

## Answers to RC1

**RC1:** The paper “Permafrost distribution in steep slopes in Norway: measurements, statistical modelling and geomorphological implications” by Magnin et al. presents a new rock permafrost model for mainland Norway and draws conclusions of permafrost distribution in rockwalls on current rock instability and landform development. The authors installed more than 25 rock temperature loggers in 8 key regions and used a sophisticated model approach to upscale their findings on the rockwall thermal regime into a regional rock permafrost model. Rock permafrost in Norway is responsible for a large number of rock instabilities that currently threaten infrastructure and inhabitants.

**Authors’ answer:** *At the current stage, the role of permafrost as a main cause of rock wall instability in Norway is not clear, and neither the authors nor other studies clearly claim that permafrost is responsible for a large number of rock instabilities, especially in Norway. Our study does not draw any conclusions “on current rock instability and landform developments”. Our conclusions are all related to the outcome of the rock wall logger analysis and upscaling of the data to a national scale using statistical methods (see P15 – L5-29 of the submitted version: there are no conclusion points stating about rock slope instabilities and landforms).*

**RC1:** In the past, these instabilities caused hundreds of deaths and the knowledge of rock permafrost distribution as provided by this paper is required to mitigate ongoing and future landslide hazards and risks. Therefore, the importance of this work is very high. Unfortunately, the link of permafrost distribution to rock instabilities is poorly addressed, ....

**Authors’ answer:** *Yes, this is right. But, as mentioned above, the goal of this paper is not to address the link between permafrost and instabilities. This is not feasible with the existing data and our approach. However, we believe that our study is an essential step towards assessment of the link between permafrost and instabilities, and this could be addressed in future studies.*

**RC1:** ... the use of chosen parameters of the model are incompletely explained, ...

**Authors’ answer:** *in the revised version we added a paragraph in the introduction about rock slope permafrost modelling which explains our choice in terms of model parameters.*

**RC1...**results are insufficiently presented and compared to non-connected landforms (moraine-derived rock glaciers) instead of existing instabilities.

**Authors’ answer:** *Since we do not address the role of permafrost for landform developments and rock slope instabilities, we have not shown anything about possible relations pointed out by RC1 in our results. We included the mentioned points in this study with the intention to show that our results could be used to address landforms and instabilities and therefore hint at possible applications to address geomorphological questions in our discussion. In addition, we never use the term “moraine-derived” for the rock glaciers inventory that we introduce in the manuscript.*

**RC1:** In addition, the reader needs knowledge on Norwegian locations to understand the research set up.

**Authors' answer:** *Yes, we noticed indeed that some terms we use may confuse some readers not familiar with the Norwegian settings and that we also referred to locations that are not clearly displayed in any of the maps. We have corrected this following the details comments. We have removed specific terms that were not essential to the study (such as "Scandes") and made sure that we only referred to locations displayed in Figures.*

**RC1:** In current state, the paper focuses on permafrost and lacks on geomorphology and would be better suited for a journal focusing on periglacial phenomena than Earth Surface Dynamics. However, a revision which address the shortcomings would improve the manuscript and the suitability.

**Authors' answer:** *As mentioned above, here we strongly disagree with RC1, based on our introductory statements above. We notice also that neither RC2 nor the handling associate editor gave us a similar opinion.*

**RC1:**

- 1) Link between permafrost and rock stabilities. Permafrost affects rock stability, however, this effect can be both positive and negative, thus, permafrost affects driving and resisting factors as previously discussed Krautblatter et al. (2013) and Draebing et al. (2014). Permafrost aggradation for example following the LIA causes cryostatic pressures (Wegmann et al., 1998), however, this does not provoke ice segregation and large rock slope failures as suggested by the authors. Ice segregation can be amplified by permafrost when active-layer thaw increase rock moisture that can migrate towards the freezing front at the top of the permafrost as identified by Murton et al. (2006). However, this effect is limited to the upper 20 m of rock depth (Krautblatter et al., 2013), thus, the normal load of the overlying bedrock would counter the effects of ice pressure. Therefore, ice segregation cannot cause rock slope failure with shear planes below 20m depth. The authors are not addressing further effects of permafrost on instability. The instability of permafrost rockwalls is also affected by active-layer thaw that can cause small-scale rockfall as conceptually discussed by Draebing et al. (2014) and derived from rockfall inventories by Ravanel et al. (2010; 2017). Permafrost warming and degradation increase instability as mechanically described by Krautblatter et al. (2013) and can result in an increase of rockfall activity (Ravanel and Deline, 2010). Due to these findings, several authors discusses a connection between rock slope failures and quaternary climate fluctuations in Norway (Hilger et al., 2018; Matthews et al., 2018). The authors should include these findings in their introduction and in the discussion of their results. Beneath these permafrost effects, rock slope stability is controlled by paraglacial effects which are non-glaciated processes conditioned by former glaciation as the authors mentioned. McColl (2012) and McColl and Draebing (2019) recently reviewed paraglacial effects on rockwall stability and connection to permafrost dynamics. Oversteepening of rockwalls results in stress redistributions and can prone rockwalls towards instability. Thus, areas affected by permafrost are very often also affected by current or

former glaciation, paraglacial and periglacial effects are hard to decipher. This can become even more complicated as paraglacial adjustment can work over more than one glacial cycle (Grämiger et al., 2017). The author introduce vaguely paraglacial effects without including any up-to-date literature or discussing a potential influence. The problem of deciphering periglacial and paraglacial processes should be addressed in the discussion.

**Authors' answer:** *Thank you for the detailed comment. We re-wrote the paragraph to make it more accurate and aligned with the relevant literature (see the revised introduction), including some of the points here mentioned. However, the given explanation here goes far beyond our paper's concern, and we kept a concise paragraph. Additionally, we didn't not address the following comment: "problem of decipher periglacial and paraglacial processes". This is worth a paper alone, and we can have a long discussion about "periglacial" and "paraglacial" and other semantic issues, and as mentioned in the beginning, we think this is not the objective of our manuscript.*

**RC1:**

- 2) The modelling approach The authors a priori chose a slope angle threshold of 40° to identify steep rock slopes. There is no geomorphic argument why this threshold is chosen. Previous models by Hipp et al. (2014) and Steiger et al. (2016) chose a threshold of 50° and 60° for steep rock slopes in Norway. Before extrapolating the results to entire Norway, the authors should try to evaluate their threshold. They can map rockwalls from orthophotos for small areas or data subsets and compare them to rockwalls derived by their threshold approach to test the sensitivity of their model.

**Authors' answer:** *Thank you for this comment, which is totally correct. Depending on the study such threshold varies because there is no strict DEM slope value above which a real-world steep slope is accurately represented on a DEM. Furthermore, it does not exist a scientifically-based threshold. The main characteristic of rock wall permafrost is the absence of perennial, continuous and insulating snow cover in winter. However, as mentioned in this comment, this can happen locally, not continuously in space, in slope up to 75°. If we take slope > 75° on the DEM we use, there will be almost no slopes left, because as all DEMS, it underestimates slope angles due to data interpolation. In a dem, true vertical doesn't exist, as the walls have a certain horizontal extent, even if they are vertical. In addition, a systematic comparison of a DEM slope angle to the real-world slope angle is not feasible over a whole country and possible comparison at the local scale will be only valuable for the considered area due to varying quality of the DEM over space. It has to be noted that in the case of vertical walls, the aerial fraction within a km<sup>2</sup> would be zero, even though it could still have lots of walls in this area. We therefore decided 40° as it is rather conservative: it includes mid-steep slopes more or less affected by snow but does not exclude steep slope areas by taking a higher slope angle. This involves that some slopes considered in our study may be affected by snow deposit and this is why we provide interpretation-keys together with the permafrost probability value. In the current state, it seems like the best possible option and we have explicated this choice and its limitations in the revised version. See 2<sup>nd</sup> paragraph of section 3.4 in the revised version.*

**RC1:** Also they could compare their derived rockwalls with the location of instabilities mapped by Oppikofer et al. (2015).

**Authors' answer:** *Yes, this would be a nice addition. Unfortunately, this is not feasible: see P13 L30-35 and P14 L1-2 of the submitted version. It is explained that the instability database gathers many types of instabilities (not only steep rock slope instabilities), that they are inventoried by a point at the middle of the slope (not necessarily in its steepest part). Therefore, we did the best we can, taking into account data limitations, which is still relevant to hint at possible research perspectives.*

**RC1:** The rock temperature loggers are installed following the approach by Gruber et al. (2004) which choose the steepest part of the rockwall to limit effects of snow accumulation. Therefore, the setup excludes snow cover, which can be present even in rockwalls with a slope angle up to 75° (Haberkorn et al., 2015a; Haberkorn et al., 2015b; Haberkorn et al., 2017; Phillips et al., 2017). Figure 5 shows that large rockwall areas are covered by snow cover. A coarse DEM with a resolution of 10 m will smooth out ledges that enable snow accumulation (Draebing et al., 2017; Haberkorn et al., 2015a), therefore, the chosen resolution will limit the effects of snow cover. Snow cover is also highly heterogeneous in space and time, which makes it very difficult to include in modelling approaches. However, the author should mention and discuss this shortcoming of the model resulting from chosen logger locations and DEM resolution.

**Authors' answer:** *Again, RC1 rises a valid and important point. In a local scale, snow is an important moderator of ground thermal regime even in steep rock walls with ledges, and overall slope ruggedness. However, in our regional approach, whatever the DEM resolution the snow effect will not be accounted for. Independent of the DEM resolution, the model is either able to consider snow (e.g. physic-based model for site scale), or the model does not account for snow, such as in our approach (statistics, appropriate for large spatial coverage) and in that case the capability of the DEM to represent ledges has no influence. We did mention and discuss the lack of consideration for snow effect in our study, see in the explanation of our modelling approach P7 L30-32 and P8 L1-6 and in the discussion P 12 L3-8 and 13-17. This is why we provide interpretation-keys with the probability map, to consider the snow effect as at the current stage, this is the best we can do for such statistic-based approach. It has to be noted that the Norwegian conditions are very different than the conditions in the Alps, and that studies conducted on snow control in the Alps cited in this comment are not strictly transferable to the Norwegian case. Indeed, due to the dominant control of solar radiation on the permafrost distribution and dynamics in the European Alps, the snow albedo strongly controls the snow effect, especially on South faces (Magnin et al., 2017b; Haberkorn et al., 2017). In Norway, the snow control on steep rock faces has not been investigated yet quantitatively, and therefore we limit our explanations and discussion to what is possible to say. We better explained how we handle the lack of consideration for snow in the revised version, first in the introduction (3<sup>rd</sup> paragraph), section 3.1 2<sup>nd</sup> paragraph, First 3 lines of section 3.3. section 3.4 2<sup>nd</sup> paragraph, section 3.5 3<sup>rd</sup> paragraph, section 5.2 2<sup>nd</sup> paragraph.*

**RC1:** In their model, the authors simulate PISR using GIS. It is unknown which latitudinal location and the time period they chose to run the PISR algorithm provided by ArcGIS. Solar parameters show large changes between North and South of Norway and model results should reflect this. The author should therefore provide more information on the modelling approach and how they incorporate differences within their data set.

**Authors' answer:** *We provide details (including time period and time resolution) about the PISR calculation in P6 L24-35, P7 L1-3 and 5-7. The latitude is intrinsic to the DEM, therefore for each grid cell the specific latitude is attributed and considered in the calculation. This is why there are great difference between Northern and Southern Norway, and that this is reflected in our results: P9 L14-35, P10 L 1-6 for example, based on Figure 9. This issue is also further discussed P 11 L21-35.*

**RC1:** The authors classify permafrost occurrence based on bedrock setting. They refer to Figure 5 where they highlight three areas and suggest different fracture properties in these areas. From the photo alone, fractures or degree of fracturing is not visible. It remains unclear where the fracture information comes from for e.g. local sites or even entire Norway. However, the authors use this information to classify permafrost into isolated, sporadic, discontinuous and continuous permafrost based on a permafrost classification scheme for the Arctic. In Alpine areas topography has strong control on permafrost distribution and the use of this scheme is limited. The same authors use a permafrost probability approach in the Mont Blanc Massif (Magnin et al., 2015; Ravelin et al., 2017), which is better suited. The authors connect this scheme somehow to slope ruggedness and fractures but it is completely unclear where they derive the information from necessary for the classification. If you apply the classification to the rockwall in Figure 5 and assume the fracture properties are correct discontinuous permafrost can be located in direct proximity to isolated or spontaneous permafrost. It would make more sense to model rockwall permafrost and compare every pixel to its neighbouring pixels to identify isolated, sporadic, discontinuous and continuous permafrost.

**Authors' answer:** *In this case we think RC1 may have misunderstood. We classify permafrost occurrence according to permafrost probability: P7 L22-25 of the submitted version and also see Figure 9 for example. We refer to Figure 5 to give an example of how to interpret the permafrost probability according to the bedrock settings. It is the map user who is supposed to interpret the map according to the bedrock settings in his/her area of interest, and Figure 5 only gives an example of possible interpretation according to the state of the art knowledge. However, we admit that using the term "fractured" might be confusing as we associate to surface slope angle and ruggedness, which are not strictly linked. We therefore re-wrote the legend of Figure 5 to avoid such confusing terms and developed our conceptual approach to interpret the permafrost probability map in more details in the 2<sup>nd</sup> paragraph section 3.5. Furthermore, the studies referred in this comment use a similar approach as in this paper: calculation of a statistical distribution first, and interpretation afterwards. But to be precise, in our study we use a slightly different approach than in the referred studies, we calculated an index while in the present paper we calculate a probability. In both cases, our approach is based on the state of the art relevant for the investigated area.*

**RC1:** Rockwalls are not uniformly distributed in Norway and conclusions on rock permafrost occurrence cannot be used without normalization. Differences between East and West Norway are caused by a decrease of rockwall occurrence and not permafrost occurrence. Other periglacial landforms are abundant in the east and the authors cannot conclude on permafrost distribution without normalization.

**Authors' answer:** *We think that this is a highly relevant idea, but the argumentation merely denies what we wrote in the submitted version: "Differences between East and Western Norway are caused by a decrease of rock wall occurrence, not permafrost occurrence". This is exactly what we meant P11, L30-32: "The number of permafrost observations decreases with continentality as most of the rock walls are found close to the coast ....". Maybe the fact that we did not precise "the number of **rock slope** permafrost occurrences" have confused RC1 who thought about "permafrost observations" in general? As it is our results section focusing on rock slope permafrost we thought it was not necessary to mention "rock slope" but we added this precision in the revised version.*

*Additionally, normalizing the results will fade the topographical control (rock wall occurrences and elevation) on our results, which is definitely not what we wanted to show. We think that this is very important to show that permafrost distribution is also a results of the topographical settings, and this is why we preferred to show all observations in Figure 10 for example rather than normalized results. In this study, we do not use rock walls as permafrost indicators, which in that case would make sense to normalize the results before using rock walls to map permafrost distribution. And we also explain in the submitted paper that the very few number of rock walls occurrences in Eastern Norway makes a statistical analysis poorly relevant, including normalization. We chose what we think is the most appropriate way to express our results and therefore avoid normalization.*

**RC1:** Comparison to rock glaciers and the use of instabilities The authors compare their permafrost distribution to other landforms such as rock glaciers and found a strong local connection to moraine-derived rock glaciers. Areas affected by rock permafrost are very often previously glaciated and inhabit other periglacial and glacial landforms. Permafrost rockwalls can produce material that can accumulate on snowfields and with time, the material can develop into a talus-derived rock glacier. The permafrost develops when debris-covered snow develops into ice via ice metamorphosis. The permafrost in the rock glacier has no causal connection to permafrost in the rockwall. Moraine-derived rock glaciers are developed from creeping former dead ice and is more connected to previous glaciation. Due to a lack of connection, a comparison makes no sense.

**Authors' answer:** *Again, we think RC1 has misunderstood something. Our paper doesn't claim a "strong connection" or causal between rock glaciers and rock wall permafrost. We do not mention the rock glacier origin, and the term "moraine-derived" rock glaciers is not used at all in our paper. Our paper simply shows in sect. 5.4 that mapped active rock glaciers often are surrounded by rock wall permafrost. It therefore suggests to explore the link between permafrost in debris slopes and in rock walls. Our statement with this respect is as follows:*

*"This underpins the interest for studying the connection of permafrost rock walls and adjacent landforms which so far, to the author's knowledge, has not been directly addressed. Indeed, permafrost dynamics can influence material supply pattern to these landforms, as e.g. frost cracking varies greatly within and without permafrost environments (e.g. Hales and Roering, 2007)."*

**RC1:** The author should focus on their objectives and present directly from the beginning the Norwegian rock instability inventory by Oppikofer et al. (2015). They should test if their threshold-

derived rockwalls include all instabilities. Furthermore, they should compare their permafrost distribution to the location of instabilities. Can you develop a relationship based on your data?

**Authors' answer:** *And again, we think RC1 has misunderstood, which of course could be our wording or language. We have therefore clarified our paper's objectives in the title and in the introduction to avoid such misunderstanding are our objectives are definitely not to present the rock instability inventory and its link with permafrost as claimed by RC1. See other comments above the limitations of such study in the current state. Defining a relationship between permafrost distribution and the slope instabilities inventory is not feasible in the current state.*

**RC1:** What would be interesting is to model future permafrost distribution as previously done by Hipp et al. (2014) by using temperature increase scenarios. The authors can compare future permafrost distribution to slow creeping rockslides (e.g. Jettan) and other instabilities in the inventory and can draw conclusions of permafrost degradation on potential instability sites. The map could identify hot spots of future rock slope failures, which can be used for hazard mitigation such as planning or zoning. This would be of more interest than comparing permafrost distribution to other periglacial landforms.

**Authors' answer:** *Yes, this would indeed be interesting. It is a.o. therefore we wanted to publish our results as a base line of rock wall permafrost distribution in Norway. Like in Hipp et al., (2014) we of course work on calibrating transient heat flow models for the study sites. Until now we have given priority to look on the thermal history rather than the future, soon to be published in a PhD theses and papers therein. We certainly will work on the future impact, but probably more site specific. However, this is not the objective of this study.*

**RC1:** In summary, this paper can be a very important contribution to Earth Surface Dynamics if linkages between permafrost and instability are improved and coherently discussed. The model set up should be better explained and sensitivity of the rock slope angle threshold evaluated. The paper should focus more on rock wall instabilities, thus, the Norwegian landslide inventory provides a unique dataset and comparison to current and future rock permafrost distribution would provide valuable information for geomorphologists but also for hazards mitigation by the managing authorities.

**Authors' answer:** *In the revision we better explained our model set up, also following comments from RC2. We better detailed the choice of loggers' locations (section 3.1, 2<sup>nd</sup> paragraph) and slope threshold choice (see answer to former comment about this point). Concerning the relationship between permafrost and rock wall stability, see our introductory comments to this reply.*

## Answers to RC2

**RC2:** The manuscript presents an important study for a better understanding on how permafrost is distributed within rock walls. It relies on a high number of rock temperature data and the outcomes of the study are significant. The manuscript is rather well written, but sometimes the text is a bit confused and thus not always easy to follow. The results are generally well discussed, and the last section of the discussion presents interesting and original reflections. However, the manuscript contains some issues, the most important one being that the model is not enough clearly explained, as for the model parameters. The way on how the RST data were used to calibrate the model must be much better explained. Some figures must also be improved, because not enough clear or not enough explained. For non Norwegian people it is sometimes difficult to follow. I present here after some general comments, and then more specific comments.

**Author's answer:** *We thank reviewer 2 for insightful comments, and try to address the points raised above. Details of our revision are given in the following.*

**RC2:** In the Introduction, a chapter on the different models used hitherto to predict the occurrence of permafrost in rock walls is missing. What is the story of the research in this field ? Which models were used ? Where ? etc.

**Author's answer:** *We have added a paragraph summarizing the history of rock slope permafrost studies, with a specific focus on statistical approaches as this allows better introduction of our study.*

**RC2:** The method used to predict MARST must be better explained. It's not clear how the authors used the measured MARST to predict MARST. In the equation (1), MARST is predicted from PISR and MAAT only, and I guess the relation between MARST and PISR / MAAT is expressed in the coefficients b and c, but it's not expressively shown. So this section is a bit confused and needs then additional details.

**Author's answer:** *We have provided more information about the modelling approach, by providing first the statistical basis of multiple linear regression model as a first Equation, and explaining how we link the 3 variables. See Lines 1-6 P6 of the revised version. Then, concerning the statistical significance of each coefficients, this is provided in section 4.1 and we can not give such information in the methods as this is purely results of our study.*

**RC2:** The permafrost occurrence is defined when the rock surface temperature is  $\leq 0^{\circ}\text{C}$ . This is true for permafrost in equilibrium with current climate conditions, but permafrost can be present at depth even with positive surface temperatures, due to thermal offset and thermal inertia. In this case permafrost is not in equilibrium with the current conditions. This must be taken into consideration. Obviously it is, since MAAT is calculated for the period 1981-2010, but this must be better explained.

**Author's answer:** *Yes, this is entirely true and needs to be explicitly provided in the interpretation of our permafrost probability map. We therefore added a few explanations about this point in Section 3.5 in order to refine the interpretation keys.*

**RC2:** The concepts of “lower limit of permafrost” and of “lowermost observations of the lower altitudinal limit” (LAL) must be better defined. At this stage the difference is not clear it is not evident to understand why the second was introduced.

**Author’s answer:** *Yes indeed. We were not very clear with the use of these concepts. In section 4.2 we introduce the concept of LAL, and avoided the use of the «lowermost observation of the LAL».*

*“In Figure 9 we display the distribution of the lowest occurrence of each permafrost class (i.e. the distribution of the occurrences of probability 0.1, 0.5, and 0.9) according to elevation and latitude. We refer to these lowest occurrences as the “lower altitudinal limit” (LAL) of each permafrost class.”*

*And we replaced the «lower limit of permafrost» by the «LAL» as it is basically the same concept, but that we referred to in a different manner in the results and discussion sections.*

**RC2:** In the discussion chapter, section 5.3 is very difficult to follow. It contains many descriptive parts that compare permafrost elevation in north and south faces with more gentle slopes in specific locations, which are not always visible on Fig. 11. In the end it is very difficult to capture the main message. I suggest to reorganize the text, to be more systematic in the comparisons, and also more synthetic.

**Author’s answer:** *We re-wrote this section following this comments. We removed parts of the description to focus more the key message and hope it is clearer in the revised version.*

**RC2:** Regarding FigureS3, there are huge differences between the outcomes of the two models. This must be addressed in the text.

**Author’s answer:** *When re-writing the section, we have given explanation to these differences. See at the end of the 1st paragraph of section 5.3.*

**RC2:** The question of the influence of continentality on the permafrost occurrence and lower limit should be also addressed more largely, and not only for Norway. Some studies (e.g. Sattler et al. 2016 for permafrost in New Zealand) showed that permafrost may reach lower elevations in more humid locations than in continental ones.

**Author’s answer:** *Thank you for reminding us this relevant study from Sattler et al. (2016). In the revised version we reinforced our argumentation on cloud cover effect of maritime areas as suggested in this study in section 5.2, but we did not extend the discussion on the continentality too much in section 5.3 because it will center the discussion on gentle slopes permafrost while we focus on steep rock slopes.*

**RC2:** Regarding the form, in the introduction especially there is a lack of transitions between some sentences. It is often a juxtaposition of sentences, without any link. Ex. .1, l.30, P.2, l.13, l.20. The text contains also many typos, especially in the Discussion sections. I noticed some in the specific remarks below, but in the end, I renounced to do it for all the manuscript. So please check them carefully.

**Author's answer:** *As mentioned in the introductory words, we re-wrote the introduction and hope it is better organized and more logic. Thank you for noticing some typos, we thoroughly checked the entire revised version before submission.*

### **Specific comments:**

P.2, l.14. Why "in the" ? → *that was a mistake, corrected*

P.2, l.16. Rock fall and rock avalanches are not agents, but processes, or events. → *Right, corrected*

P.2, l.22-34. How many of these 800 events triggered from potentially permafrost-affected rock walls ? → *this is unknown.*

P.3,l.12. Geologists are also interested in the influence of permafrost on rock wall stability, so choose a more general term than "geomorphologists". → *Right, we replaced by «earth scientists»*

P.5, l.2. This kind of sentence should be moved in the state-of-the-art section in the introduction → *Done, we merged it with the new paragraph explaining the state of the art in terms of rock wall permafrost modelling.*

p.5, l.3. Add a reference. → *This is merged with the state of the art in the introduction, with relevant references in the revised version.*

p.5, l.8. "therefore" means that from the former line we can directly derive the equation presented. This is not so obvious. Please be more precise. Then I don't understand how the coefficients  $a$ ,  $b$  and  $c$  are calculated. → *in the revised version we provide the statistical basis of multiple linear regression models and explicit how the coefficients  $a$ ,  $b$  and  $c$  are calculated in section 3.3 which is the appropriate section for such explanation.*

p.5, l.9. It is unclear if the way to predict MARST is original in this study or if it has been already proposed in former studies. → *We have elucidated that comments with the new paragraph in the introduction, the new lines introducing section 3.2, and added also some details in section 3.3 which is the most appropriate to give such information.*

p.5, l.16. The comma must be moved after "model" → *done*

p.6, l.14. Unclear to what correspond those 85 MARST points. Due to different years ? → *yes, we stated it explicitly in the revised version.*

p.6, l.24 and following. It's unclear which DEM resolution the authors choose : 1 m or 10 m ? → *At this line, the different DEMs have not been introduced yet. They are introduced l27-29. Then we explain which are possible criteria to choose the DEM we use (hillshading effect or better representation of the real-world topography). Then, L34-35 and P7 L1-2 we explain that finally, we chose the DEM based on the best match with real world conditions at location of RST loggers.*

p.7, l.11. Fig 4, not 5 → *True, we changed it*

p.9 l.18. The “LAL” is not easy to understand. What is the difference with the lower limit of permafrost ? Why defining this new concept ? → *See answer to the general comment.*

p.10, l.3-6. The lower limit of discontinuous permafrost must be better defined (see my general comments). → *Yes, we improved it, see answer to former comment.*

Fig. 10 does not show between 0°C isotherm and latitude. → *True; we actually kept these sentences from a former version where we fitted a linear regression between the LAL of discontinuous permafrost and latitude, but finally removed it because it was not bringing something completely new compared to Figure 9. We removed these sentences in the revised version.*

In the same section the decrease of the lower limit of permafrost northwards appears to be more pronounced than the decrease of the LAL. How can the authors explain this ? And again, it is really not easy to follow, the difference between the two concepts being not clear. → *Yes, regarding this comments, it is clear the the concept was not clearly explained and this is why such a question arise. With the explanation at the beginning of the question, such question should not arise. The Lower limit of permafrost is the same as the LAL, and this LAL decreases northward for each permafrost class.*

p.10, l.8. Show on a map where the Caledonides are. Then, since most of the rock walls are located in the interior mountain massifs they should also be located in more continental conditions. Thus there is a problem of logic in this sentence. → *Yes, this is true that this sentence is not logic. We rewrote it to avoid confusion.*

p.10, l.26. Check the syntax. → *Done, we rewrote it.*

p.11, l.8. . . . 2 m AT. First, AT must be spelt out here (the reader may have forgotten the meaning of the acronym). Second, from where comes this 2m AT ? I did not find it in the method section. → *we wrote air tempertaure with plain text and removed the «2 m» as this was not a relevant detail to understand the study and refers to details of Lussana et al. work.*

p.11, l.14-15 : Fig.13 (there is no Fig.,. 15) → *right! corrected*

p.12, l.3. Remove the coma after “both”. → *Done*

p.12, l.5-8. A bit confused. Do not put such long explanations into brackets. → *We rewrote the sentence.*

p.12, l.26. Both figures 10 and 13 do not show this decreasing elevation of permafrost across continentality. → *This is true that for Figure 13, the link with continentality is less evident and we rewrote the text accordingly.*

Fig. 5. Indicate in the caption the significance of the yellow dots. → *Done*

Fig. 9. How can the authors explain the plateau between 62°N and 66°N ? → *This is due to rock wall distribution, and air temperature. We mentioned it in the submitted version P9 L23-24 but rephrased it in the revised version to point ut out clearly.*

Fig. 9 and 10. Please align the grid on the labels of the Y axis. → *Done*

Fig. 11. Please explain somewhere why the threshold 0.5 is used to create the CryWall map. In the caption indicate what show the squares and the yellow circles. → Done.

Fig. 12. The maps are quite difficult to read. The legend must show the 6 colors present in the map. I suggest to make the differences between the two used models clearer, by having more distinct colors. Same remark for Fig. S3 and S5. → Here we chose to use the same color range for both models because we want to make the different permafrost classes comparable between the gentle slope permafrost model and the rock wall permafrost model (CryoWALL map). By using different colors, it would be harder to directly compare the permafrost classes of the 2 maps. Thus, this is not 6 colors but 3 colors with different transparency. Instead of changing the colors, we have improved the legend as this is true that this was not clear and lead to confusion. We did the same for Fig. S1.