not correct. The observed massive sedimentary ice outcrop is the obviously the remnant of a snow/ice field which developed outside of the moraine and was partly covered by the crumbling and northward advancing movement of its front in recent years. The permanent snow field is well observable on the Swissimages time series.

AR: We disagree with the reviewer due to the following reasons: The mentioned structure is clearly separated from the permanent snow field and shows distinctly differing sedimentary structures (e.g. their spatial orientation) as the neighbouring snow/ice patch. If this structure would be a remnant of the permanent snow patch and would be covered slowly by the advancing moraine complex there must be a distinct layering of snow/ice and debris. As shown in Figure 3, the sedimentary structure of the ice looks rather similar to glacier ice with included fine material. Besides, there are no larger blocks visible in contrast to the top of the moraine. Such would be expected as they must have tumbled down into the snow patch and were incorporated. Since this massive and comparatively pure ice extends several meters into the landform, we unfortunately have to reject the theory you described. Otherwise, stronger, coarser impurities would have to be found in the ice.

RC: l. 266 - Please make use of Swissimage to determine the flow fields.

AR: See comments above. We included the flow field information.

RC: l. 273 – The "uplifting" area is pretty large. Could it be possible to explain it ? It looks it might be partly due to a convergent flow, with both N-S and S-N components, not depicted by the DInSAR approach.

AR: Yes, probably it is partly due to convergent flow. We added this information to the text. Nevertheless, it could also be a result of difficulties in phase-unwrapping in this area. This is slightly visible in Fig. 5 e, but is also discussed in Chapter 5.1.

RC: l. 279 (and other) – I would suggest to be very careful when using the term "movement". In fact, it is talked about the surface movement, which is the combination of the movement of the entire mass (here a back-creeping one) and subsidence due to melting of embedded glacier ice.

AR: That is right, thank you for the comment. We adjusted the text accordingly and replaced "movement" by "surface movement" at all relevant positions in the manuscript.

RC: l. 290 – Figure 5 – I have some issues with the E-W component, because the N-S, which is significant in some areas, is missed. So, again, it would be worth of providing the flow field as well (derived from orthoimages).

AR: See comments above. We added the flow field data derived from an IMCORR approach to Fig. 5 in order to clarify the open questions.

RC: l. 302-303 – "In the upper part, close to the source area of the rock glacier, only elevation changes occurred during the investigated period" – It is difficult to see (this is small), but an eastward movement seems to occur having a look to Fig. 5. A look at Swissimage confirms that a horizontal movement in the range of 0.1 m/y is occurring there.

AR: Yes, in parts there is very little surface movement, but this is close to the lower detection limit of our approach. To clarify, we added the following sentence: "Lateral surface movement is in a range of up to 0.1 m/a in this area and therefore at the lower detection limit of the method."

RC: l. 309 – It would be better to undertake an analysis comparing the resistivity (namely the ice type) with the 2D horizontal movement, not only its E-W component, except if it has been demonstrated that the latter is well correlated to the former.

AR: Thank you for pointing that out. As mentioned in previous parts of the manuscript, we use DInSAR despite its limitation to East-West displacement as it provides consistent data with high temporal resolution particularly suited for seasonal analysis at large spatial coverage, potentially at global scale. Even though we are just sensitive to horizontal movement in East-West direction, the systematic differences in behaviour are already visible. Having information about the full flow vector would be quite conceivable in a next step.

RC: l. 316 (and other) – To avoid any misunderstanding I would avoid talking about moraine complexes only, but strongly suggest to systematically name them "thrust moraine complexes" or "push moraine complexes".

AR: We adjusted the text and used "thrust moraine complex" everywhere.

RC: l. 317-318 – "… suggests that the morphodynamics of moraines are more consistently influenced by internal structures and, consequently, by ice content, whereas rock glaciers exhibit greater variability". Thrust moraines (push moraines) are often containing embedded glacier ice in their internal (facing former glacier) section. This ice is located close to surface, covered by an insufficient layer of debris to protect it from melting during the warm season. This is not a question of ice content (for sure there is a lot of ice there), but a question of internal structure of the thrust moraine, ice type and its location at depth.

AR: That is right and that is what we wanted to say. But in our point of view, the ice content is also important since volume change and resulting subsidence are strongly related to the ice content.

RC: l. 320-321 – "…linkages with permafrost properties for both kinds of landforms". Probably not with permafrost properties, but with the internal distribution of ice types. Moreover, it cannot be generalized to "both kinds of landforms", but only to those having been investigated in this study.

AR: Thanks, you are right. We adjusted the text accordingly.

RC: l. 325-326 – "Striking is the bimodal distribution of horizontal displacement in the second class for the rock glacier data points". What is the second class ?

AR: The second class refers to the ice-poor permafrost class. We added this to the text.

RC: l. 328-329 – "…which higher resistivities (probably also higher ice contents) mean higher morphodynamics and seasonal variation. This is valid, especially for the vertical movement and the seasonality". Agree. This is due to the occurrence of glacier ice at shallow depth. Note that on an inclined surface, the melt-out of the ice and the subsidence is also conducting to some downslope movement, which is not related to a deeper-seated creep process (permafrost creep).

AR: That is totally right. We added the following comment to the text: "In inclined areas characterized by surface subsidence, the horizontal movement might be affected by lateral shift during the subsidence process, which is why the surface movement cannot be distinctly related to permafrost creep and is therefore more independent from subsurface properties."

RC: l. 335 – Figure 6 – I don't understand the unit of the seasonality (mm/6 days). I might have missed the necessary explanation.

The elevation change values are the seasonal ones multiplied by three, or the actual ones during the summer time only (which would be preferable).

AR: All values use the summarized velocity during the snow-free period. In order to have comparable and consistent values for both, horizontal (e-w) displacement and vertical elevation change, we have applied the multiplication with a factor of 3 to both of them.

Regarding the unit for seasonality: Thank you for pointing out that this information was not included in the methodology section so far. We have added this information there: "Absolute seasonality is defined as the difference between median movement rates in June/July (early snow-free summer) and September (late snow-free summer) in mm/6d (6d = the observation window of one interferogram)."

RC: l. 340 – Figure 7 – Would it not make more sense to provide a seasonality value for both the vertical change and the E-W component separately ? Because one can assume that the former is mostly related to glacier ice melt (very high resistivity) and the latter to the permafrost (rock glacier) creep. To my understanding, the seasonality is mixing both component, no ?

AR: Thank you for pointing that out. In theory, a separate decomposition of seasonality for the vertical change and the E-W component would absolutely make sense. Buchelt et al. 2023 also showed in their analysis, that absolute seasonality shows strong spatial correlation with increased rates of elevation change, at least for some of their study sites. Therefore, we also did some tests to identify the direction of the seasonal acceleration, but due to the uncertainties in the DInSAR signal, the number of observations is too low to get a reliable result, that would be good enough to present it here. However, in the future, with more and longer time series data available, such a decomposition of the seasonal might become feasible.

Discussion

RC: l. 345 – Most of this section 5.1 is related to the methodological section (3) and should be moved there, because most of the challenges or limitations here discussed are known before the application of the methods or are linked to the conducted measurements themselves.

AR: We restructured to the two chapters and moved some parts from the discussion to the methods chapter.

RC: l. 389 – I would suggest to relate this section 5.2 more closely to the existing literature (see previous comments in the introduction section) and not only to refer to a few recent studies. In addition, it shows again the importance of disposing of the flow field data (already pointed out earlier).

AR: We added additional links to existing literature and included the information regarding the flow field into the discussion.

RC: l. 403 - I would avoid the use of "displacement", which could easily be understood as downslope movement due to deep-seated permafrost creep, but surface movement (or subsidence).

AR: We replaced "displacement" by "surface movements".

RC: l. 423-424 – "The area of uplift on the Pipji rock glacier (cf. Fig 5c) indicates compressive flow (cf. Haeberli et al., 2006) due to higher rock glacier velocities in the upper part and lower velocities in the lower units (Unit I & II)". I don't agree with this statement as this area of uplift is located at distance 300-400 m along the profile, what is 200 m upstream of the front of Unit III. There is just a tiny section of apparent lower E-W velocity, which, as written by the authors later, might be due to some unwrapping issues. The uplift might rather be due to the convergence of the flow field in this narrowing section of the rock glacier (again, which might be checked if this flow field is established).

AR: We agree in parts with your comment. Probably there is also convergent flow in the flow field which is important for the uplift. But, nevertheless, also the horizontal displacements are important. Maybe it is not visible quite well due to the scale of the map, but the area upwards of the uplifting area is moving slightly faster than the lower part. It is not so much, but there is a difference which might contribute to the mass gain in the uplifting area.

RC: l. 456-458 – "…, as so far no distinction has been made between thrust moraines and rock glaciers in many rock glacier inventories (Bertone et al., 2022; Nyenhuis, 2005; Rock glacier inventories and kinematics (RGIK), 2023)." This is a wrong statement. RGIK (2023, p. 9) specifies for the "glacier forefield-connected" category : the rock glacier develops within or from a (formerly) glaciated area. Interaction between the glacier or ice patch and the rock glacier feature is prevalent, but essentially restricted to phases of glacier advance (e.g., Little Ice Age). Embedded glacier ice within the rock glacier is possible. When receding, which is a common pattern today, the glacier has disconnected from the rock glacier or may have disappeared entirely. This category includes till-derived rock glaciers, which correspond to the classical debris rock glacier definition and to some push-moraines (glacitectonized frozen sediments)". It means that push-moraines (thrust moraines) are included in rock glacier inventories if they express the morphological criteria of a rock glacier, or in other words, if they are back-creeping or creeping outward of the glacier forefield and develop a distinct front. If they are only subsiding, they are disregarded. It shows the importance of treating the morphodynamics caused by creep and subsidence separately.

AR: Thank you for pointing out that our statement might be misleading. We clearly support the initiative of the RGIK community to standardize rock glacier mapping and monitoring. From a technical point of view, the inclusion of thrust moraines into the category glacier forefield-connected rock glacier does make sense. However, our main concern is that these landforms clearly show a different movement behaviour compared to other glacier

forefield-connected landforms. We simply want to point out that the selection of areas must be carried out very sensitively and critically reviewed, particularly if we want to extract a corresponding climate signal. As you also mentioned in a comment previously, even though such landforms might show horizontal movement, this movement might rather reflect downslope movement related to subsidence:

Your comment on *l. 328-329*: "*Note that on an inclined surface, the melt-out of the ice and the subsidence is also conducting to some downslope movement, which is not related to a deeper-seated creep process (permafrost creep).*"

Therefore, we would recommend excluding thrust moraines and areas of glacier forefield-connected rock glaciers underlain by sedimentary ice from RGV calculation, at least those that are affected mainly by subsidence.

RC: l. 459-464 – "As morphodynamical parameters such as rock glacier velocity are derived as essential climate variable (ECV) based on such inventories (Hu et al.,2025; Streletskiy et al., 2021; World Meteorological Organization (WMO), 2022), a lack of distinction between the different landforms may lead to a distortion of the parameters and thus of the assessment of morphodynamic changes in the context of climate change." The community having written the "Baseline concepts for Rock Glacier Velocity as an associated parameter of ECV Permafrost", available from the RGIK website (www.rgik.org), is fully aware about this issue. On p. 9, one can read that: "Rock glacier velocity time series must refer to a consistent flow field representing the downslope movement of a rock glacier unit or a part of it. Considered surface displacements should represent the downslope movement of the rock glacier related to permafrost creep and should not be significantly altered by local disturbing processes (e.g. movement of isolated boulders, ice melt induced subsidence). Areas affected by such local processes should be avoided for the measurement/computation of the time series". On p. 11 (see also Hu et al. 2025), RGV is defined as "time series of annualized surface velocity (...) of single or a part of rock glacier unit (...) and refers to observed surface velocities related to permafrost creep." It also means that surface movement related to subsidence must be excluded.

I could suggest the authors to alter their statement in support of the RGIK guidelines.

AR: See comment above. We regret if we did not express ourselves clearly. We definitely support the RGIK initiative. We just wanted to point out here that an explicit selection of representative areas must be carried out with care, and that areas characterized by subsidence (especially thrust or push moraines) should be excluded. We tried to clarify in the text and slightly adjusted the respective statements.

RC: l. 484-485 : "...distinguishing between thrust moraines and rock glaciers is essential due to their distinct internal structures and morphodynamic processes". Just note that the present study has only been undertaken on two thrust moraines and two rock glaciers and that the findings cannot be extended to all thrust moraines and rock glaciers.

AR: That is right, we clarified in the text.

RC: l. 487-489 : "Since many rock glacier inventories do not yet differentiate between these landforms, this omission may lead to inaccuracies in assessing morphodynamic changes, particularly when rock glacier velocity is used as an essential climate variable". See my comments above.

AR: See comment above.

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