ESURF – Submission of

***"Permafrost in monitored unstable rock slopes in Norway – New insights from rock wall temperature monitoring, geophysical surveying and numerical modelling"***

by Etzelmüller et al

## Comments to the editor and the reviewers.

Thank you for the detailed reviews, which we try to address in detail. Comments from the reviewers are marked in red italics, while our replies are marked in black.

We want to add one additional author: Dr. Tom Rune Lauknes from NORCE/Tromsø because of the reflector data we used, which were received from a third party and not published explicitly before. Dr. Lauknes had several comments, two references and minor additions which has been incorporated into the text, and which to a major degree also answered some of the review comments. Some more substantial comments are given below together with the review response for completeness.

Both reviewer 1 and reviewer 2 commented on both uncertainties (e.g. RMS errors etc) and partly discrepancies of the results and interpretation. To mitigate this, we included an additional paragraph in the discussion (Paragraph 5.1., see below). Finally, we changed the title slightly to better reflect the contents of the paper. It reads now:

*"Permafrost in monitored unstable rock slopes in Norway – New insights from temperature and surface velocity measurements, geophysical surveying and ground temperature modelling".*

**Initial changes by the authors:**

- we replaced GPS with GNSS (Global Navigation Satellite System) everywhere
- we replaces "crevasses" with "fractures" as the first mainly may be associated to glaciers.
- we added under 3.1:

"*Three satellite corner reflectors were installed at Gamanjunni3 in 2012, and their displacement are currently being operationally monitored using Sentinel-1 satellite InSAR*."

and

*" A time-series InSAR methodology, based on the GSAR software {Larsen, 2005 #4280; Lauknes, 2010 #3669; Eriksen, 2017 #4217} was used to estimate displacement rates.*

*From snow-free scenes with a revisit period of 11 days acquired from ascending- and descending satellite geometry, two stacks of interferograms were produced using GSAR. Mean yearly velocities from both TerraSAR-X stacks (2009–2014) were combined to a 2D InSAR displacement vector surface (12 x 12 m ground resolution) with enhanced sensitivity to displacement in the east-west up-down plane. For details on processing, verification and limitations see Eriksen et al {, 2017 #4217}."*

We added the following references:

*Lauknes, T.R., Shanker, A.P., Dehls, J.F., Zebker, H.A., Henderson, I.H.C. and Larsen, Y., 2010. Detailed rockslide mapping in northern Norway with small baseline and persistent scatterer interferometric SAR time series methods. Remote Sensing of Environment, 114(9), pp.2097-2109.*

*Larsen, Y., Engen, G., Lauknes, T.R., Malnes, E. and Høgda, K.A., 2005. A generic differential InSAR processing system, with applications to land subsidence and SWE retrieval. Proc. Advances in SAR interferometry from ENVISAT and ERS missions (FRINGE 2005), ESA ESRIN, Frascati, Italy, 28.*

*G. Majala, E. Anda, H. Berg, O. Eikenæs, G. Helgås, T. Oppikofer, et al. (2016): Fare- og risikoklassifisering av ustabile fjellparti Institution: NVE 2016 Report Number: 77-2016 Oslo: NVEDOI: https://publikasjoner.nve.no/rapport/2016/rapport2016_77.pdf*

## Referee 1 (Louise Vick)

I would like to congratulate the (extensive list of) authors for compiling and presenting a wide array of technical data, for more than one location. It cannot have been easy. In particular I really enjoyed the two-three lines of conclusion in every section, which helps to summarise or underscore the most important results. The manuscript can be difficult to follow by virtue of presenting so many different formats of data (time series, maps, profiles etc). It is good that the authors have taken care to clearly present and describe figures. The paper presents a lot of nice data and acts as a reservoir of sorts for information needed in ongoing rock slope studies.

I have made many small comments on grammar, figures, text, references etc in the attached pdf. In the beginning I made grammar suggestions, but soon switched to just highlighting text where the grammar is incorrect. There are also more important comments related to the content contained in the pdf, and in the interests of saving time have not bothered to write everything out again- please forward to the authors so that they may read them. I have described some of the more important comments here.

We thank Dr. Wick for the overall positive evaluation of our manuscript and the tremendous help and suggestion to improve it. We understand the manuscript might be difficult to follow, and we have tried to improve the readability.

In some parts of the text, mostly the discussion, the paragraphs are quite rambling and are not formed in a logical manner. For example, section 5.1. I am missing the point of all of these arguments until the last section. Perhaps if the arguments were better set out, more logical or more well developed these would make sense to me. I am also missing a clear explanation between permafrost changes and changes in geotechnical characteristics. This information is seen as accepted, but the discussion is too complicated to follow without fully understanding what the different relationships are. I am missing a clear explanation of the relationship between resistivity and permafrost. This is stated at one point the results, but in the discussion it is difficult to follow (e.g. line 814).

We have tried to clarify the points a bit more. We are aware, the manuscript is long, and important information is hidden here and there and easy to miss. E.g. the relationship between permafrost (thaw) and stability/geotechnical properties is summarised in the introduction, as an important base line for the hypothesis ("Permafrost influences rockslide dynamics"). The relationship between ERT and permafrost is introduced shortly in 3.4.

The comparison of different rockslides and their annual movement patterns is particularly difficult to follow. I have suggested a table to make it easier for the reader to keep track of the different patterns and how they compare to this study. The logic of presenting these comparisons, and threading into it the concept of reversible damage and 'rock breathing' further complicates the arguments for me. What is the main point of this section? The ideas behind why the slide moves more in late winter/spring are not presented until the conclusions (key word being pressure). It wouldn't be too hard to set these out. Also, what does water infiltration mean for the potential for permafrost presence?

Yes, you are absolutely right. The main part of this section was to review observations from similar settings than Mannen and Gamanjunni, a sort of review. We see that the "rock breathing" does not add any additional to the discussion, and we removed this part. The other examples are more related to seasonality, and we find it relevant for the discussion. Concerning the increased water pressure etc., we have written in the original manuscript:

*"The possible higher early spring and summer velocities and lower displacement rates during fall and winter might be related to high water input in the fractures due to snow melt causing hydraulic/hydrostatic pressures and contributing to the melting of ice/snow in fractures formed during the winter. During summer and fall, the fractures might be free of ice/snow at the end of the melting season and water infiltration might have less impact. The lower velocities at Mannen during years with lower snow cover (Figure 5f) also supports this interpretation."*

Concerning summarizing this in an additional table, we try to avoid this because the paper already is lengthy, but can include it if demanded by the editor.

The temperature modelling of the sites seems to be in direct opposition to many of the conclusions drawn, including thickness, depth and extent of frozen areas taken from the geophysical surveys and temperature records. Also, in the presence of the permafrost all together as opposed to only local patches (e.g. as presented for the first time in the discussion lines c. 725-735). It may be that I misunderstand the interpretation of the modelling, but then this means the model results and their relationship to the presence of permafrost should be better explained.

Well, yes and no. We have rephrased and reconstructed the discussion to make things more clear. We added a new paragraph in the discussion ("*5.1. Hypothesis and uncertainties"*), which defines the hypotheses, and elaborates about uncertainties, and how the different observations and methods has to be interpreted. The paragraph now reads:

*"5.1. Hypothesis and uncertainties*

*Both Gámanjunni-3 and Mannen are considered as high-risk unstable slopes {Hermanns, 2013 #3893; Majala, 2016 #4282} and are continuously monitored. The movement was initiated several millennia after deglaciation, thus climatic changes have been discussed as a factor influencing the dynamics of the instability (Hilger et al., 2021). Cosmogenic nuclide (CN) dating shows that the current displacements of the slopes is larger than the Holocene average (Böhme et al., 2019;Hilger et al., 2021), indicating atmospheric warming as a likely influencing factor. For both sites, we therefore hypothesise that permafrost warming and/or degradation might be a substantial explanation for the temporal displacement pattern.*

*Our study combines a variety of methods, ranging from point observations (e.g. rock wall temperature measurements) via local surveys (e.g. geophysical measurements) to larger-scale modelling along an entire slope setting (e.g. geothermal modelling). Each method has uncertainties, and includes different pre-conditions, representing many pieces of a puzzle to form a consistent picture. For example, the heat flow modelling represents the lower extent of possible permafrost at Gámanjunni-3 as indicated in the ERT surveys, but does not represent local thawed areas and variations indicated by the ERT.*

*The methods used are independent of each other, and may contradict in places, but result in an overall explainable pattern. The temperature modelling does not account for fractures and other structures in a rockslide area (e.g. where water can penetrate); Cryogrid-2D is a two-dimensional model purely based on heat conduction, which results in a smoothed and simplified version of reality {Myhra, 2017 #3967; Myhra, 2019 #4134}. The ERT profiles were measured in rough terrain, both the resistivity values and data noise are very sensible to cracks and fractures, strong topographic variations, or local water penetration. This leads to a high variability of resistivity and may produce inversion artefacts, such as in the transition between plateau and rock wall at Gámanjunni (Figure 10), which is also represented by the partly high root mean square errors (RMS) of the inverted tomograms, which vary between 6 and 20%. Therefore, comparing ERT and a* *more large-scaled temperature model is not meaningful on a local site level. Our ERT and 4PM results for Gámanjunni clearly show potentially frozen areas within the rock slide area, disappearing down-slope. The potentially frozen areas are at a depth of c. 30-50 m, which is close to the results of the temperature modelling at these places. However, locally thawed areas in the rockslides and possible taliks as indicated in the ERT are certainly realistic, but cannot be covered by the simplified heat flow model."*

The main conclusion- from line 818- is not something i would draw from both the results and the discussion. This conclusion (Our study suggests a coupling of permafrost development and landslide dynamics) contradicts the line from 777?

Good point, we deleted the last point as it was not that important here. It was a sort of speculation.

Geophysics at Gam-3 is missing the interpretation of signals around the rock glacier- this information is promised earlier on but not delivered in the results section.

This is addressed below. There is no 4PM modelling over the rock glacier, only one ERT profile.

The mapping of the rock slide features at Mannen is missing. It is difficult to place any of the results into context for a reader unfamiliar with the site. It is also difficult to place the geophysics in context without a better map and a map inset in the results section scrolling between figure 1 and figures 10-12 makes interpretations much more difficult for the reader.

We have added inlet maps for the figures 10-12, and make it easier for the reader.

Date ranges for monitoring are difficult to find and follow. It would be easier if these are stated absolutely everytime they need to be referenced.

*We provided some more mapping data, and made inlet maps for the ERT profiles.*

Corner reflectors- are these for satellite insar? This is not clarified. Terrasar-x is at some point referenced, but not when expected in the methods, and no limitations are put on (potential) satellite-insar derived data.

This is more carified now in the text (see comment in the introduction of this reply).

**Selected comments from the PDF manuscript**

Reviewer 1 provided numerous comments and suggestions directly in the PDF of the manuscript. Larger changes are discussed below, small annotations, corrections and typos are all accepted and changed in the revised version.

I think you under-sell the paper in this abstract.

We see that point, we revised the abstract and added some more substantial information.

Figure 1: Just a comment that these maps are hard to read. Could help to remove the grid lines since you already give lat long in the above figures. Also why are there mapped lineaments for Gam but not Mannen presented? I assume you explain ERT and GT at some point but not clear here. Also PF prob, GST and BTS classes.

There was a similar comment from Reviewer 2, and the figures are revised a bit to ease readability. Concerning the lineaments in Mannen, we lack an updated and reliable data set of those at present. The lineaments are not that important to the discussion and message of this manuscript, so this is hopefully ok. We also removed the very simplified inlet figure by Dahle et al., as it is very simple and not necessary correspond common knowledge. However, we do not have a replacement yet. As with the lineaments, we think it is not crucial for the study.

Figure 2: You state three scenarios in the above paragraph

We have revised this, se updated Figure 2

Paragr. 3.1.: I am missing a definition of the corner reflectors. What are these? Satellite insar calibrations, or also for the GB InSAR?

This has been addressed earlier in this response letter, see above.

Paragr. 3.3.: I assume sampled from the surface? And does spatial variation in lithology affect these resistivity results? Gam-3 is alternating in lithology throughout the slope?

Yes, the blocks are sampled from the surface. The Gámanjunni sample are taken from the slope of the sliding, the Mannen samples are from the plateau surface behind the back scarp.

Concerning varying lithology and representative samples, here we mean that we took samples from the site that represent the variety of lithology from the sites. This is also the reason why we included a sample from Nordnesfjellet. We wrote (original line 255):

*"The anisotropy due to foliated minerals accounts for certain deviations in the measured laboratory arrays. These correspond to variations in the field where small-scale changes of meta-sediment rock types appear."*

So, we think that the choice of the samples is valid for interpreting the field measurements.

l. 325: But what is the survey actually? Just a probe into the snow pack, or something you install? State the instrument

Ok, BTS is a standard method within permafrost science. We added some sentences describing the approach. It is quite simple, but well-documented in literature. It is a field method with point measurements.

*"The survey was done using a long stick with a thermistor mounted at the bottom. At each site, is penetrated through the snow, and the BTS temperature is registered using a standard multi-meter. At least to measurements are carried out at each site to address small-scale variability."*

l.419: what do you mean by transition between moving block and rockslide material?

*Sentence was re-phrased.*

l. 421: Is this important? Then perhaps you could state for the permafrost-illiterate what you mean by this and how you came to this conclusion.

See the paragraph about the BTS measurements; temperatures below -3°C indicate a high probability for permafrost presence at the BTS measurement site, there is a large literature around this observation. This is a good and independent indication that there is permafrost around Gam-3. So, we need this sentence.

l. 451: Are you meaning corner reflectors for satellite insar, or something else? unclear.

The sentence now reads:

*"The site's three satellite corner reflectors provided a continuous data series since 2015, based on Copernicus Sentinel-1 data."*

l. 466: First time we hear of TerraSAR-X data?

Yes, we included in paragr. 3.1. the following sentence:

*“Satellite based interferometric synthetic aperture radar (InSAR) using TerraSAR-X data (2009–2014) was used to retrieve high-resolution information about surface displacement.”*

L 479: What are you defining as the lower permafrost boundary? The solid black line at 0 degs? This seems to contradic your earlier statement that half the rockslide is now outside of permafrost. And what do you mean by the conclusion about deep-seated permafrost? Unclear

We see your point. We have a bit rephrased the paragraph. This is a model result with simplifications, where the goal is to show a possible distributional scenario of permafrost under the unstable slope. Yes, the thick black line is the 0-degree isotherm and denotes the transition between permafrost and no permafrost in the model for a certain set of parameters (air temperature forcing. snow cover etc.). The supplemental figure shows results for warmer or cooler settings as we do not know that back in time exactly. The major message we wanted to show is:

1. There has been a warming
2. The warming is of course more fast close to the surface
3. We have a transition from permafrost to no permafrost in the rockslide body, and today around 50% is covered (a bit more in this run shown in Figure 7, a bit less if T is 1°C warmer, see appendix)
4. We can assume deep permafrost in places, which might influence e.g. deeper fractures etc.

Figure 8: (comment from a permafrost idiot)... doesn't this 'potential' data directly contradict your model? You are showing thawed rock in places where you have thick layers of -0 temps in the model. Or is it that frozen ground and temperature are not considered indicative of one another?

We have commented this “mismatch” during the general comments above. We have stated in the text (5.2.) that ERT and model result not necessarily coincidence locally:

*“This interplay, together with air and water advection in crevasses produces a complicated thermal pattern, which is not reproduced by our heat flow modelling.”*

l. 598: clarify why 300 m is considered an appropriate thickness in the model, or better explain the model

The 300 m refers to the modelled maximum thickness, which is derived from under the plateau. The model indicates today a thickness of below 100 m in the central and lower parts of the instability.

l. 603: what is meant by decompaction here? dissagregation of the rock mass, or decompaction of the material (decompaction implies expansion of sorts)

Yes, this is meant in terms of expansion (or opening of voids etc).

Figures Geophysics: With all of these geophys figures it would be nice to have one inset to see the spatial context (e.g. hillshade or topo with survey trace overlaid) to keep up with the context- otherwise there is a lot of scrolling between figure 1 and figs 10-11

The figures has been revised, and we tried to indicate locations etc.

**Figure 11**: I am missing information about the changes around the rock glacier area, and annotations on NVE-2

The rock glacier area is not covered by the 4PM modelling (combined ERT and seismics), we only have one ERT there (see Figure 8, where the 4PM area is indicated). In the description of the ERT results (4.2., Figure 8), we wrote:

*"The overall resistivity values within the rock glacier are lower (10-20 kΩm), and the more resistive surface layer is somewhat shallower (c. 25 m) compared to the rockslide part in the centre of the profile."*

Can the height of the GW table be commented on so as to decouple rockslide and regular flow processes?

Well, probable difficult. This is a quite coarse spacing, so depicting GW table from these soundings could be difficult.

Seems planar- how does this linear feature fit with the presented sliding surface?

The zone interpreted as potential fracture zone in the tomograms is – compared to the overall length of the ERT profile – a local anomaly with an extension of ~100 m. Even if it seems to have a planar lower boundary in the 4PM result, we would rather interpret it as a local anomaly with higher fracturation than in the surroundings, but without giving too much weight to the exact geometry of this anomaly

Hauck & Hilbich 2018 and : Better to put this 4PM information in the text?

The 4PM method is based on Hauck et al 2011, and is shortly mentioned in the main text. We also give a more thorough background in the Appendix C. Hauck & Hilbich 2018 is a consulting report to NVE, which is available on the NVE web site:

https://www.nve.no/media/7646/report-gamanjunni-4phasemodel-unifr-2018.pdf

and can be studied there. Our results and figures 11 and Appendix C are based on that report.

l. 631: the backscarp is never presented for Mannen

It is presented several places (e.g. Figure 2 etc), but probably not formally introduced as back scarp, but defined as “slip suface” in the setting, we added the word “back scarp” there. We also see a bit confusion about localization, and added an image in Figure 2, highlighting the back scarp and the (often snow-filled) crevasse below.

l. 637: again, state the measurement period to give context to these relative statements

done

l. 697: from memory didn't Böhme et al prove that there was a phase of faster movement earlier? They relate it to HTM

Yes, but recent displacement rates are still higher than those estimated based on the CN datings, according to Hilger et al.

l. 700: I am missing how the presence of permafrost, and the modelled reduction in extent, relate to the current displacement. What is the link here? Build your argument.

Ok, yes, we have re-structured the discussion section. We moved the first introducing paragraph directly under and the main discussion heading, and subsequently we try to build up the argumentation. First, we discuss permafrost presence and dynamics. With that in place, we try to discuss the possible coupling of permafrost and short and long-term displacement rates.

l. 721: why does it indicate that? How is warming of only the south rock wall (and here i am assuming the back scarp but it is not clear) connected to permafrost degradation at the site in general?

This sentence was not good, we have re-formulated the passage, to make it more clear. The south-oriented wall is warmer than the north in general, the warming there triggered a change from sub-zero to above-zero annual average temperatures since 2000. It reads now:

*"For Gámanjunni-3, MAAT have risen over the last 140 years, and since 1880 the rise was around +1.8°C. Estimated rock-wall temperatures in all orientations have been mostly negative between 1880 and 2020. Since c. 2000, however, the south-oriented rock wall showed mean annual temperatures close to or above 0°C (Figure 4c). Permafrost warming and possible degradation might have accelerated since c. 2000, which could influence the geotechnical properties of the site."*

l. 736: are these conclusions not directly opposing your modelling? This factor seems to be ignored.

No, we do not think so. What we state is that we have permafrost, we have mote on Gamanjunni (discontinuous) than on Mannen (sporadic) and that ground temperatures have warmed significantly since 1880. The thermal model does support this clearly.

l. 784: it is not clear to me what these last two sentences mean? And deformable ice, rock material or rock mass?

Sentences deleted….

l. 794: is this relevant (shear strength argument) for rock glaciers? Since it's movement is not along bedrock fractures as I understand but rather across shear bands within loose material

No, the movement of rock glaciers is related to deformation of ice layers within the landform. Especially Swiss colleagues, who have drilled and logged several boreholes in rock glaciers, have demonstrated this. There is quite an extensive literature about rock glacier rheology and movement. So, the shear strength argument is related to ice layers facilitating rock glacier movement.

l. 810: Do you mean in the subsurface extension? and l. 811: Are you now talking about a new rockslide developing? Unclear and somewhat out of the blue...

This passage is actually misplaced there, and we removed these two sentences.

l 843-850: this section could be aided by a table of the various rockslides presented here and their seasonal movement patterns

This is certainly a good idea, however, the manuscript already has extensive with figures and tables, so it maybe is stretching a bit the length. We would like to avoid this, but if the editor insists, we can prepare such a table.

l. 853: cracks in rock? Was the rock failure 100000 m3? What failed. Context!

We have re-phrased the sentence based on Phillips et al 2017, and reads now:

*"These observations agree also with other studies, e.g. Philips et al (2017) report on 6000 years old ice derived from tension crack at a rock pillar. The rock pillar collapsed in 2014, and had a volume of around 150000 m3."*

l. 857: this is a repetition

Sentence deleted here, we kept the next "repetition", as it is in a summarising context.

l. 860: permafrost deg or warming is accelerating in the north?

Sentence has been changed, it reads now:

*"This warming trend has been documented all over Europe (Etzelmüller et al., 2020), and is responsible for permafrost degradation in Norway (Borge et al., 2017), possible influencing both rock glacier velocities and triggering of landslides (Eriksen et al., 2018; Frauenfelder et al., 2018)."*

l. 873: This conclusion does not naturally follow from the discussion on rock glaciers

the paragraph is changed a bit following comments from Review 2, and we also deleted the last sentence.

l. 880-882: this conclusion is not something i would draw from both the results and your discussion

Here we slightly disagree. We think our results presented in this manuscript justify this conclusion point, so we would like to keep it as it is.

## Referee 2 (Oliver Sass):

*This is a very interesting and inspiring publication presenting a wealth of data, which work well together to provide a clear and consistent picture of rock slope dynamics influenced by warming permafrost. The paper is well-written and to the point. The figures are very complex and on the whole, there is more data presented than can be "digested" in a standard-length paper. But I understand that the authors like to present all the data together, even if this sometimes makes things confusing for the reader. I recommend the final acceptance of the paper after some minor revisions.*

We thank Dr. Sass for the overall positive evaluation of our manuscript. The paper could of course cut into two or several pieces; however, the overall discussion then would have been lost.

Fig. 1: Add "A" and "B" in the upper left inset

Clarify the relation between upper middle and upper right (took me some time to figure out). The red circles in these insets are somehow confusing - the caption says that these are climate stations, while the legend says these are warm BTS sites and "met. stations" would be in yellow. Insets "B" and "C" both are extremely busy, I found it hard to tell where top and bottom of the slope are, and where the landslides actually are. This is partly due to the brownish background, but mainly due to too much information.

The figure is re-worked, hopefully addressing your points satisfactorily.

line 213/214: six temperature data loggers in total; 3 loggers in the back scarps of Gámanjunni; 2 loggers at Mannen - where is number 6?

changed to “five loggers”, number 6 is on the other side of the valley in a north-facing slope, and bot directly at the Gámanjunni site.

line 445: please explain briefly what a "failed rockslide" is

we added “recently-failed rockslide”

line 446: "delineated by the dotted red line" - I cannot find such a line, do you mean the lilac one?

yes, we changed to a red line which is better visible.

Figure 9: An RMS error of 19.9% is given. I know well that in areas of high resistivity contrasts, errors are usually quite high. However, 20% is a lot, and it is never discussed how this might affect the validity of the conclusions.

What is more, no RMS errors at all are provided for all the other ERT profiles. Even if the RMS is not the only criterion for the reliability of an ERT section, it needs to be shown and discussed.

We added RMS errors for all sections in the relevant figures. We also added maps showing positions of the profiles, as requested by review 1. We discussed a bit the RMS values in a new section of the discussion (paragraph 5.1., see above).

Fig. 10 is a good example for the missing error discussion. The odd patterns in profile GEDY-2 at around 450 m (plateau crest) seem to me to be typical for unrealistic patterns which emerge in an ERT section under the influence of high contrasts and pronounced topography. The section (C) is not very helpful and seems to cover part of (A) - should be left out.

Yes, there are of course artefacts related to the steep topography, and the overall pattern is influenced in it. We added also a bit around the artefacts because of the steep topography. In the results and the discussion, we always refer to the overall pattern rather special locations, especially in the rock wall transitions, which are influenced by the topography. We try to make this more clear in the revision.

line 814: "highest velocities and lowest ERT values" - shouldn't it say: highest ERT values?

yes, changed.

line 851: "Philips et al. indicate...": WHERE did they find 6000 yr old ice? In the study area? Somewhere completely different?

According to their paper they have carbon-14 (14C) dated organic material in possible permafrost ice originated from a tension crack. This is interpretation, however, as they dated ice found in the rock fall material. However, page 432, first paragraph.

line 860: "accelerating in the north" - what does this mean? Rephrase

changed to:

"*This warming since 2000 has been documented all over Europe (Etzelmüller et al., 2020), and is responsible for permafrost degradation in Norway (Borge et al., 2017), possible influencing rock glacier velocities and triggering of land slides (Eriksen et al., 2018;Frauenfelder et al., 2018)*"

line 866 ff: "can accelerate into the future"

Consider looking at the many publications of Kellerer-Pirklbauer on rock glaciers in the Austrian Alps. Temporary acceleration due to warming of the ice in rock glaciers is quite common, and might be followed by a stop of any movement once the ice has actually melted. This should not be mixed with the behaviour of rock slopes where ice melt can lead to acceleration or collapse.

line 870: Again, rock glacier and rock slope are mixed here. Keep this apart!

We agree, we added a sentence that the rock glacier movement may stop when ice is melted out or super-saturation of ice ends. We also separated thisse sentences as own paragraph.

line 886: "Increased displacements rates are associated with lower ground temperatures and higher ground resistivity...": This sentence is somewhat misleading. Lower ground temperatures well below zero would probably reduce displacement rates. That's what happened during the Holocene. The velocity increase at lower temperatures at Gámanjunni is due to the fact that areas of subzero temperatures move faster than areas of above-zero temperatures, because there is still ice that can warm up, deform and finally melt. Once the ice has melted, the temperature-displacement relation will probably disappear or reverse.

line 887: Here you give the corresponding explanation to line 886. Make it clearer in the previous sentence that acceleration occurs around the freezing point.

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Yes, that what we meant, we re-phrased a bit to make the relationship more clear. The bullet point now reads:

"*Displacement rates of Gámanjunni rockslide co-vary significantly with sub-surface resistivity and modelled temperature. Increased displacements rates seems to be associated with sub-zero ground temperatures and higher ground resistivity. This might be related to the presence of ground ice in fractures and pores close to the melting point, facilitating increased deformation.*"

line 893: "The movement mechanism seems to be different for both systems..." (rockslide/ rockglacier). Of course it is different! These are two totally different types of landforms with totally different dynamics.

*Yes, re-phrased.*