Author’s response to reviewers on

“Modeling present and future bedrock permafrost distribution in the Sisimiut mountain area, West Greenland”

By Marcer et al., The Cryosphere Discuss.,

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Note to the referees

We would like to express our gratitude to both referees for their efforts in reviewing our manuscript and for their valuable feedback. We appreciate the time and effort to help us improve the quality of our study. Throughout the review process, we have considered every comment provided by the referees. As a result, we have made major modifications to our study, in order to improve its overall quality. In the following sections, the reviewers can find a summary of the major changes that will help to navigate through the revised manuscript.

Structure

Both reviewers noted issues with the text structure in both rounds of review. In response, we have embraced the suggestion to merge the results and discussion sections. Moreover, we have reorganized the entire text to ensure that sections in the methods align with those in the results and discussion, facilitating smoother navigation for readers. We have also revised and re-arranged all figures to address concerns about excessive jumps between distant figures during our discussion. Furthermore, we have provided a clear statement of our research questions in the introduction and ensured that the conclusions directly respond to these questions.

Revised methods

We recognized that our methodology introduces confusion due to the multitude of methods employed. Consequently, we embarked on substantial revisions of our methods embracing all suggestions from the reviewers. Eventually, we run all models from scratch with updated methodologies, which resulted in slightly different outcomes. The methods revised are the following:

1. **Discarding the snow cover model** - After considering the points raised by the reviewers, we decided that this method generates confusion, without adding meaningful value to the study. **We have now limited our analysis to snow-free steep terrain, specifically rock walls**. This terrain still aligns with our primary interest in the broader context of mass movements. As a result, we now refer to Rock Surface Temperature (RST) instead of Ground Surface Temperature (GST). We continue to utilize deep borehole data from flat terrain, but cautiously. On the one hand, data from SIS2021-01, situated in wind-exposed settings that ensure snow-free conditions, can be used to calibrate our heat transfer model. On the other hand, SIS2019-02, which experiences snow cover during the winter due to drifts, is used to assess how well our model (calibrated for snow-free conditions) applies to snow-covered bedrock. We utilize the results to draw conclusions about the model's uncertainty in managing snow-covered bedrock. The uncertainty range derived from this analysis is integrated into the model outputs, particularly in 2D simulations, where snow accumulation may occur on some sections of the topography. This integration delineates a "transition zone" within the temperature range where the model's uncertainty prevents the clear distinction of permafrost presence or absence.

2. We discard data from flat terrain, soil and iButtons. Apart from the comment above, it was expressed concern for using data from sensors installed with different techniques. Consequently, we have simplified our study by **exclusively utilizing geoprecision data** to ensure a consistent dataset.

3. In our permafrost maps, we have **ceased using the MGT20** as doubts were expressed by both reviewers regarding its use. Instead, we rely on the classic Mean RST (MRST), which we average over 20-year time spans for 2002-2022 (current distribution of MRST) and 2080-2100 (projected distribution under the RCP scenarios). It is important to note that MRST values are warmer than MGT20 values, reflecting the ground conditions at depth, where permafrost may exist despite warmer surface temperatures due to ground inertia. Consequently, our results show higher elevations for the 0°C isotherms

4. In response to R2's suggestion, we have **quantified the uncertainty associated with our weather data** using air temperature data from the AWS in Sisimiut (see new Figure 4). Additionally, we have chosen to use ERA5 data rather than AWS data for forcing the model. This decision allows us to better control the uncertainty on the RST model generated form regional-scale data.

5. We updated the downscaling procedure and now **use the TopoSCALE algorithm** instead of our previous method. This algorithm is the state of the art of weather downscaling in complex terrain and we have started recently to use it in our research group. This decision enhances the clarity of our manuscript, since we have one single RST model to apply to downscaled data, instead of a model for each dataset.

ERT

Geophysics has garnered varying opinions from the two reviewers and the editor, ranging from its usefulness to the suggestion that it should be discarded. In this revision round, we continue to support the use of geophysics for several reasons, including the fact that it provides essential data from mountain terrain. We address these concerns as follows:

1. We have clarified in the introduction why we chose to use Electrical Resistivity Tomography (ERT) and highlighted how this method has been employed in a similar manner by other studies addressing similar issues.

2. We improved our methodological description, thanks to the reviewer's suggestions

3. Thanks to merging results and discussion, we have now a more comprehensive description of ERT results and discussion on their value versus their limitation.

4. We have introduced a new figure (figure 7) that quantitatively compares ERT predictions of permafrost with 2D model outputs. This quantification allows us to describe the agreement between the two methods and derive meaningful conclusions from it.

New RST data

We now include RST data from the year 2021 – 2022. Now, our observational period for the RST model is two years (2020-2022). This point has been raised few times, especially during the first revision round. We agree that longer time series brings more solidity to our results.

In conclusion, we thank again the reviewers for their work in helping us refine our study. While we have considered and appreciated all style-related comments, some sections have been removed or significantly altered (e.g., regarding snow modeling) to streamline the paper. These comments are not specifically answered in the following sections, but rest assured that no comments have been disregarded.

Referee #1

We thank the reviewer for this detailed feedback that helped us to improve the text. All comments where useful to reshape the study and manuscript as described in the "Notes to the referees" section. In addition to the concerns addressed by that section, we add that:

- All general comments regarding structure, figures and units are accepted and integrated.
- All style-related comments are accepted.
- All qualitative descriptions are removed in favor of quantitative descriptions (e.g. comment L352, 356, 425)
- We have restructured the figures and subfigures to make sure that we cite them in ascending order. We now do not jump back to old figures through the text (e.g. comment L351, L421). Thank you for the suggestion; that helped massively in working on the text.

Hereafter, detailed answer to any comment that is not related to the previously described topics.

[R] L 154 - What is the convergence criterion? Maybe provide the value reached after 3 iterations.

[A] We now describe the convergence - L 144

[R] L 157 - So you extracted three core samples from the two rock samples you collected in the field?

[A] This is misleading; we have samples from the surface only. The sentence is now corrected – L 147

[R]L 163 - The internal sensor of what? What is the precision of this sensor (compared to the external one)?

[A] We have now provided an explanation of what these sensors are - L 154

[R] L 166 - Right now, the connection between these three sentences is not clear. I suggest to provide a more detailed description of the steps undertaken in your laboratory experiment. Maybe you could also briefly address how you consider secondary porosity or which assumption you use in this regard, respectively.

[A] We have now improved the description of the steps for the laboratory analysis. L145 to 158. We briefly address the issue on secondary porosity at L319. Thank you for raising the point.

[R] L 285 - I would not write this as an inline equation but instead as a numbered equation (even if it remains the only one in the manuscript). In this way, the different terms/variables in the equation can be properly introduced (e.g., dT is not addressed in the text). Moreover, I strongly suggest to highlight which variables in the equation are scalars or vectors/matrices, respectively.

[A] Since we do not model snow conditions anymore, we discard this equation. The boundary conditions are given by the RST model, shown in figure 5, P5.

[R] L 291 - Figure 3a shows only the time series obtained from a single logger

[A] We now show all loggers time series (new figure 2a, P12).

[R] Which loggers and why where they selected? Random selection? I suggest to incorporate/highlight this comparison in Figure 3.

[A] We have now dedicated figure 2b to this comparison P12.

[R] L 307 - This section somehow breaks the flow or the connections between the other (sub)sections, respectively. Actually, you could consider to present the ERT tomogram as part of M&M since it is not addressed in the Discussion anyway.

[A] Considering the text in the section “Notes to the referees”, we have now expanded the discussion on the ERT and connected their result to the temperature data. See L311 to 316 and L324 to 326.

[R] L 311 - Be careful here, based on your petrophysical analysis in the laboratory and your ERT field measurements (single campaign) technically you can only discriminate between frozen and unfrozen conditions. For permafrost we need frozen conditions for at least two consecutive years. And I do not see how this information can be obtained from your data/analysis.

[A] We can discriminate between frozen and unfrozen conditions below the depth of the zero annual amplitude. We therefore expect permafrost existence here. This is now specified in the text (L307), as well as supported by the observations from the temperature data (L325)

[R] L 324 - How do you quantify that it is "optimal"?

[A] This is now described in M&M L220

[R] L 333 - Was this abbreviation introduced somewhere? MGT20 is a result of your GST modeling? If yes, then this should be made clear in the text. Otherwise I would consider that you obtained MGT20 from another data source and this whole sections would belong to M&M.

[A] MGT20 was introduced in M&M at line 272. R2 is also confused by this quantity. We just decided to not use it anymore and use the classic Mean Rock Surface Temperature instead – See the Note to the referees.

[R] L 338 - Please write your figure captions consistently throughout the manuscript. Here you write "on panel a" while in other figure captions you just write "(a)".

[A] We have now make the captions consistent through the manuscript. Thank you for the suggestion

[R] L 341 - This section appears to be disconnected from previous and following sections. Please reconsider the structure of the Results section or the manuscript, respectively.

[A] This is now part of the section “Model testing” P10, L233

[R]L 342 - 2D model simulation of which parameter based on which input data?

[A] This was described in the dedicated section: 2D models L 275. We have now moved this to the “Model testing” section. P10, L233

[R] L384 - In line 330f you wrote "[...] indicating errors up to 2 °C above the depth of zero annual amplitude (20 m depth), while the errors are consistently smaller than 0.15 °C below this depth."

[A] We now refer to figure 6c to describe errors, P17. We are now happy to describe errors across different depth ranges, thank you for this suggestion.

[R] L 388 - Are you referring to the results presented in Figure 5? The agreement between modeled temperatures and observed electrical resistivity needs to be quantified.

[A] Quantification now provided in new figure 7, P18, and L401 to 403

[R] L 403 - This could also be due to the data or processing, inversion (e.g., 2D vs 3D). Also if your geophysical results have such a substantial limitation with regard to the validation of your modeling approach why present them anyway? How is this supported by your geophysical investigations?

[A] The limitations here described are limited to a small area of the profile. The ERT profile is very valuable to our study. Considering the answer above, we have now put more emphasis on how we can interpret the ERT data and how they validate our understanding of permafrost in the area.

[R] L 411 - In Figure 9, I can see this for Nattoralinnguaq but less clearly for Nasaasaaq. How do you explain this?

[A] That's because the north face of Naasaasaq summit can accumulate more snow than the south face (see flat surfaces in old fig 9). We now use profile on the ridge instead of the summit, which is homogenously steep on both sides, and results, are clearer (see fig 10, P21).

[R] L 417 - So what is the dominant factor here? Solar radiation or elevation-dependent air temperature variations? From the current formulation this is not entirely clear.

[A] We have reformulated this concept L321 to 326.

[R] Actually, in the Discussion you state that is a substantial disagreement between modeled temperatures and the observed electrical response.

[A] The agreement between 2D model and ERT data is now in new figure 7, P18, and L401 to 403. An evaluation of the agreement, and why we consider it satisfactory, is at L412

[R] L 448 . To convey […]

[A] Thank you very much for this. We like the text and included as closing statement in the conclusions, L481.

Referee #2

General comments

[R] In their revised manuscript "Modelling present and future bedrock permafrost distribution in the Sisimiut mountain area, West Greenland", originally "Characteristics and evolution of bedrock permafrost in the Sisimiut mountain area, West Greenland", the authors use a minimalist approach to modelling spatial permafrost distribution and its future evolution. In my opinion, the revision of the manuscript has massively improved its quality, although some points still remain open. Since the changes were very extensive, a few more questions arose during the re-review of the manuscript, which are addressed in the following. The reviewers' points of criticism regarding formal aspects have been incorporated very well for the most part. Nevertheless, there are some sentences, especially in the newly written sections, whose wording should be revised. I have made some suggestions here, but would recommend that the manuscript be critically proofread once again. The illustrations have also been significantly improved. A few suggestions, or points of criticism, are given below in the line-by-line comments.

[A] Many thanks to R2 for this work. We appreciate the effort and did our best to reshape the study according to the points raised. Please, see "Notes to the referee" for a summary. In short, all points raised are accepted and integrated, bringing to a major revision of the methods and the study.

[R] I don't want to appear cynical at this point, but I want to be deliberately provocative; does this mean that the heat-transfer model works in the depth ranges where heat transfer no longer takes place? That is, below the zero annual amplitude? And, isn't it the area above zero amplitude that is relevant for permafrost dynamics in the coming decades and for potential mass movements? Of course, climatic trends are relevant for long-term permafrost development and not short-term dynamics. Nevertheless, seasonal effects in particular are important for these dynamics. Since a central point of the paper is to test the uses of this minimalist approach, a much more comprehensive evaluation and discussion of these points (including the database on climate and weather data) would be very relevant to me.

[A] Thank you for your thought-provoking comments. Regarding your first question, this study is designed to understand the long-term evolution and zonation of permafrost in the area rather than the short-term dynamics. We acknowledge the importance of seasonal effects in permafrost dynamics, but our focus is on identifying the presence and potential future changes in permafrost distribution. Modeling shallow permafrost dynamics and potential mass movements is indeed a complex and challenging task, and it falls beyond the scope of our study. We've clarified this in the introduction by explicitly stating our research questions and objectives (L65). This is taken up again in the conclusions, where we clearly state that our model is not suitable for shallow dynamics, and other methodologies should be adopted instead (L481). In response to your suggestion for a more comprehensive evaluation of climate and weather data, we have now implemented a validation procedure, which quantifies the accuracy and reliability of our data (See dedicated sections 3.3.1 P7, and 4.3.1 P14, dedicated figure 4).

[R] I still cannot fully understand this line of reasoning. The seasonal variability driven by the snow cover is very central to the dynamics of the subsurface temperatures above the zero annual amplitude. If these heat fluxes cannot be correctly reproduced in the model, why can the model accurately model the temperatures below the zero annual amplitude?

[A] You've rightly pointed out the significance of seasonal variability driven by snow cover in subsurface temperature dynamics above the zero annual amplitude. In our approach, the model includes offsets to account for the overall average effect, which allowed it to approximate temperatures below the zero annual amplitude for long term permafrost dynamics. However, upon further reflection and considering your input, we have decided to revise our methodology and made the decision to limit our analysis to snow-free areas, specifically rock walls. We observe the effect of snow cover on the data from borehole SIS2019-02, and use these data to discuss how snow cover affect bedrock temperatures and generates uncertainty in our model. By doing so, we acknowledge the complexities associated with modeling the snow cover's impact on permafrost dynamics, and we have chosen to focus on areas where the model can provide more reliable and meaningful results.

[R] I understand the importance of using ERT to achieve more spatial ground data. Especially in tough terrain like at your study site. Anyhow, I still think the results of ERT-Measurement should be discussed in more detail. I have addressed these points in the line-by-line comments.

[A] Thank you for your feedback, and we appreciate your emphasis on the importance of discussing the ERT results in more detail. In response to your comments, we have made several improvements to the paper. First, we have updated the ERT description in relation to the observed temperature data (L322 to 326) and the heat transfer model (See L400 to 415). Additionally, we have merged the results and discussion sections to ensure that the information is not scattered across the text, allowing for a more coherent and comprehensive presentation. Furthermore, we have dedicated a new figure (Figure 7, P18) to comparing the ERT data to our model, quantifying the agreement and disagreement between the two models. This addition will provide readers with a clearer understanding of how the ERT measurements align with our modeling results.

[R] In my opinion, one point that falls into the area of both fundamental and formal criticism is the discussion and conclusions. While chapter 5.1 clearly discusses the model uncertainties (in my opinion, this could be done in more detail), chapter 5.3 gives results on the future development of subsurface temperatures. There is no real discussion. Here I would wish that the results were discussed more clearly according to the objectives, which in my opinion could also be made more concrete. I have a similar feeling about the conclusions. Here there is a focus on the outlook regarding the relationship between permafrost degradation and mass movements. This is a topic that is not addressed in the paper, with the exception of the introduction. It is also not clear to what extent the temperatures below the zero annual amplitude that could be modelled are related to mass movements. Rather, I think it would be of interest to know where the weaknesses and possibilities of the minimalist approach are to be seen. This is an issue that I think is of great importance. And here a clear evaluation of the approaches carried out would take the scientific community much further.

[A] We have taken your feedback into account and made the following improvements:

- Regarding model uncertainties, we have provided a more detailed description of these uncertainties in the paper. Specifically, we have included a better explanation of model errors concerning the available data from boreholes and ERT measurements. We define an uncertainty range that accounts for the model's root mean square error (RMSE) in training (See figure 6c, L364 to 367) and the uncertainty introduced by the possibility of snow cover (L384 to 389). This uncertainty is now integrated into the model outputs when describing future scenarios, especially in complex terrain using our 2D models (see L389 to 395). We have also incorporated this uncertainty into the discussion of permafrost evolution in future scenarios (see L431-440, figure 10 P21).

- In the conclusions section, we have revised our focus to emphasize the main outputs of our modeling approach (L465 to 480). We now clearly state the role of our model in describing present and future patterns of permafrost distribution in rock walls within the study area. We have now clarified that the model is not designed to describe processes directly lined to mass movements, and that other modeling approaches are required to make this further step (L482 to 485).

Line-by-Line comments

Due to the substantial changes to the text, we do not keep track of style-related comments, as we integrated them when consistent to the new structure. In particular, as explained in the “Note to the referees” section, we decided to discard our snow model, also thanks to the points raised. Therefore, we will not further discuss here this issue. Here, we provide response to content-related comments.

[R] In the chapter 3.1 you mention that iButtons were installed in soil. Anyhow, you did not mention the soil in the site description. As the thermal regime of soil most probably differs significantly from the other substrates it would be nice to have some information on the soils, maybe thickness and distribution? Or do you consider it as insignificant?

Is there a reason, why holes in bedrock are 10x300 for geoprecision loggers and 22x100 for the iButtons? Also, why you used different sealing? And why in 50mm in gravel?

[A] We acknowledge that the combination of loggers and ground conditions is confusing, and we've decided to homogenize the data sources and focus on a more consistent approach in our study. Therefore, for surface temperature data, we will only use data from the geoprecision loggers installed in rock walls in our study. This decision allows us to maintain a more consistent and coherent dataset for analysis. Also, this decision is in line with focus our study on rock walls only, as described above. Regarding the different hole sizes and sealing methods, this was due the practical issues of drilling 30 cm with 22 mm bit, which was not really possible with our driller.

[R] I think it was really helpful that you added some background information on ERT in this chapter. However, to me, the structure is a little confusing. You start with the setup of the measurement, followed by basics on ERT, back to the field-setup followed by data processing. Could you maybe rearrange this in a more straightforward way? […] The wording here could be somewhat misleading and imply that one can directly derive information on thermal conditions from ERT. Even if only qualitatively. Here I would find it helpful to formulate the whole thing a little more precisely. Perhaps a clear introduction to the chapter would be helpful, in which it is made clear at the beginning that the information on the thermal status is derived from the ERT measurement in combination with the laboratory calibration.

[A] Thank you for the comments. We have restructured the whole section accordingly (also considering the comments form R1 who shared similar concerns). We have now a structure consistent with the suggestions. We also focus on describing how the laboratory analysis is used to interpret the profile. We have also discarded some technical information that are not relevant to the analysis and rather refer to previous studies that described in deep detail the method we apply (L128)

[R] In this paragraph values were changed compared to the first version of the manuscript. The changes are tracked, but – far as I can see – without explanation. Changes include porosity, conductivity and information on TF and downward/upward. Could you please explain the reasons for the changes and how they affect the data?

[A] The explanation is given in previous rebuttal. In summary, we repeated the experiment with different time steps for temperature intervals. Time steps and temperature steps are now explained in the methods (L156 to 158).

[R] Here you argue, that varying the rock-type parameters results in temperature differences smaller than 0.1 °C. Could you add some information on the values you tested to get a little more context on the errors?

[A] We have now included the thermal properties in the calibration process and describe how porosity has more influence on the model outputs. We specify that we started the calibration procedure using an initial set of parameters and could not improve the model results by varying them (L360 to 363).

[R] It would be helpful if you could add an indication in Figure 5b on where North and South is. I have a question about the lithological fault shown in Figure 5a. Firstly, could you provide additional information on this in the text? Secondly, this lithological fault is located directly in the border area between relatively low and very high resistivities. I could imagine that in the vicinity of fault zones, the rock may be more weathered or fractured, which could potentially have an influence on the resistivity distribution. Partly you discuss this a little in Chapter 5.1 (L401-404) coming to the conclusion, that "a direct comparison between model and geophysics is not meaningful" I think this might need a little more background information.

[A] We have added a north arrow in the figure, thank you for noticing. We have added a more comprehensive discussion about this part of the tomogram, also including some considerations on the expected permafrost properties at this location versus the ERT. Hopefully, having merged results and discussion, the information about this issue are not scattered across the text anymore. (See L311 to 321)

[R] Could you provide a little more information on the results and also in the discussion of results? You clearly mention the errors of the model, but you do not go into detail. I think what is interesting is that temperatures in the uppermost 10-20 m are massively overestimated, showing positive temperatures while borehole temperatures are negative (it looks like in the shallow subsurface modelled temperatures are around 2 °C, while Data show values of -4 °C, is that correct?) In contrast, Temperatures at greater depths seem to be more underestimated around the errors of around 0.15°C. In the discussion you mostly point out, that the problem might be the climate data. Could there also be other reasons? Could effects be also driven by parameters of the heat transfer model?

[A] We have now included a discussion on why we observe large errors on the uppermost part of the heat transfer model. That is, probably because the model is based on conductivity while, at these depths, advective processes may take place and dominate heat transfer. We base this hypothesis from the discussion from Magnin et al (2017), who observe as well high deviances down to 6 m depth. We also indicate that the model overestimation in SIS2019-02 should be taken with care, as we have 4 measures and only from the same time in the year (fall – early winter). We suggest that advective processes at this time and location tend to cool the borehole more than pure conduction would allow. (See L381 to 383)

[R] Could you be a little more precise with locating the frozen ground? At least in the ERT it looks like the direct summit is unfrozen (or in the transition zone?)

[A] A more precise description of the areas of the profile is now added in section 4.2, P13. To this purpose, also see new figure 7 P18.

[R] This might be a relatively fundamental comment, but could you provide reasons, why you refer to MGT20 as a relevant depth with regard to ground temperatures? I can see, that the reliability of your model is relatively weak in the area above. Anyhow, if you know, that there is an area, where the temperature does not change, is the critical aspect on the detection of this depth or to model the temperature itself? With regard to MGT20 could you please add some information on how to understand this parameter? Does this value refer to the depth below the ground surface or vertically? Considering the topographic conditions, it has made it very difficult for me to understand what actually happens with a change in this MGT20 isotherm. Especially since both the introduction and the conclusions focus on the relationship between permafrost degradation and rockfalls, it would be interesting to explain the significance of this MGT20 in this context. Another commonly used measure, for example, is the temperature on top of permafrost (TTOP). Could this approach be more effective than MGT20? Could you maybe provide reference, why this value makes sense in this context?

[A] We acknowledge that the MGT20 is not very popular among both reviewers. We therefore now use a more classical indicator: the Mean Rock Surface Temperature (MRST) across a long period (we use 20 years). MRST is conceptually similar to the TTOP parameter (TTOP accounts for different snow/ground classes, which we do not need to since we only have the class “snow free bedrock”). Other studies use the same approach, for example Boeckli at (2013) in the APIM map, or Gruber (2012) in the PZI map. The original reason we chose to use the MGT20 is that it gives a representation of deep ground conditions, as it extracted form transient simulations. However, deep ground conditions are already described by the 2D simulations, without all the inconvenient relative to topography and geometry you describe. Therefore, we discard the MGT20 in favor of the MRST without loosing value of the model output. Reviewers should notice that MRST are “warmer” than MGT20 as they represent surface conditions rather than deep temperatures – reason why the polar plots in figure 8, P19 are different than the previous version of the manuscript.

[R] I think the discussion could be a little more comprehensive at this point. Reference is made to figure and a mean error of 0.14 °C between 0 and 100 m depth. On the one hand, no information about this 0.14 °C is given in the text or in figure 4. On the other hand, does it make sense at this point to mention the mean error over the entire depth, if the errors between 0-20 m depth are significantly higher with more than 2 °C? In my opinion, these aspects could be considered in the discussion in a little more detail.

[A] To answer this comment we added figure 6c, P17. We now present model RMSE across the borehole depth. RMSE values are derived for different time aggregations, i.e. for the raw monthly model output, and as an average across the whole observational period of SIS2021-01. The discussion now refers to this figure when addressing the model errors (L364 to 367).

[R] As already asked in the first review, it would be interesting to include a little more discussion on the climatic database that has been used. In your reply to my question you wrote "Although this would be an interesting study, it is not covered by the paper's aim." Here I partly do not agree. I think, if a study is conducted, and it has been decided to use a certain database, one basic hypothesis should be, that the database is suitable to answer the research question. With regard to this, I think this aspect should be discussed a little more detailed.

[A] We embrace the comment and have now reshaped the study accordingly. We now provide a comparison between the different weather datasets and the air temperature measured by the Sisimiut weather station (see dedicated figure 4, and sections 3.3.1, 4.3.1). We have also decided to use AWS data as validation data only, i.e. we do not force the model with them. We decided to take this path as, in our future work, we will need to model permafrost distribution in areas where we do not have such long weather station data (AWS data are sparse in Greenland). We therefore prefer keeping this dataset for controlling what is the uncertainty generated by weather data available at the regional scale. This is now explained (L168 to 174)

[R] I consider the information in this figure to be somewhat misleading. Basically, the comparison is very interesting, but it seems that the data is not really representative for the regions listed. It appears that meta-studies are cited, but as far as I could understand based on the literature mentioned, they are mostly case studies referring to specific locations. While the studies in Norway include one northern and one southern site, for the European Alps there is one study that refers to an area in the Mont Blanc massif. Is this correct? How representative are the studies listed for the regions? Can the graphic be optimized and made more concrete?

[A] Based on the comment, we decided to discard this figure. Comparison with previous studies on this matter can still be found in the text (L289 to 296)

[R] I think some of the main outcomes of the manuscript are missing in the conclusions. As discussed in chapter 5.1, there are some uncertainties regarding the model as well as the prediction of permafrost distribution. If I understood it correctly, one important aspect of the manuscript is, to check, if permafrost distribution and evolution can be predicted using a minimal amount of data. Therefore, maybe an important methodological aspect. These points are not part of the conclusion at all. As mentioned above, one main aspect of the conclusions is on rockfall. A topic, that can not be regarded as an outcome of the manuscript. I would suggest to optimize the conclusions – and to some parts the discussion – to get a more precise focus on these aspects

[A] We agree with the comment. We have now revised the conclusion and listed the main outcomes of the study. As explained above, we make clear that slope stability is just the frame of the project and we do not aim to model processes directly connected. Therefore, this matter is only briefly presented as an outlook in the last sentences of the conclusion (L481 to 488)