

Author's response to reviewers on

"Characteristics and evolution of bedrock permafrost in the Sisimiut mountain area, West Greenland"

by Marcer et al., The Cryosphere Discuss.,

<https://tc.copernicus.org/preprints/tc-2022-189/>

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Referee #1

Anonymous Referee #1

Referee comment on "Characteristics and evolution of bedrock permafrost in the Sisimiut mountain area, West Greenland" by Marco Marcer et al., *The Cryosphere Discuss.*, <https://doi.org/10.5194/tc-2022-189-RC1>, 2022

The manuscript "Characteristics and evolution of bedrock permafrost in the Sisimiut mountain area, West Greenland" presents an efficient approach for the modeling of bedrock permafrost in Greenland. Aiming at the prediction of the bedrock permafrost evolution the study considers two different regional carbon pathway scenarios to model the permafrost distribution at the end of the 21st century. Accordingly, this manuscript addresses a topic that has so far been underrepresented in the existing literature, and thus is particularly relevant for readers of *The Cryosphere*. The abstract of the manuscript is well-written arousing the interest of the reader by providing a concise yet complete overview of the study. Unfortunately, in its current version the manuscript itself fails to meet the expectations raised by the abstract. While the introduction is also of good quality (objective/aim of the study are nicely described) the following sections do not adequately present the otherwise great outcome of this study.

We thank the reviewer for this detailed feedback that will help us improve the text. We are also glad that the reviewer values the topic we chose for our study, and most of the concerns regard the text rather than the essence of the study. In this document, the reviewer can find the answers to the main points, while we reply to the notes in the annex directly in the supplementary file.

General comments

In particular, my main points of concern are:

Structure The manuscript lacks a clear structure, which also affects the adequate separation of the (content in the) different sections. In particular, the authors could consider merging the Results and Discussion sections, which would allow for a more concise presentation of this really interesting study.

We agree that current structure of the manuscript does not meet the requirements for scientific publication. As direct response to this comment, we would like to keep a Results and Discussion split, in favor of a traditional writing style. We thank the reviewer for the great effort in commenting the text, pointing out where structure could be improved. In particular, the major changes:

- We moved the section introducing snow modeling to the introduction P2L45-P2L54.
- We restructured the methods' sections, and added a clear "Snow cover modelling" section P9L215
- We removed the calibration results from the results section and cited these results as loggers properties in the methods P4L107, P4L111
- We made the discussion significantly more concise. We avoid all the discussion on slope stability, which is not relevant for this study.

Figures In general, the figures are of good quality and nicely prepared, i.e., by just looking at the figures the potential of the study is evident. However, not all figures are correctly referenced in the text. Moreover, the figure contents are not properly described/discussed in the text with some sub figures not being addressed at all.

We agree with the comment; we apologize for the incorrect referencing of figure in the text and we thank the reviewer for pointing out the error. Figures are now properly referenced and addressed in the text. We also have added a new figure (Fig. 10, P21), to support the discussion of section 5.2. Also, following the instructions from reviewer #2, we made major changes to:

- Fig. 3: data are now color coded depending on snow cover type, P13
- Fig. 4: data and model from SIS2019-02 are now presented in the figure, P14

Tables In general, the structure of the tables is fine; yet, in the text, the authors refer to a Table 3 that is not included in the manuscript.

Thank you for this remarks; this is a bad blunder. Table 3 belongs to an older version of the document, while in this version we write the material's properties are given in the text instead. Also notice, following the instructions from reviewer #2, we improved Table 1 (P6) and Table 2 (P8).

Captions The text of figure and table captions should provide more information so that the reader can easily understand the presented content. In the current version of the manuscript, the captions lack a consistent structure and information content.

This is also a solid point, and we agree with the reviewer. We have improved the captions of all figures.

Numbers and Units The authors did not implement the guidelines regarding the correct formatting of numbers and units.

Yes, we overlooked these guidelines, and we will solve the issue upon revision. We thank the reviewer for highlighting with detail in the text where this should be corrected, this helped us to revise the text. See the point by point corrections for details.

General I suggest that the authors consult and implement the manuscript preparation guidelines provided by The Cryosphere to ensure the manuscript meets the formatting requirements as well as general quality standards (especially with respect to sentence/paragraph structure and formulations). Obviously, in the current version, the manuscript does not fulfill standards expected for scientific publications. However, due to the relevance of the presented study for permafrost research in Greenland and taking into account the good quality of the (final) products of the study I suggest that the manuscript should be reconsidered for publication in The Cryosphere after major revisions.

We would like to thank the reviewer for this work; we appreciate the effort to help us improving the text.

Finally, we propose a major revision at L137, concerning the petrophysical analysis. In order to clarify the evolution of temperatures below -10°C , we re-saturated the samples and measured their electrical conductivity as a function of temperature in a thermostat bath. This protocol has been used in previous studies such as Coperey et al. 2019 (<https://doi.org/10.1029/2018JB017015>). We chose to redo these measurements because the measurements in the first version were done randomly between -8 and $+10^{\circ}\text{C}$. The temperature was not controlled and stagnated during the measurement, which led to an error. For these reasons, we chose to perform these measurements with a thermostat bath in order to control the temperature of the bath as well as the sample. We also extended the temperature range from -15 to $+15^{\circ}\text{C}$ and double checked that the sample temperature was at equilibrium before each measurement. The results of the measurements are more precise and very similar as they are within the same range of values.

Line-by-line comments

In the supplementary file, I provide detailed comments and suggestions that might help the authors during the revision of the manuscript. The annotations use the following code:

Highlighted (yellow) Should be addressed/considered during the revision.

Highlighted (red) Needs to be addressed/considered during the revision.

Strike-through (red) Remove.

Underline (red) Indicates repeated words in single sentences.

Thank you for this very detailed work. We respond to these comments with page and line number of the original comment, reviewer comment, authors' answer and, when relevant, page and line position of the new text.

[P1 L10] frozen/unfrozen

Now "frozen/unfrozen", P1L10

[P1 L12] until the end of the 21st century.

Now "until the end of the 21st century", P1L12

[P2 L27] Maybe add reference(s) here, e.g., PERMOS.

Added Pellet and Noetzli (2020); Isaksen et al (2022), P2L23

[P2 L42] as well as

Now "as well as", P2L36

[P2 L41] and more complex 3D

Now "more complex 2D", P2L37

[P2 L46] surface

Now "ground surface", P2L42

[P2 L47] Rephrase to improve sentence structure and readability.

Changed to:

" This approach has the advantage of reaching good performances while requiring only basic climatic input, i.e. air temperature and solar radiation.", P2L43-44

[P2 L55] Rephrase

Changed to:

"The statistical model is then used to compute time series of GST at any location in the landscape and for the period 1850-2022.", P3L60-61

[P2 L55] -/ ... use the COMSOL heat ...

-/ "Use" and "using" in one sentence; suggest to rephrase

Second "using" removed, P3L64

[P3 L57] Whitespace between number and unit missing. Found throughout the document - needs to be addressed.

Yes, this is has been corrected through the text

[P3 L58] based on

Accepted, P3L66 -67

[P3 L59] Wording could be improved.

Sentence changed to:

"This methodology develops a bidimensional transect of ground freezing conditions at a given survey date, which is compared to our 2D numerical simulations". P3L67-69.

[P3 L63] Check the guidelines on how to refer to figures (and tables) in the text:

<https://www.the-cryosphere.net/submission.html#figurestable>

Figures and tables are now referred following the guidelines.

[P3 L70] Reference?

This is based on the data from Cappelen et al 2021. This part has been modified and reference made explicit, P3L81-86

[P3 L75] In the area,

Sentence modified to:

"This climate locates Sisimiut in the sporadic permafrost zone (Obu et al.,2019; Biskaborn et al., 2019), and morphologically active rock glaciers are present in the area, reaching sea level elevation", P3L87

[P3 L75] Rephrase to improve wording.

Sentence modified to:

" This climate locates Sisimiut in the sporadic permafrost zone (Obu et al., 2019; Biskaborn et al., 2019), and morphologically active rock glaciers are present in the area, reaching sea level elevation", P3L87

[P3 L77] Is this sentence necessary?

Sentence removed

[P4 L86] Abbreviation ERT has not been introduced yet.

Now referred as "geophysical profile", P4L88

[P4 L90] ?

Removed

[P4 L91] Suggest to combine with section 3.1.1.

Now combined with 3.1.1 in section 3.1, P4L92

[P4 L93] Separate sentence?

We split the sentence in two, P5L102

[P5 L96] is a common trade-off

Accepted, P5L106

[P5 L99] Is this an assumption? Or can you provide data/references to sustain this?

This is based on Gubler et al 2011. Sentence changed to:

"According to Gubler et al. (2011), about 5-10% of the deployed loggers can be expected to fail and protecting the logger with plastic film (as we did) helps to reduce failures", P5L109-110

[P5 L103] Use either iButton or DS1922L (or both) - be consistent.

Yes, we use iButtons consistently through the document now

[P5 L105] Suggest to merge with section 3.1.1.

Yes, see answer [P4 L91]

[P6 L121] m.a.s.l. Ensure consistent usage within the document.

Yes, m.a.s.l. is used consistently through the document now

[P6 L122] (approximately 100 m high)

accepted, P6L137

[P6 L124] Which infrastructure was relevant for the decision (besides the walking path that is mentioned in the second part of the sentence)?

This summit is located near a road, simplifying the logistics. The sentence has been changed to:

"This specific mountain was chosen for its accessibility, as the road leading to the airport passes just nearby a short path that leads to a popular viewpoint to the summit." P6139-140

[P6 L125] conducted in early

accepted, P6L144

[P6 L125] 100 m long

accepted, P6L145

[P6 L126] 500 m long

accepted, P6L145

[P6 L126] deployed with 5 m spacing

accepted, P6L145

[P6 L128] improve the galvanic contact/reduce the contact resistances

accepted, P6L147

[P6 L129] Hard to follow - consider rephrasing. I suggest to provide a brief description of the electrical resistivity tomography method. Such description is particularly relevant as you mention "(in-phase) conductivity measurements [...] obtain[ed] at a frequency of 1 Hz" (line 145f).

We have added a brief introduction to the methodology:

"Electrical resistivity tomography (ERT) yields only qualitative information on the thermal state of materials because electrical conductivity depends on many parameters including water content, salinity, cation

exchange capacity, and temperature. The advantages of these geophysical methods are their low cost and the fact that they provide 2D or 3D tomograms/images of the subsurface." P6L141-144

[P6 L131] ... was extracted from a DEM based on electrode positions measured with a handheld GPS device.

accepted, P6L149

[P6 L136] What was the convergence criterion?

The difference between two successive data misfit error is smaller than a prescribed value.

[P6 L133] Provide further information about the filtering.

We have modified the sentence to:

"We cleaned 4% of the data 150 point acquired before the inversion (549 points acquired, 528 inverted) by filtering out the outliers from the pseudo section." P7L151

[P6 L138] two rock samples

accepted, P7L155

[P6 L139] ?

Sentence changed to:

"In addition to the field measurements, we performed a laboratory electrical conductivity experiment on two rock samples collected in the field from the rockwalls on the south and north face." P7L155-156

[P6 L141] were dried for 24 hours

accepted, P7L159

[P6 L140] were

accepted, P7L158

[P6 L142] are

accepted, P7L158

[P6 L143] Check if this is the correct format.

Format corrected, P7L161

[P6 L139] The three granite cubic core samples considered for laboratory analyses ...

accepted, P7L156

[P6 L146] Consider rephrasing to make it clearer and enhance readability.

Section changed to:

"We moved the freezing point temperature $TF = 0\text{ }^{\circ}\text{C}$ based on direct observations on instrumented boreholes for G-RF. The measurements with $TF = -3\text{ }^{\circ}\text{C}$ reflect the fact that the measurements were made only in the downward direction of the temperatures and not in the upward direction. These analyses define the relation between resistivity collected in the field and freeze-thaw conditions of the ERT transect." P7L166-169

[P6 L146] What does this frequency refer to? Was never introduced.

(See comment regarding a description of the ERT method).

The complex conductivity in the laboratory is observed by making measurements at different frequencies of the electrical field to observe the polarization of the ice between the grains for example. Only the data at 1 Hz is of interest to us here as it is representative of the field measurement conditions. Induced polarization can be done in the frequency or time domains. Usually we do frequency domain measurement in the lab and time-domain measurements in the field. We can go from one domain to the other through a Fourier transform analysis.

[P7 L152] evaluates

accepted, P7L172

[P7 L154] refers to

accepted, P7L174

[P7 L160] provides

Sentence modified to:

"The regression is then used to generate air temperature for the period prior to 1958", P7L182-183

[P7 L161] based on

See answer [P6 L160]

[P7 L161] yielding the air temperature values of dataset c.

See answer [P6 L160]

[P7 L163] from/through

accepted, P8L185

[P7 L165] Earth

accepted, P8L187

[P7 L166] Rephrase to improve sentence structure and readability.

Sentence modified to:

"The NorESM1 model is also chosen by several authors in Greenland for cryosphere evolution modelling due to its good performance in the region (Colgan et al., 2016; Hofer et al., 2020) thanks to his good performance in the region (Fettweis et al., 2011)." P8L189-191

[P8 L172] depends

accepted, P8L194

[P8 L174] Never introduced

Now introduced at P8L196

[P8 L177] Never introduced.

Now introduced at P8L200

[P8 L178] from
accepted, P8L198

[P8 L179] -/ What is i?
-/ Does * denote a convolution or just a multiplication?
-/ In the text, write variables/terms of the equation also in math style/mode.
We deleted this formula., as it is not providing useful information and generates confusion instead.

[P8 L187] in
accepted, P9L224

[P8 L200] Not clear - provide further information.
This information is trivial and confusing, as it repeats the previous section. It has been removed

[P8 L200] by using
changed to "trained with", P9L214

[P9 L206] which assumes the local thermal equilibrium hypothesis to be applicable/valid.
accepted, P10L232

[P9 L207] There is no Table 3 in the manuscript.
Thank you for the remarks. Table 3 belonged to an older version of the text. We now present the material properties directly in the text instead of using a table

[P9 L211] from
accepted, P10239

[P9 L209] Check correct formatting.
Formatting corrected, P10L236

[P10 L220] from the
accepted, P11L250

[P10 L224] The structure of this description is hard to follow - consider rephrasing.
Changerd to:
"The numerical model is calibrated for two parameters: matrix porosity and initial conditions in 1850. The calibration is carried out by simulating conditions in SIS2021-01, from 1850 to 2022 using a 1D geometry of a 100 m column. The simulation results are then compared to the field data acquired during the period August 2021 to April 2022. This is repeated for different combinations of matrix porosity and initial conditions, aiming to minimize the difference between data and model results." P11L254-256

[P10 L233] Little Ice Age?
Accepted, P11L263

[P10 L240] through
Accepted, P11L269

[P10 L244] profile
Accepted, P12L279

[P10 L245] are of interest

The entire sentence has been changed to provide a stronger argument for the 2D models location choice: "For this reason, we compute 2D model for two location of special interest: the ERT profile and the Nasaasaaq summit. The first location is chosen to compare the ERT data to our model, while the second location allows us to observe permafrost distribution and evolution in the tallest mountain in the study area." P12L178-281

[P10 L246] Unclear.

-/ What is the elevation profile z?

-/ Which solid?

Provide further information and a more precise description of the workflow.

The sentence has been modified to:

"For each location we set-up a north-south transect in the QGIS software, and used it to sample the elevation profile from the DEM. The elevation profiles are then imported into COMSOL as 2D geometry using the parametric function option. " P12L281-283. In COMSOL, 2D geometries are called "solid". This information is not necessary and is now deleted.

[P11 L249] See comment regarding the other equation above.

Equation changed accordingly, P12L285

[P11 L250] where

Changed to "where", P12L285

[P11 L251] from borehole

Changed to "from the borehole", P12L286

[P11 L244] I consider this a report of the sensor calibration and not results of this study.

We now present this briefly in the methods while describing the sensors, P5L106, and P5L111

[P11 L259] Likely 4.2.1

The whole section structure has been modified and this part belongs now to 4.1, P12L289

[P11 L265] Likely 4.2.2

This is now integrated in 4.1, P12L289

[P11 L260] In SIS2019-02, temperature data were

Sentence modified to:

"In SIS2019-02, the depth of zero annual amplitude is approximately 20 m. Below this depth, temperature data indicate a minimum of temperature of +0.3 °C, reached at 30 m depth, and a temperature of +1.0 °C at 100 m.", P12L301-303

[P11 L261] , which indicate

See answer P11L260

[P11 L261] whitespaces between number and unit missing

See answer P11L260

[P11 L262] The length of the time series does not allow for a precise delineation of the ALT?

We now do not talk about active layer, but rather of depth of zero annual amplitude: "Since temperatures are positive below the depth of zero annual amplitude, the measurements at SIS2019-02 indicate absence of permafrost.", P12L303-304

[P11 L266] Different number of loggers mentioned above.

Now corrected, P5L101

[P11 L267] Figure 3 does not show this.

Now corrected to "Fifteen loggers present snow free data (Fig. 3a), seven present thick snow cover and six present intermediate characteristics (Fig. 3b).", P12L291-292

[P11 L267] For

Sentence changed to:

"To show the effect of elevation, we compare two snow-free loggers installed on south facing rockwalls, one at sea level (MAGST = +3.5 °C) and at 460 m.a.s.l. (MAGST = +1.2 °C).", P12L297

[P11 L271] Use (and) instead of -

Sentence modified to:

"We used this value of +1.58 °C, comparable to previous findings in Greenland (Rasmussen et al., 2018), as constant offset when modeling snow cover." P14L322

[P11 L274] Also lateral variations observable.

Sentence changed to

"As shown in Fig.5b, the electrical conductivity tomograms acquired show a vertical and also lateral variations distribution of the conductivities", P13L308-309

[P11 L274] As shown in Fig. 4b, the electrical ...

See answer P11L274

[P11 L260] Why are the data not presented (figure or table)?

Data are now presented in figure 4b, P14L323

[P12 L276] How did you perform the petrophysical analysis?

We have added a brief introduction to the methodology: "“Before performing the laboratory measurements, the samples were dried for 24 hours then saturated under vacuum with degassed water from melted snow taken in the field. The samples were left several weeks in the solution to reach chemical equilibrium before performing the laboratory measurements. The water conductivity at 25 °C and at equilibrium was 0,0118 S m⁻¹ for G-DA and 0.0142 S m⁻¹ for G-RF and G-LR. The sample holder was placed in a heat-resistant insulating bag immersed in a thermostat bath (KISS K6 from Huber; bath volume: 4.5 l). The temperature of this bath was controlled with internal sensor the temperature of the sample was

control with external sensor with a precision of 0.1 °C. Glycol was used as heat carrying fluid and the conductivity measurements were carried out with the impedancemeter.”, P7L159-164

[P12 L275] ?

Figure 5 now shows the color bar with scale in resistivity and also in conductivity.

[P12 L276] -3.5

Agreed, P13L310

[P12 L277] close to

Agreed, P13L310

[P12 L277] Is this an assumption, an interpretation or an a-priori known fact?

This is an interpretation. The sentence has been moved to the discussion:

"This anomaly occurs near a large lithological fault, visible in the field. Overall, the observations indicate that ground characteristics at this location are not isotropic and a direct comparison between model and geophysics is not meaningful.", P20L401-403

[P12 L277] and the lower

This was a mistake, it is now changed to "the upper", P13L311

[P13 L284] Figure 5!

Almost all references to figures are wrong.

Figures references have been updated through the text

[P13 L284] For

Accepted, P13L317

[P13 L292] deviates

Accepted, P14L327

[P13 L292] from

Accepted, P14L327

[P13 L289] 0.03 is the value for which parameter/variable?

For porosity. Sentence now changed to "The optimal porosity was achieved for a value of 0.03, ", P14L323

[P14 L296] What are cold/warm colors? Please refer to the actual values (or colors).

Sentence removed, not relevant

[P14 L298] down

Sentence removed as already the concept is already explained in the methodology section, P11L269

[P14 L299] different?

This information is trivial, and omitted in the new version of the text

[P14 L300] Why is such plot relevant/needed?

This plot gives a straight-forward summary of ground temperature at different elevations and aspects. It is used in the text to give description of permafrost distribution in terms of elevation and aspect, P15L335-P16L338

[P14 L304] This sentence seems to be incomplete. What is the actual message?

Sentence modified to: "The colder MGT20 occurs on the north face of the Nasaasaaq peak (763 m.a.s.l.), reaching -4.0 °C.", P16L337

[P14 L308] Figure 7 does not compare modeling and ERT - maybe the authors meant to refer to Fig. 4b and c?

Figure is now properly referenced, P16L344

[P14 L314] This is presented in Figure 8c?

Accepted, P17L349

[P15 L316] 21st

Accepted, P17L351

[P15 L316] RCP 2.6

--> Needs to be consistent throughout the document. Check other occurrences.

This is now corrected through the text

[P15 L324] Formatting needs to be consistent in the entire manuscript.

Formatting now consistent: 2090-2100

[P16 L336] such as

accepted, P17L373

[P17 L337] ?

Sentence changed to "The images are computed using the 2D model of ground temperature evolution.", P19L386

[P17 L342] skyview?

accepted, P17L378

[P18 L346] Conclusions/Outlook?

Sentence deleted, as not providing relevant information

[P18 L348] Consider rephrasing

Paragraph deleted; this issue is now explained in the introduction, P2L45-54

[P18 L348] substantially influence

See answer P18L348

[P18 L352] Introduction/Methods?

See answer P18L348

[P18 L356] ?

Rephrase to make it clear.

See answer P18L348

[P18 L358] information ...

Paragraph rephrased, "On the other hand, our model is more reliable when describing ground temperatures below the depth of zero annual amplitude. Here, the model has a maximum error of 0.15 °C when compared to SIS2019-02, which is used only as validation dataset. SIS2019-02 has also different snow conditions than SIS2021-01, suggesting that our SnowP map and offset provide an acceptable boundary condition to model long term effect of recurrent snow cover induced by topographical patterns.", P19L395-P20L398

[P18 L360] How/where is this demonstrated?

See answer P18L360

[P18 L362] Read like part of an introduction. Consider rephrasing it as discussion otherwise move to the introduction or methods section.

The concepts described in this paragraph have been integrated in the introduction, P2L45-51, and in the conclusion, P21L445

[P18 L378] In particular,

Paragraph integrated in the conclusion, P21L445

[P19 L380] Introduction?

This paragraph has been removed, as focusing too much on slope stability, which is not the aim of this study.

[P19 L390] reducing

Paragraph integrated in the conclusion, P21L445

[P19 L402] Have you conducted a statistical analysis? If not, I would not use the term "significant(ly)".
accepted, P19L389

[P19 L404] ?

Sentence changed to:

"While Magnin et al. (2017a) had near-in situ long term weather station data, [..]", P19L391

[P20 L415] How is the "acceptable confidence" defined?

This is now omitted; we rather provide the measured uncertainty of our model compared to the validation data, P19L394-396

[P20 L415] Merge sentences and make the description more concise.

This concept has been developed more in detail:

"The general agreement between model and geophysical data also indicates that the model is suitable for describing permafrost extents in a wide range of elevations, slopes and aspects. Most of the disagreement

between model and data is due to the electrical conductivity anomaly on the north face of the geophysical profile. This anomaly occurs near a large lithological fault, visible in the field. Overall, the observations indicate that ground characteristics at this location are not isotropic and a direct comparison between model and geophysics is not meaningful.", P20L398-402

[P20 L438] 21st
accepted, P20L424

[P20 L439] 21st
accepted, P20L425

[P21 L448] Unclear -> rephrase.

This has been rephrased and moved to the conclusion: "Although the correlation between permafrost degradation and rockfall activity is accepted within the scientific community (Raveland and Deline, 2011; Patton et al., 2019), the process chain linking the two phenomena is very complex.", P21L441-443

[P21 L452] Conclusions

Accepted, rephrased to be integrated in the conclusion:
"Therefore, future efforts in the area should focus on investigating slope stability characteristics, and their relation to permafrost distribution and degradation.", P21L443

[P21 L468] selected

Rephrased as: "In this sense, our modeling approach based on weather parameters readily available for the whole region, downscalable with a simple topographical approach, provides a good first assessment for mountain permafrost zonation", P21L446

[P21 L460] depending on the considered scenario
accepted, P21L437

[P23 L511] Etzelmüller
corrected, P26L499

[P23 L514] Already published
corrected, P26L502

Referee #2

Anonymous Referee #2

Referee comment on "Characteristics and evolution of bedrock permafrost in the Sisimiut mountain area, West Greenland" by Marco Marcer et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-189-RC1>, 2022

In the manuscript "Characteristics and evolution of bedrock permafrost in the Sisimiut mountain area, West Greenland", a minimalist approach is used to model the spatial distribution and future evolution of bedrock permafrost in the region around Sisimiut, Greenland. Soil temperature measurements from the hydrological year 2020/21 and air temperature data from climate stations in the area are used as input data. The model results are verified with borehole data and a geophysical measurement (ERT). Accordingly, the manuscript addresses a current topic relevant to The Cryosphere. In general, the presented approach is extremely interesting, since a model of small-scale permafrost distribution could be achieved with relatively little data. Nevertheless, no fundamentally new concepts are presented. However, the factor that few publications exist on the distribution and future evolution of permafrost in Greenland makes the manuscript relevant. Unfortunately, there are a number of points of critique that outweigh the many positive aspects of the manuscript. This includes formal, structural as well as methodological and content-related aspects, which I will address below in general, as well as in specific comments within the manuscript. [We would like to thank the reviewer for this feedback. We could see that there has been a lot of effort to help us improve the text with reasoned, fair and challenging points. In addition, we appreciate the work done to improve the language in the text, thanks to several comments in the annex. In general, we can see that most of the comments regard the text structure and the way we present and discuss our results. We are glad to see that the reviewer agree with the fundamental aspects of the study and the methodologies. Here we respond to the reviewer's comments, to which we mostly agree, and that we will use as a guide to propose a new version of the text. Due to the variety of comment typology, we organized our answers in the following way:](#)

- [Response to the general comments found in the document "Comment on tc-2022-189". Some answers concern main issues that are often present in the annotated annex. These answers are referred in square brackets as, e.g. \[M1\], \[M2\].. The square brackets numbers are sometimes used in the point-by-point comments when relevant.](#)
- [Line by Line answers to the annotations in the annex file. This is presented with page and line number of the original comment, reviewer comment, authors' answer and, when relevant, page and line position of the new text.](#)

General comments

Formal Aspects:

- Overall, the manuscript is well written, but typos and incomplete sentences are frequently encountered. Also, a number of sentences are long and somewhat difficult to follow.
- [We agree with this comment. We would like to thank the reviewer \(also R#1 detailed this issue\) for the detailed noting on the text that helped us to improve readability.](#)

- Mathematical formulas, symbols, and units are not used consistently throughout the manuscript and according to The Cryosphere's specifications.
- Yes, the text was missing consistency on this aspect. We corrected the text according to TC's specifications.
- Cross-references to figures and tables are largely incorrect. Also, references in the text could be more precisely placed and all points in the figures (a, b, c..) could be addressed.
- Yes, there was a lot of confusion in the text and we apologize about that. Thanks to reviewer for marking in the text all these mistakes. We have improved referencing through the text by precisely addressing figures and subfigures.
- Furthermore, the figure captions and table headings could be more comprehensive in order to simplify the understanding of the results without having to read through the text. The font size of the axis labels in the graphics is inconsistent and sometimes too small.
- We have implemented the reviewer's suggestions and produced new figures with updated captions.

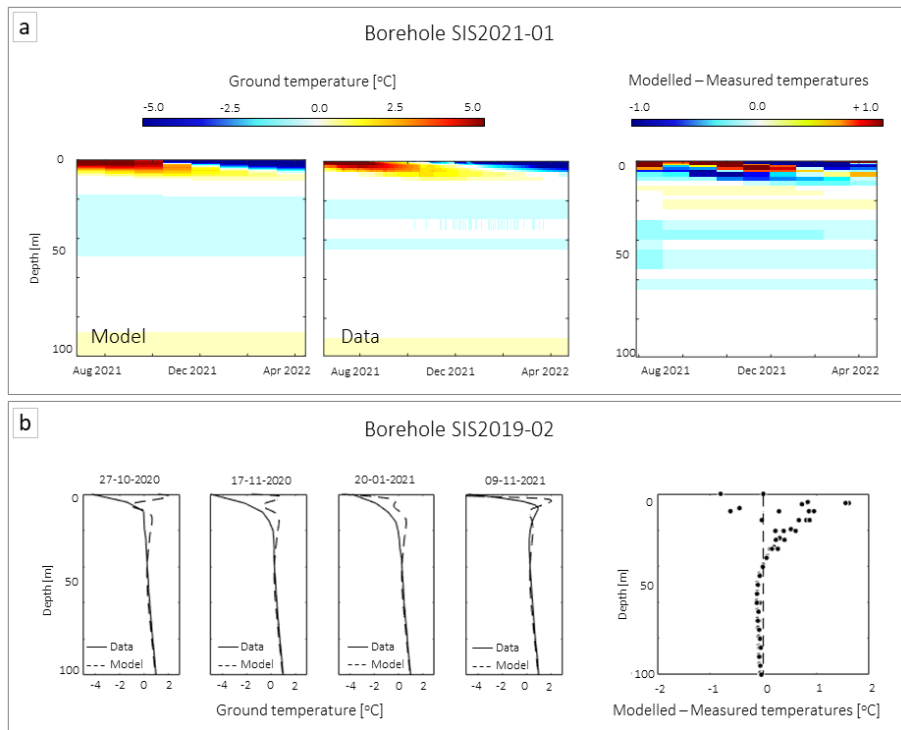
Structural Aspects

- [M1] Generally speaking, the manuscript could be structured more stringently. It should be examined whether one could separate more clearly between methodological background, results and the discussion or merge chapters. For example, climate data, temperature measurements, and geophysics provide a data basis for the modeling, are presented as results, but in contrast to the modeling results are not presented in great depth. Here one could check how far the manuscript can be restructured to address the results sufficiently without letting the main point of the manuscript fade into the background.
- [M1] We agree that the manuscript needed re-structuring. In particular, the discussion section contains several paragraphs that belong to the introduction. This causes some confusion through the text as the reader does not receive the necessary background to understand some of the methodology used and actual discussion, e.g. snow modeling with thermal offset. In some instances, we agree that some results should not be presented in the results section, as they are not the focus of the manuscript, but rather rapidly mentioned in the methods section, e.g. calibration results. In this sense, we are going to reshape the manuscript to improve readability, and would allow us to describe the results more in depth without losing the main point of the manuscript into details. In general, we used this comment to make a number of modifications through the manuscript. The major changes are the following:
 - We removed from the introduction the sentences describing slope stability issues.
 - We added to the introduction a paragraph describing snow cover modeling in permafrost research, and our strategy. This can be found at P2L45-54. In this sense, we discussion relative to snow cover modeling, as suggested in the annotated text by the reviewer.
 - The large portions of the discussion concerning slope stability have been compressed into few sentences and moved to the conclusion. This can be found at P21L442-447
- In some cases, the naming of the chapters does not perfectly match the contents. For example, the chapter "Ground Surface Temperature" is primarily about the calibration of the temperature sensors.

- Yes, good observation. We have adapted the chapter naming to match the actual content. Also, following the ideas from [M1] and the reviewer’s comment in the annotated text, we have condensed some subchapters into larger chapters, causing a substantial reorganization of the text’s structure. We now have, in the methods and results:
 - Ground temperature monitoring
 - Geophysical data
 - Modeling

Comments on the content

- In my view, the title of the manuscript does not optimally reflect its content. Perhaps a title can be found that focuses on the modeling approach as well as the spatial aspects of permafrost distribution, rather than the characterization of permafrost?
- Interesting observation; we agree with the reviewer and the title should better reflect the actual paper’s content. We suggest “Modeling and mapping bedrock temperature in the Sisimiut mountain area, West Greenland”.
- [M2] As noted above, I think some data could be presented much more comprehensively. This is particularly true for the borehole temperature data. For example, it is not really clear from the information presented whether permafrost - or at least perennial subsurface ice - is present in both boreholes.
- [M2] We agree on this point. To this regards, we have updated figure 6, which now appears in the text as figure 4, P14. This figure provides clear visualization of boreholes data. In addition, the text is failing to refer to this figure while presenting the boreholes results. In this sense, we have added proper referencing to the figure, to help the reader visualize the data we present in the results.



- It would also be interesting to show to what extent the climate data generated from stations 300 km to the south and 250 km to the north are truly representative of the Sisimiut region. This aspect does not appear again in the results section.
- Although this would be an interesting study, it is not covered by the paper's aim. What we aim to evaluate, among other issues, is how the available data are suitable to model permafrost temperatures. Our measure of performance is comparing model to data concerning permafrost only.
- [M3] The use of terms is partly not quite clear. Since the measurement series in the boreholes is only one year, it should be checked whether permafrost can be assumed with certainty, or one should speak of "frozen ground" in places. In my opinion it could be checked whether "Permanently Frozen" can be used instead of "Permafrosten".
- [M3] Yes, interesting point. It is true that we do not have more than two years of measurements, so we cannot define the ground as "permafrost". However, and this concerns SIS2021-01 and ERT profile, we observe negative temperatures at depths that are below the reach of the summer heat. Since these areas remain frozen under seasonal fluctuation, we talk about permafrost. We agree that this point is not clear through the text, and we have improved the text in this sense; now we do not refer to the "active layer", but to the "depth of zero annual amplitude". We agree that "Permanently frozen" is better than "Permafrosten", and have changed this through the text.

Fundamental aspects

[M4] The authors conclude that their "modeling approach based on few weather parameters, downscalable with a simple topographical approach, provides a good trade-off between results quality and uncertainty". Even though I have to agree that the results look very promising and are graphically presented in an excellent way, I miss some basic information to evaluate the quality of the results. This concerns the representativity of the boreholes (Lines 404-405) as well as the input climate data (Lines 402-404), the length of the measurement period (Lines 344- 345) as well as the influence of the snow cover (Lines 348-361). Points that are addressed very critically by the authors themselves in the course of their discussion. However, justifications, why the results can be regarded as representative nevertheless, come somewhat briefly.

[M4] Yes, this is a very good point. The discussion on the uncertainties is more substantial than the discussion on the validation, and the reader is left in doubt about the actual value of the study. We believe that a thorough description of uncertainties is necessary to develop further studies in the region, but we also believe that our results are valuable despite these uncertainties. The main point that we have now developed in the text are the following:

- Our heat transfer model reproduce deep ground temperatures, i.e. below the depth of zero annual amplitude, within 0.15 C for both boreholes (P14L331). This is key result as, thanks to ground thermal inertia, deep ground temperatures are influenced by climatic trends rather than short-term variability. In this sense, our heat transfer model has good performance in predicting deep ground temperatures despite the short measurement period. This suggest that the GST model has good coupling with the available weather data.
- These boreholes have similar elevation/aspect, but substantially different snow conditions. Using our snow modelling approach, we manage to model deep ground temperatures accurately. Although we miss the seasonal variability induced by the snow-ground complex interactions, this method is suitable for the goal of modeling deep ground temperatures in varying snow conditions, when snow conditions are determined by topographical patterns (P19L397).

- The ERT data are acquired to test the model at varying elevation and aspects, and overcome the limited representativity of the two boreholes in mountain terrain. Our model is able to reproduce the observed temperature patterns. This result is very important because it allows us to trust the model in complex topographical settings, which is fundamental for mountain permafrost mapping (P20L398).

[M5] Rather, it is written absolutely reasonable that "(...) the time period covered by our data is still too short (only one year) to fully understand the predictive performance of our model. Maintaining the operational (of) the GST monitoring network and updating the model as time passes will be crucial to define with more confidence this source of uncertainty". The question arises whether it would not be within the scope of possibilities either to optimize and extend the data basis, or to check and justify more clearly that the currently available results are nevertheless relevant and representative, and are superior in their significance to simpler models.

[M5] We would like to point out that this sentence describes the performance of the GST model, not the modeling performance in its whole (GST + heat transfer). The meaning of this sentence is that we achieve excellent fit between data and model, but we are cautious to suggest that this empirical approach is as good as the CryoGrid SEB, since we do not have long-term data. The methods and results restructuring now clearly separates GST and heat transfer models. We have also rephrased the discussion accordingly, and hopefully this concept is clearer now.

We would like to point out that our model (GST + heat transfer) offers a substantial improvement to previous studies in Greenland, by reducing the spatial resolution from km to m scale, by calibrating and validating on dedicated data, and by including topography in downscaling. We have now specified the weaknesses of available permafrost models in the region, and how they are not suitable for characterizing mountain permafrost (P2L25-28).

In my opinion, the current state of the manuscript does not meet the requirements of a scientific publication as well as the quality standard of The Cryosphere. As already noted, I think that the work has very good approaches and an absolute relevance. For this reason, I would like to suggest that the manuscript should be reconsidered for publication in The Cryosphere after a major revision. Due to the large number of comments and suggestions, these are included in the supplemental document. Comments marked in yellow correspond to basic comments that should be considered in the revision. Green markings refer to phrases and sentences that should be rephrased to facilitate understanding.

We thank the reviewer for the feedback and we agree with the comment. We would also like to thank the reviewer for the high quality work that was done to improve our manuscript, with both general remarks as well as detailed comments in the annex. The revision has been provided with competence and detail.

[M6] Finally, we propose a major revision at L137, concerning the petrophysical analysis. In order to clarify the evolution of temperatures below -10°C , we re-saturated the samples and measured their electrical conductivity as a function of temperature in a thermostat bath. This protocol has been used in previous studies such as Coperey et al. 2019 (<https://doi.org/10.1029/2018JB017015>). We chose to redo these measurements because the measurements in the first version were done randomly between -8 and $+10^{\circ}\text{C}$. The temperature was not controlled and stagnated during the measurement, which led to an error. For these reasons, we chose to perform these measurements with a thermostat bath in order to control the temperature of the bath as well as the sample. We also extended the temperature range from -15 to $+15^{\circ}\text{C}$ and double checked that the sample temperature was at equilibrium before each measurement. The results of the measurements are more precise and very similar as they are within the same range of values.

Line-by-line comments

[P1 L15] The definition could be more precise and supported with references.

Changed to:

"The term "permafrost" defines ground presenting temperatures that remain below 0 °C for at least two consecutive years. In cold mountain regions, complex topography influences shading, snow distribution and ground type, causing a highly variable distribution of ground temperatures and permafrost (Etzelmüller, 2013)", P1L15-17

[P1 L17] Haeberli is always a good source, but this statement could be supported with more recent literature. Possibly, among other sources, this one: <https://doi.org/10.1016/j.geomorph.2019.04.029>

Sentence changed and newer references added:

"Several field studies describe a significant correlation between warming climate, mountain permafrost degradation and increased slope instability, observed as rockfall frequency (Raveland and Deline, 2011; Gallach et al., 2020), large rockslide occurrence (Patton et al., 2019; Guerin et al., 2020; Frauenfelder et al., 2018; Walter et al., 2020), high elevation infrastructure destabilization (Duvillard et al., 2019) and debris permafrost creep rate increase (Marcer et al., 2021).", P1L17-21

[P1 L18] Please provide references

See answer P1L17

[P1 L6] ground surface temperature data

accepted, P1L5-6

[P1 L6] more specific?

for the hydrological year 2020/21?

Accepted, sentence changed : "We first acquire ground surface temperature data for the hydrological year 2020/21 to model bedrock surface temperatures time series from weather forcing on the period 1850 - 2022.", P1L5-7

[P1 L14] In terms of content, I don't think the introduction is very balanced. Slope stability is very present, but is not further addressed in the rest of the manuscript. On the other hand, the state of research is not fully presented. The influence of snowpack, which is frequently addressed later, does not appear here, and other points raised in the discussion would fit better in the introduction.

This is a valid point and we thank the reviewer for the remark (see also [M1]). We agree that slope stability takes too much of the introduction, while other issues are not presented at all – as influence of snowpack and modeling state of the art. This issue also drags into most of the discussion, as pointed out by the reviewer later in the text. Therefore, according to the reviewer's notes, we have restructured the Introduction accordingly. The paragraph regarding slope stability has been deleted. We instead present a more detailed state of the art of available permafrost models in Greenland - P2L24-31. We also provide a paragraph describing snow influence and modeling strategy:

"Another challenge in modeling mountain permafrost is due to the influence of snow cover. Snow is known to cause severe disturbance to ground surface temperatures, which can significantly affect active layer thickness even when accumulating in isolated patches (Magnin et al., 2017b). Models are sensitive to snow

characteristics, causing estimation of permafrost extents to greatly vary depending on the modeling assumptions (Czekirda et al., 2019). Although some numerical models are able to describe snow physics at hectometric resolution (Gisnås et al., 2014), it becomes extremely challenging to achieve a good knowledge of spatial characteristics of the snow in complex terrain, given the spatial variability of weather forcing, as wind and shading. To overcome this issue when modeling mountain permafrost, snow is often accounted with a topographical approach, based on filtering snow covered areas using a slope threshold (Magnin et al., 2019) to exclude them from the model or to apply specific offsets (Boeckli et al., 2012). Overall, this method allows for a first order quantification of ground temperatures in complex terrain when detailed snow data are not available.", P2L44-53

[P1 L14] Also, the objective could be formulated more clearly. The objective of quantifying permafrost conditions based on modeling in the Sisimut region somewhat exceeds the content of the manuscript, as no reference is made to the entire Greenland.

We also agree that the object of the study is not properly defined, as our results are limited to a small area in Greenland, and by no means are relevant at the national scale. The objective is now formulated more clearly as follows:

"The aim of this study is to move a first step towards a high resolution regional characterization of mountain permafrost in Greenland. To do so, we focus on the Sisimiut area, (68° N on the west coast), where we have a relatively large amount of data.", P1L55-P2L57

[P1 L0] The title, in my opinion, could be a little more closely tailored to the contents of the manuscript. Perhaps the modeling aspect as well as the spatial permafrost distribution could be brought to the focus a bit.

The title is now changed to:

"Modeling present and future bedrock permafrost distribution in the Sisimiut mountain area, West Greenland", P1L0

[P2 L26] references? if with reference to the preceding sources, please rephrase

It is now rephrased as following:

"Therefore, understanding the spatial distribution of mountain permafrost and its future evolution is a key step in understanding these hazards, and several countries started comprehensive programs to monitor this phenomena as a basis for risk assessment (Pellet and Noetzli, 2020; Isaksen et al., 2022).", P1L21-P2L23

[P2 L29] please provide references

There are no references because, as the sentence explains, there is no study dealing with this specific issue. References relating to available models for lowland permafrost now presented as following:

"Available models are based on numerical simulations at kilometer scale (Daanen et al., 2011), are not calibrated with in-situ data (Gruber, 2012), or valid for sedimentary terrain only (Obu et al., 2019).", P2L25-26

[P2 L45] ground surface?

accepted, P2L42

[P2 L50] on?

accepted, P2L56

[P2 L49] Could you be a little more specific about the objective? The focus is clearly on the Sisimiut region. A permafrost quantification related to Greenland is not given. And regional approaches to permafrost quantification already exist in Greenland, so "first" seems a bit imprecise.

Notice that available models at regional scale do not provide quantification of mountain permafrost, due to their low resolution are lack of representativity in mountain terrain/bedrock. This now highlighted as following:

"Available models are based on numerical simulations at kilometer scale (Daanen et al., 2011), are not calibrated with in-situ data(Gruber, 2012), or valid for sedimentary terrain only (Obu et al., 2019).", P2L25-26

The sentence concerned by this specific comment, has been changed to the following:

"The aim of this study is to move a first step towards a high resolution regional characterization of mountain permafrost in Greenland. To do so, we focus on the Sisimiut area, (68° N on the west coast), where we have a relatively large amount of data.", P1L55-P2L57

[P2 L49] Furthermore, you write in chapter 3.2.6 that the analysis of permafrost evolution would be a goal. Does that also apply here?

This goal is now explicit as following:

"Finally, we model future evolution of permafrost distribution in the area using scenarios RCP 2.6 and RCP 8.5, observing a relevant permafrost loss.", P3L69-70

[P2 L50] perhaps rephrase? In general, it would be nice if accurate weather data were available, but if not available there is a need for alternatives I suppose. Furthermore, instead of writing "a large number" just write 28 loggers. I am not sure if 28 is really a large number.

Sentence changed as following:

"In fall 2020 we installed 28 surface temperature loggers in the area measuring Ground Surface Temperature (GST), covering the local range of elevations and aspects.",P3L57-58

[P2 L53] difficult sentence. "allowing creating" please rephrase

Sentence changed as following:

"The statistical model is then used to compute time series of GST at any location in the landscape and for the period 1850-2022.",P3L60-61

[P2 L41] Noetzli! please correct throughout the document and in the bibliography.

accepted, P2L37, P26L587

[P2 L31] what is the difference between this and your study? You also criticize later that the borehole data are not representative?

Partly of this point can be referred to [M4]. In addition to that, notice that previous studies do not have data describing variability due to local topography nor bedrock conditions. This is because they do not focus on mountain permafrost, but rather on large scale sedimentary permafrost patterns. In our study, we have the GST network covering the local range of elevations and aspects, boreholes drilled in bedrock, and high elevation geophysical data. This allow us to increase the model resolution from 1 km (Obu et al, 2019) to 10 m, which describes the spatial variability of mountain permafrost and is more suitable for the purpose of mapping permafrost in a small area. We made this concept more clear through several modifications. In particular at:

- "Available models are based on numerical simulations at kilometer scale (Daanen et al., 2011), are not calibrated with in-situ data (Gruber, 2012), or valid for sedimentary terrain only (Obu et al., 2019).", P2L25-27
- "A major challenge when modeling mountain permafrost in this region is due to data availability, as ground temperature data are limited to few low-land sedimentary boreholes that are not representative for higher elevation and complex terrain (Obu et al., 2019). A common strategy to overcome this issue is based on the approach developed in Switzerland in the early 2000's (Gruber et al., 2004) relying on a network of permanent surface temperature loggers", P2L32-35
- "The aim of this study is to move a first step towards a high resolution regional characterization of mountain permafrost in Greenland. To do so, we focus on the Sisimiut area, (68° N on the west coast), where we have a relatively large amount of data. In fall 2020 we installed 28 surface temperature loggers in the area measuring Ground Surface Temperature (GST), covering the local range of elevations and aspects.", P2L55-58
- "We test our model for 1D simulation, which we compare to temperature data obtained by two 100 m deep boreholes drilled in bedrock at low elevation in 2019 and 2021. To obtain field data on ground temperature at high elevation, we used the approach proposed by Duvillard et al. (2020) based on geophysical surveys and calibration of resistivity - temperature dependencies in laboratory experiments.", P3L64-67

[P3 L62] repetition of words, please rephrase

Sentence changed as following:

"Our study site is located in the mountains surrounding Sisimiut, a city on the coastline of the widest non-glaciated area in West Greenland, about 200 km from the Greenland Ice sheet (see Fig.1).", P3L73-74

[P3 L68] additional references should be added for these points. The evidence is not optimal like this. Temperature means are given for the period 2000-2020, precipitation means for 1961-1990, without reference. No sources are provided for temperature trends either. The information that they have not changed precipitation needs to be substantiated. Furthermore, it would be interesting to know if precipitation has remained constant as an annual average, or if there may have been seasonal variations in precipitation.

Following the comment, the sentences have been rephrased as following:

"Climatically, Sisimiut is located in the low arctic oceanic area, and weather data are recorded at the airport weather station (Cappelen et al., 2021; Cappelen and Jensen, 2021). The warmest month is July (6.3 °C on average), while the coldest is March (-14.0 °C). Mean annual air temperature increased from -3.5 °C in 1961-1981 to -1.8 °C in 2000-2020. Mean annual precipitation decreased from 509 mm in 1961-1981 to 422 mm in 1984-2004, year in which the rain gauge was decommissioned. Decrease in precipitation concerns both solid (mean monthly precipitation in January-April decreased from 28 mm in 1961-1981 to 25 mm in 1984-2004) and liquid (mean monthly precipitation in June-September decreased from 58 mm in 1961-1981 to 49 mm in 1984-2004) precipitation.", P3L81-87

[P3 L77] In my opinion, you could either increase the information content here or delete the sentence

Sentence deleted

[P3 L76] Can you mark the block glaciers in the map / figure 1? Are there any references to the activity of the block glaciers?

Rock glaciers are now marked in the figure. No, these rock glaciers have not been investigated so far, so there no references. The activity is judged by us using the morphological characteristics of the front.

[P3 L75] sentence structure, please rephrase

Sentence rephrased as following:

"This climate locates Sisimiut in the sporadic permafrost zone (Obu et al.,2019; Biskaborn et al., 2019), and morphologically active rock glaciers are present in the area, reaching sea level elevation", P3L87-88

[P3 L80] The content of the chapter refers primarily to the calibration of the loggers and therefore does not fit the title. Maybe change the general structure of the subsections in chapter 3? Maybe instead of the subdivision into Field data and Modeling, Temperature Data, Geophysiscs and Modeling?

The strucutre of chapter 3 has been changed according to the comment

[P3 L65] Table 1 lists that 11 loggers were installed in the soil. Can you provide any information about the substrate other than the bedrock? Is there block cover, soil, and what are the conditions? I think especially with reference to heat fluxes it would be important to know the ground cover.

This is now specified as following:

"In soil conditions, loggers where placed in 50 mm holes manually dug in gravel.", P5L118-119

[P3 L56] Please provide a reference for COMSOL (COMSOL Multiphysics Simulation Software?) and rephrase the sentence.

Sentence rephrased as following:

"In this study, we use COMSOL Multiphysics® heat transfer module, connected to Matlab through LiveLink (COMSOL Inc., 2015).", P3L63-64

[P4 L86] abbreviation has not yet been introduced

We now adress it here as "geophysical profile", P4L88

[P4 L86] ground surface temperature sensors?

Referred now as "GST sensors", P4L88

[P4 L86] is shown is used for a-c. Maybe rephrase

Rephrased as following:

"Map of the entire study area (a), with location of deep boreholes SIS2019-02 and SIS2021-01 and rockglaciers fronts, identified by green stars. Detail of the Nattoralinnguaq area, where most of the GST sensors are installed (b). South face of Nattoralinnguaq and Miguttunguup Qulaa (picture taken from Sisimiut in October 2020) with GST loggers and geophysical profile locations (c).", P4L88

[P4 L90] ?

Now deleted

[P4 L91] Maybe call this chapter GST Monitoring?

Now called "Ground temperature monitoring", to include borehole in the same section as proposed in P3L80.

[P4 L97] maybe split sentence?

Sentence splitted and rephrased as following:

"We established a GST monitoring network consisting of 28 individual monitoring locations, covering as evenly as possible the range of aspects, elevations and slopes at the study site. Data were acquired for one year, from fall 2020 to fall 2021.", P5L101-102

[P4 L94] Please support this statement with additional or more appropriate sources. Gubler et al. (2011) deal with iButtons in detail, but in Gruber et al. (2004) I could not find any reference to iButtons nor to M-Log 5. Thus, references to M-Log5 are completely missing.

Sentence rephrased and references added:

"Both iButtons and Geoprecisions are widely used in permafrost studies and the community has previous experience in their strength and weaknesses (Gruber et al., 2004; Gubler et al., 2011; Magnin et al., 2015a, 2019; Hipp et al., 2014; Schmidt et al., 2021; Duvillard et al., 2020).", P5L103-105

[P5 L96] The information on the data loggers is difficult to follow. There are several points here.

- The Geoprecision loggers could be named more clearly (M-Log5 (W?) Rock?).
- Is it correct that TNode sensors are installed? They are listed on the Geoprecision page for the ThermistoreStrings, while PT1000 sensors are listed for the M-Log 5.
- The use of temperature resolution and uncertainty (chapter 3.1.1) is not quite clear to me. Here it would be very useful to list the accuracy as well. This is clearly different between M-Log and iButtons. In this respect, I cannot quite understand why the uncertainty can be better than 0.02 °C when the accuracy of the iButtons is +/- 0.5 °C according to the manufacturer.

Thanks for the comment; we agree to modify the table accordingly. To answer the specific questions:

- We will name the loggers correctly, according to manufacturer: M-Log5W-Rock and M-Log5W-String
- Good point. TNode sensors are installed only in the M-Log5W-String. PT1000 are installed in M-Log5W-Rock
- We add the resolution and accuracy as declared by the manufacturer.

The table has been modified according to the comment, P6L133

[P5 L102] Can you provide information on how the loggers were installed? Was drilling done? How were the iButtons installed in the soil at a depth of 5 cm, how in the Bedrock?

This information is now added as following:

"These loggers were placed by drilling a 10 x 300 mm hole and sealing the sensor using frost resistant resin. For The iButtons (19 loggers in total), were installed in other more accessible conditions, such as flat bedrock (6), soil (11) and easy-access rockwalls (2) (See Fig. 1). In bedrock conditions, loggers were placed in 22 x 100 mm holes, sealed with a mixture of sand and frost resistant sealant. In soil conditions, loggers where placed in 50 mm holes manually dug in gravel.", P5L115-119

[P5 L110] Information about the HOBO logger, regarding accuray and resolution is missing. In my opinion, it would improve the overview if the technical information about all loggers or temperature sensors were prepared together and illustrated in one table. Then a chapter for the installation and positioning of the loggers could be written.

See main answer [M4]. The table has been modified according to the comment, P6L133

[P6 L120] Only one ERT transect was measured? Rename chapter with reference to ERT?

Chapter renamed as "Geophysical data", P6L134

[P6 L123] what infrastructure elements other than the path were relevant? Were there possibly also scientifically relevant reasons for the site selection?

This is now specified as following:

"This summit presents typical characteristics of the mountains in the Palasip Qaqqā– Sammisōq ridge: a steep and rocky south face approximately 100 m high with a debris slope underneath, and a more gentle north face characterized by small vegetation patches and some short steeper sections. This specific mountain was chosen for its accessibility, as the road leading to the airport passes just nearby a short path that leads to a popular viewpoint to the summit.", P6L137-145

[P6 L122] These informations would be interesting in the study site description

This is a specific summit within a mountain range. We therefore provide in the study site description the description for the typical mountain characteristics:

"The mountains of the region typically have pyramid-shaped summits and steep rockwalls generating debris slopes underneath. Mountains are dominated by bedrock, although vegetation patches are common at up to 400 m.a.s.l.", P3L78-80

While here we state that this specific summit "[...] presents typical characteristics of the mountains in the Palasip Qaqqā– Sammisōq ridge: a steep and rocky south face approximately 100 m high with a debris slope underneath, and a more gentle north face characterized by small vegetation patches and some short steeper sections.", P6L137-140

[P6 L129] Please rephrase, sentence is difficult to understand. Especially as there is no background on ERT the 4-point setup of the Wenner Array is difficult to understand. Furthermore you could add further Information like the Voltage you used

We simplified the sentence:

"The Wenner configuration was used because of its best signal-to-noise ratio in complex environments (Dahlin and Zhou, 2004; Kneisel, 2006).", P7L148-149.

The use of the Wenner protocol in complex environments is very common, and we suggest to not enter the details of this methodology as we are presenting it from the user's perspective only. We used a voltage between 0 and 600 volt. We suggest not adding this information as it is not useful

[P6 L133] please provide background on the filtering

Sentence rephrased as following:

"We cleaned 4% of the data point acquired before the inversion (549 points acquired, 528 inverted) by filtering out the outliers from the pseudo section.", P7L150-152

[P6 L136] Third iteration sound quite early to me. Please provide information on the convergence criterion

The data here are of good quality and the convergence criterion was taken from having the difference between the data misfit value below a prescribed threshold.

[P6 L134] sentence structure - based on? What do you mean with the pseudo section?

A pseudo-section is a depth representation of the measurements (apparent resistivity using Ohm's law). For each quadrupole, we can define a pseudo-depth from the distance between the electrodes. The data are shown on a cross-section called a pseudo-section. Sentence rephrased as shown in P5L133

[P6 L131] sentence hard to follow, please rephrase.

Sentence rephrased as following:

"Topography was extracted from a 2 m resolution digital elevation model (DEM, Porter (2018)) based on electrode positions measured with a handheld GPS device.", P7L149-151

[P6 L132] What is the spatial resolution of the DEM?

2 meter, now added to the text, P7L150

[P6 L138] `two rock samples

accepted, P7L155

[P6 L140] were

please rephrase sentence

Sentence rephrased as following:

"The three granite cubic core samples considered for laboratory analyses (sample G-RF, G-LR and G-DA) are characterized by a porosity of $\Phi = 0.032$ for G-RF, $\Phi = 0.015$ for G-LR and $\Phi = 0.023$ for G-DA." P7L156-158

[P6 L141] were

accepted, P7L158

[P6 L141] for 24 hours

At what Temperature?

Sentence rephrased as following:

"Before performing the laboratory measurements, the samples were dried for 24 hours at 60 °C, then saturated under vacuum with degassed water from melted snow taken in the field.", P7L158-159

[P6 L142] were

accepted, P7L159

[P6 L143] repetition of words (previous sentence)

Sentence rephrased as following:

"The samples were then left several weeks in the solution to reach chemical equilibrium", P7159-160

[P6 L145] unclear information. Measurements were conducted under room temperature, after samples were removed from the freezer?

See [M6]

[P6 L146] please provide background on frequency

This is the frequency of the electrical field used to do frequency-domain induced polarization.

[P6 L146] sentence hard to follow, maybe rephrase.

Sentence rephrased as following:

"We moved the freezing point temperature $TF = 0$ °C based on direct observations on instrumented boreholes for G-RF. The measurements with $TF = -3$ °C reflect the fact that the measurements were made only in the downward direction of the temperatures and not in the upward direction", P6L166-169

[P6 L148] This sounds like the aim of the Lab Analysis, maybe move it to the beginning of the chapter?
Accordingly, we moved the sentence to P7L156-157

[P7 L152] please rephrase

Sentence rephrased as following:

"The methodology evaluates Ground Surface Temperature (GST) time series with an empirical approach, which are then used as upper boundary conditions for a heat transfer numerical model.", P7L172

[P7 L167] its

accepted, P8L189

[P7 L163] from

accepted, P8L185

[P7 L149] the information in the text and in the table on the data basis is somewhat confusing. Perhaps you could make this a little clearer and optimize the table.

- It is not immediately clear in the table that dataset a already consists of data from two weather stations.

- It is not obvious without reading in the text, why for datasets a, b and c temperature data for the period until 2021 are available, but only until 1979 are used

- it is understandable that reanalysis data are used to include non-measured parameters. But, why are the temperature data measured in Sisimiut not used anyway?

- if I understand it correctly, the merged data set c includes the solar radiation from d? If so, this information should be visible in the table.

We improved the table accordingly to the comment, P8L179.

In particular:

- We changed the datasets names, giving priority to the datasets directly used in the modeling.
- We highlight that dataset a is generated from datasets f and c. Datasets f are used only to generate air temperature for dataset a, therefore they are described separately from the datasets directly used in the modeling.
- We use as much as ERA5 data as available, since they available in the whole region. This helps us to understand the model performance with generic data available everywhere in Greenland

[P7 L152] published in 2022

accepted, P7L172

[P7 L160] 2021?

accepted, P8L182

[P7 L163] can you provide clear references for the Copernicus database and the CryoGrid SEB?

References added: Hersbach et al, 2020; Westermann et al, 2016, P8L186-187

[P7 L165] Earth

accepted, P8L187

[P8 L175] Three times PISR in one sentence. Could you try to rephrase?

Sentence rephrased as following:

"Solar radiation forcing is downscaled using the ratio between the PISR at each logger location and the PISR at the ERA5 reference grid", P8L198-199

[P8 L177] abbreviation has not yet been introduced. could you insert the reference to Conrad et al. (2015) immediately after SAGA to make the context clearer?

The reference Conrad et al. (2015) refers to the PISR module, not to the software SAGA. The sentence has been rephrased as following:

"The PISR map is evaluated using the software System for Automated Geoscientific Analyses (SAGA) and the module PISR (Conrad et al., 2015).", P8L199-200

[P8 L180] Partial repeat to sentence in line 175. Can this be reworded and summarized more specifically?

Formula has been removed as it does not bring substantially new information. This paragraph has been included in a dedicated section "Snow cover modeling", P9L215.

[P8 L187] Could you provide additional information? How many Pictures, how many Winters? What Year and Month? Several Pictures per Winter?

Furthermore, it should be "by interpreting" and "in a GIS"

Sentence rephrased as following:

"This dataset is created by interpretation of 5 landscape pictures of the study area taken at the peak of the snow accumulation season (late April) in winters 2021 and 2022. We manually assigned snow/no snow areas in a GIS and combined this dataset to terrain parameters to train a binary classifier.", P9L222-224

[P8 L183] Considering the fact that snow thickness has significant influence on the ground thermal regime, is there also information on the thickness of the snow cover?

No, we do not have that information, as we found very difficult to model snow depth distribution at the landscape scale. This is due to the combination of strong winds, relatively shallow snow cover and rugged terrain. For this reason we use only probability of presence/absence, under the hypothesis that this is a proxy for snow depth too. This hypothesis finds support by the fact that snow depth tends to be consistent across seasons in the arctic, due to the dominance of strong winds in spatial distribution patterns. This concept is now presented as following:

"The classifier provides a probability of snow cover for a given set of curvature and slope, which can be extrapolated at the landscape scale creating a map of snow cover probability SnowP (Fig.2). The map identifies drift traps where snow is most likely to accumulate. The validity of this method is based on the observation that snow drift patterns in the arctic are generally stable over time due to relatively dry and windy weather (Parr et al., 2020).", P9L225-228

[P8 L192] 2022

accepted, P9L205

[P8 L198] logger time series?

Sentence rephrased as following:

"We then split this database into training and validation sets, following a pseudo-randomized cross validation approach, as we randomly exclude entire GST time series from training.", P9L212-214

[P8 L199] allows the observation of

Sentence removed as it does not provide substantially new information

[P8 L200] This is difficult to understand. Could you rephrase and provide additional information?

This sentence is misleading and does not provide substantially new information. We removed. The relevant information is now given by:

"While GST data from the period 2020-2021 are used as dependent variable, the climatic datasets overlapping on the same periods are used as predictors. Datasets a, c and d (see Table 2) are fitted to the GST data using air temperature and solar radiation as predictors. For dataset b, we also use cloud cover, dew point temperature, total precipitation, wind speed and direction as additional predictors.", P9L208-211

[P9 L202] snow drift?

accepted, P10L232

[P9 L202] maybe include a) and b) or top and bottom for clarification?

accepted, figure updated, P9L232

[P9 L202] 1 and 2 partly cover interesting parts of the picture. Could you move the dots a little?

accepted, figure updated, P9L232

[P9 L206] please rephrase

Sentence rephrased as following:

"The heat transfer process is modelled using the "heat transfer in porous media" module in COMSOL, which assumes the local thermal equilibrium hypothesis to be valid", P9L231-232

[P9 L212] what do you mean with subsurface? Could you be more specific?

Sentence rephrased as following:

"For the 1D model we entered a custom function to describe the matrix density, which was evaluated from the cores extracted from SIS2021-01, providing an empirical function of depth increasing from 2600 kgm⁻³ at the surface to 3000 kgm⁻³ at 20 m depth, and being constant until 100 m depth", P10L238-240

[P9 L209] In chapter 2 you write that predominantly amphibolitic gneiss occurs in the study area. In 3.1.5 you also give values for porosity. Can you prove that the values assumed for granite are really applicable for the occurring rock?

We tested the sensitivity of our model to these parameters, by imposing different values typically found in igneous rocks (see Cermak et al, 1982, doi:10.1007/10201894_62). The results indicate that the model is not very sensitive to these parameters, as we obtained ground temperature differences below 0.01 C below the depth of annual amplitude. The most sensitive parameter is porosity - as suggested by previous studies, e.g. Noetzli and Gruber, 2009. For this reason we calibrate porosity and leave other parameters as COMSOL default. This is now summarized in the following sentence:

"quick sensitive analysis shows that the model computes ground temperature differences smaller than 0.01 °C when varying these parameters within the typical ranges of different crystalline rocks.", P10L236-238

[P9 L212] Why do you use different parameters for 1D and 2D Models? If values increase from 2600 to 3000, why do you apply 3000 as the average?

The argument here is the same as above at line 209; the model is not sensitive to this parameter. Since for the borehole we have a precise quantification of density, we use it. However, we are not sure how this is representative for the rest of the study area. This is now summarized in the following sentence:
"We attributed the constant density of 3000 kgm⁻³ to the 2D models, as it not known if the near-surface values measured at SIS2021-01 are representative for the entire study area.", P10L240-241

[P10 L225] ...carried out by...?
accepted, P11L253

[P10 L225] sentence is hard to understand; please consider rephrasing.

Sentence rephrased as following:

"The calibration is carried out by simulating conditions in SIS2021-01, from 1850 to 2022 using a 1D geometry of a 100 m column. The simulation results are then compared to the field data acquired during the period August 2021 to April 2022.", P11L253-255

[P10 L229] Why did you test different porosity values instead of using the porosity you presented in chapter 3.1.5?

The values presented in 3.1.5 are within the range we tested (0.01 to 0.05). However, we did not use the porosity values in 3.1.5 because they relate to surface samples and are extremely variable (0.012 to 0.046). The aim of this calibration is to find optimal porosity valid for the overall borehole depth.

[P10 L234] than modelled
accepted, P11L264

[P10 L245] please rephrase

Sentence rephrased as following:

"For this reason, we compute 2D model for two location of special interest: the ERT profile and the Nasaasaaq summit.", P12L278-279

[P10 L245] observing permafrost evolution implies long-term measurement series, whereas model data are the data basis here. Please consider rephrasing

Sentence rephrased as following:

"The first location is chosen to compare the ERT data to our model, while the second location allows us to model and understand permafrost distribution and evolution in the tallest mountain in the study area.", P 12L279-281

[P10 L246] This sentence is hard to understand. Please rephrase and include information on what is meant by "profile z" and by "a solid"

Sentence rephrased as following - considering that "profile z", and "solid" are not necessary information -
:

"For each location we set-up a north-south transect in the QGIS software, and used it to sample the elevation profile from the DEM. The elevation profiles are then imported into COMSOL as 2D geometry using the parametric function option.", P10L246-247

[P11 L248] as above, what is meant by "the solid"?

Solid is the generic COMSOL name for 2D-3D geometries. We avoid using this term now, and use 2D geometry instead.

[P11 L249] Please note that formulas and symbols in the text are used consistently and according to TC's specifications.

Formula updated following the TC's specifications, P12L285

[P11 L251] from
accepted, P12L286

[P11 L254] The
Sentence deleted and calibration results included in the Methods, P5L111, P5L106

[P11 L253] I would suggest that the calibration results be placed within the methodology in Section 3.1.1, as it is not a central result of the study, but only the calibration of the instrumentation.

Calibration results included now in the Methods, P5L111, P5L106

[P11 L254] Please use terms consistently; iButtons or DS1922-L or a combination of the terms.
iButtons used now consistently through the text

[P11 L254] have on average an absolute temperature offset?
Sentence deleted as information is not considered relevant. We provide instead the maximum measured offsets.

[P11 L260] low or maybe only: Temperatures in both borholes ... close to 0°C

We have rephrased the whole paragraph:

"Boreholes temperatures are shown in Fig. 4. In SIS2019-02, the depth of zero annual amplitude is approximately 20 m. Below this depth, temperature data indicate a minimum of temperature of +0.3 °C, reached at 30 m depth, and a temperature of +1.0 °C at 100 m. Since temperatures are positive below the depth of zero annual amplitude, the measurements at SIS2019-02 indicate absence of permafrost. SIS2021-01 shows consistently negative temperatures between 20 and 70 m depth, reaching a minimum of -0.2 °C at 30 m depth. The depth of zero annual amplitude is approximately 10 m.b.g.s.. Since we measure negative temperatures below this depth, the data from SIS2021-01 indicate the presence of permafrost.", P12L301-306

[P11 L260] Information on measurement time points and measurement frequency should be mentioned in the methods chapter

Information now moved to the methods:

"Borehole SIS2019-02 does not have a permanent sensor installed, and it was logged manually three times since it was drilled.", P5L126-127

[P11 L261] Please show Data for both boreholes!

Data now included in Figure 4b, P14

[P11 L261] Based on these statements, it is impossible to assess whether this is truly an active layer, and whether permafrost is present at all in SIS2019-02.

Good point about the incorrect terminology, we cannot talk about active layer here. This is changed to "depth of zero annual amplitude", Also, see [M3]

[P11 L262] To be precise, data for a period of at least 2 years would have to be available in order to be able to speak of permafrost and an active layer at all. Please provide data!

See answer [M3]. Borehole data are now presented in figure 4b, P14.

[P11 L263] What is the minimum temperature at both boreholes?

The minimum temperature is in the text; +0.3 C for SIS2019-02 and -0.2 C for SIS2021-01. We now specify that these are minimum temperatures measured below the depth of zero annual amplitude - see answer P11L260

[P11 L264] Please provide data!

See answer P11L261

[P11 L259] 4.2.1? and Borehole Temperatures - just to be consistent with the following chapter?

Section merged with 4.3.1 in the new section 4.1, P12L289

[P11 L259] This chapter needs to be revised. Please present data and review the use of the terms Active Layer and Permafrost.

See answer [M6]. We now use the terms "frozen ground" and "depth of zero annual amplitude"

[P11 LNaN] 4.2.2?

See answer P11L259

[P11 L266] All loggers used recorded data for a full year, from fall 2020 to fall 2021.

Sentence rephrased as following:

"GST data are measured during one full year, as loggers were installed in September-October 2020, and retrieved one year later", P12L290

[P11 L266] Could you provide more precise dates than just from fall to fall?

Loggers were installed during September 2021 at across a period of 2 weeks. For each logger we waited exactly one year to retrieve the data. We consider reporting precise dates here too consuming. Precise dates can be found in the annex data.

[P11 L266] 28? As mentioned above?

Numbers corrected, and now reported as following:

"Fifteen loggers present now-free GST data (Fig. 3a), seven present thick snow cover and six present intermediate characteristics (Fig. 3b).", P12L291

[P11 L266] what are snow free data? Please rephrase.

GST data from a logger that is snow-free. Now rephrased as in answer P11L266

[P11 L267] the reference to figure 3 does not match the contents of the sentence. But in the following sentences a reference to figure 3 (a-d) would be helpful.

Figure 3 updated accordingly, P13. References to subfigures a-d are now included in the text, P12L290-300

[P11 L268] for the south facing rockwall?

Sentence rephrased as following:

"To show the effect of elevation, we compare two snow-free loggers installed on south facing rockwalls, one at sea level (MAGST = +3.5 °C) and at 460 m.a.s.l. (MAGST = +1.2 °C).", P12L297-298

[P11 L272] For downscaling the value of +1.58 °C was assigned to...

The sentence was rephrased as following:

"We used this value of +1.58 °C, comparable to previous findings in Greenland (Rasmussen et al., 2018), as constant offset when modeling snow cover.", P14L323

[P11 L270] delete

accepted, P13L320

[P11 L271] what conditions did change and how did they affect data? Are these effects taken into account later on?

This is an average effect, as now clarified in the methods:

"We first evaluate the average temperature offset due to snow cover by comparing the mean annual GST of sensors that were/were not snow covered during the entire winter 2020-2021.", P9L216-218

Topographical conditions are later taken into account using the SnowP method:

"This offset then is multiplied to the local probability of snow cover presence/absence, that we call SnowP, varying from zero (absence of snow cover) to 1 (presence of long lasting snow cover).", P9L218-219

[P11 L265] This chapter is primarily concerned with the correction of MAGST data as a function of snow cover. The presentation of the actual surface temperatures comes somewhat short. Furthermore, a distinction is made with respect to the snow cover thickness, but not with respect to the snow cover duration. An assumption would be that in areas with thick snow cover, it also takes longer to reach the aperture, so that a more differentiated approach to MAGST could be useful? If the temperature data are to be used exclusively as input for the model and not for an independent interpretation, the question would still be whether the paper should be fundamentally restructured.

Very good comment that points out how we need a proper introduction of the snow modeling issue (see [M1]). In particular:

- We have separated methods for the evaluation of snow cover influence on the GST time series from the GST model - see P9L215
- Yes, we do not differentiate snow cover duration under the assumption that snow cover thickness is a proxy for duration. We may use a sophisticated approach for evaluating the double effect of duration and depth, but we would not be able to extrapolate these properties at the landscape scale (and for longer time periods). The offset method is used as a proxy for both processes, providing a first order approximation of the effect of snow cover on the GST. We now specify that "The validity of this method is based on the observation that snow drift patterns in the arctic are generally stable over time due to relatively dry and windy weather (Parr et al., 2020).", P9L227-228. We evaluate the quality of this method by comparing data model at our target: temperatures below the depth of annual amplitude from boreholes with different snow conditions : "Here, the

model has a maximum error of 0.15 °C when compared to SIS2019-02, which is used only as validation dataset. SIS2019-02 has also different snow conditions than SIS2021-01, suggesting that our SnowP map and offset provide an acceptable boundary condition to model long term effect of recurrent snow cover induced by topographical patterns"., P19L396-P20L397

- We are not sure what is meant by “independent interpretation”; we assume this means “model validation”. If this is the case, we highlight how data are used for both training and validation. In particular, we use cross validation on the GST modeling, and borehole/ERT data for validation of deep ground temperatures. This is now more clear through the discussion (e.g., P17L380, P19L396, P20L399)

[P11 L275] reference to chapter 3.1.5 (methods)?

We need to reject this, otherwise for consistency we would have to reference every method section while presenting the relative results.

[P11 L273] This chapter could also be revised in some places and the data presented a little more comprehensively. Among others, figure 4c talks about potentially thawed and transition zone. Are these seasonal effects or long-term permafrost degradation? The question whether on this basis one should speak of permafrost or rather only of "frozen ground" would have to be answered.

We added the following sentence:

"This indicates that permafrost presence is restricted inside the mountain and close to the surface in the north face and the upper part of the south face.", P13L311-312.

See answer [M4] about seasonal effects on permafrost.

[P12 L276] skalierung der y-Achse könnte. scaling of the axes could be identical. What does the data look like for a logger with intermediate snow conditions? What is the elevation and exposition?

See answer [M9]

[P12 L276] could you color code the data points to relate to the snow conditions?

Figure 3 updated accordingly, P13

[P12 L276] please increase font size of the axis labels a little bit.

Figure 3 updated accordingly, P13

[P12 L276] please increase the font size of the axis labels

Figure 3 updated accordingly, P13

[P12 L276] the caption is difficult to follow. In particular, it is not clear to me whether Figure 4 a really refers to the measured field data or not the laboratory analyses. Please try to rephrase the text and make the connections a bit clearer.

Caption updated as following:

"Comparison between ERT and 2D thermal model . a) Petrophysical analysis, showing in-phase electrical conductivity data versus temperature for the three samples collected along the geophysical profile; b) profile of electrical conductivity/resistivity tomography (in Sm⁻¹ and kΩm) measured on the field; c) resulting 2D numerical model of the ridge where the ERT line was conducted", P15L332

[P12 L278] can you clarify this statement? Is this an interpretation of the data? How is this lithological fault characterized and how does it affect permafrost and subsurface conditions? Here, however, the question arises whether this should be the content of the results chapter.

This sentence has been moved to the discussion and developed as following:

"Most of the disagreement between model and data is due to the electrical conductivity anomaly on the north face of the geophysical profile. This anomaly occurs near a large lithological fault, visible in the field. Overall, the observations indicate that ground characteristics at this location are not isotropic and a direct comparison between model and geophysics is not meaningful.", P20L400-403

[P13 L281] dieses Kapitel besteht zu einem großen Teil aus methodischen Hintergründen, die so auch nicht vollständig verständlich im zugehörigen Methoden-Kapitel erklärt wurden. Warum gehen nur Logger ohne Schneebedeckung ein?

The reason for this choice is explained in the method section:

"Here, we use snow-free GST data, as snow cover effects will be modelled in the next step. Previous studies used an offset-based approach based on the evaluation of a constant thermal offset between air temperature and snow free GST (Magnin et al., 2017a; Etzelmüller et al., 2022).", P9L202-205

In particular, snow cover loggers are not significant developing this modeling approach as their data variability is mostly influenced by the snow cover property rather than the terrain predictors. This is common practice while using this methodology. For ex Magnin et al 2019 : "snow accumulation over the logger would make the recorded RST unsuitable for statistical modelling because its thermal effect is not linear and depends on a variety of parameters that are highly fluctuating in space and time in high-elevated and high-relief environments, such as the snow thickness, duration and time period of the accumulation, the sun exposure of the affected rock face, and the snowpack thermal properties"

[P13 L284] 5?

Now figure 6, P13L318

[P13 L288] The argumentation is a bit difficult to follow. It would be useful to make a clear reference to the methodology (3.2.4?) right at the beginning, so that it is clear what 0.03 and "optimal porosity" should mean. If possible, this chapter should be restructured and phrased more clearly.

We now introduce the results with the following sentence:

"The model was calibrated by optimizing two parameters: matrix porosity and initial offset value.", P14L324.

[P13 L293] please rephrase and make clearer statements. "a few degrees" should be made more precise. The use of the term "active layer" should also be reconsidered in this passage.

Sentence rephrased as following:

"When tested and compared to SIS2019-02 ($z = 55$ m ; PISR = 690 kWhm⁻², SnowP = 1), the model produces similar results (Fig.4b), 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.", P14L329-331

[P14 L295] Would it be possible to use different color scales for the subsurface temperatures and temperature differences? I think in general this sentence can be deleted.

We'd rather have one color scale as the white color placed at 0 C, has a good visual effect in both ground temperatures (easy to identify cold zones) and in model evaluation (easy to identify good model performance). We will delete this sentence

[P14 L296] The presentation of the results is a bit short. The first 4 lines describe methodological aspects. In the second part, the basics of snow are given and already interpreted.

We extended the methodological description and avoided repetition here. Here we extended the results presentation including a description of the influence of snow cover:

"Snow cover plays an important role, as snow covered areas can increase of 250 m the elevation of the MGT20 0 °C isotherm. This effect is prominent on mountain flanks characterized by sequences of ridges and 340 chutes (Fig.7b), as the chutes are warmer than the ridges due to their predisposition to accumulate snow.", P16L338-340

[P14 L299] present?

Sentence now removed - see answer P14L296

[P14 L300] Figure 7

accepted, P15L334, P15L335, P16L340

[P14L301] here results and interpretation are mixed. Furthermore, statements contradict each other. First of all, one should not necessarily speak of continuous permafrost in the mountains, since topography plays a significant role in permafrost distribution.

Permafrost can be continuous in mountains. This starts at an elevation when permafrost is found also in the most unfavorable topographical conditions – e.g. south facing slopes, unfavorable snow accumulation. However, we agree that this may cause confusion since we are in arctic area, and will avoid this term. We now instead describe elevation limits of MGT20 relative to aspect and snow conditions. (P15L336)

[P14L301] Second, what is meant by "positive MGT20 (...) at which permafrost is continuous"? If permafrost exists, temperatures cannot be positive, can they?

Yes, this sentence is very confusing. Thank you for pointing it out. It is now changed to:

"Snow cover plays an important role, as snow covered areas can increase of 250 m the elevation of the MGT20 0 °C isotherm.", P16L338-339

[P14L301] Furthermore, the importance of snow cover is again emphasized here. Maybe it would be useful to discuss the snow cover and its effects on permafrost in the introduction and to give references?

We have now dedicated proper space to the snow cover issue in the introduction, P2L45-54

[P14 L301] The evidence here is not quite clear, in areas with thick snow cover permafrost does not exist? The relationship between snowpack and permafrost is highly dependent on thickness, timing of snowing in and melting out. This should be made clearer (but not in the results section).

No, our data indicate that, on average, snow has a warming effect on the ground. When we apply this effect to our model, permafrost is found at higher elevation when snow is present when other conditions (elevation and aspect) are equal. This is clarified by:

"On average, the presence of snow cover causes an offset on the MAGST of $+1.58 \text{ °C} \pm 0.41 \text{ °C}$ when other conditions do not change ($R^2 = 0.81$).", P13L320-P14L321

Yes, we agree that the relationship between snowpack and ground temperatures is complex, but we do not aim to model these interactions. As specified previously, we use a simple offset method based on empirical data to evaluate the average effects of snow cover on deep ground temperatures. This is now clarified already in the introduction, P2L45-54

[P14 L303] Sentence does not seem complete to me

Sentence rephrased as following:

"The colder MGT20 occurs on the north faces of the Nasaasaaq peak (763 m.a.s.l.), reaching -4.0 °C.", P16L337-338

[P14 L305] this headline is difficult to understand. Please consider rephrasing

Furthermore, the presentation of the results is short and there is extensive interpretation.

Headline rephrased as following:

"Comparison between 2D model and ERT profile", P16L341

We now clarify that we are describing model results rather than interpreting ERT data:

"The model indicates, as of October 2020, the presence of negative temperatures below the depth of annual amplitude on the Nattoralinnguaq summit, suggesting the presence of permafrost.", P16L347-348

We removed interpretation from the section and moved it to the discussion:

"Most of the disagreement between model and data is due to the electrical conductivity anomaly on the north face of the geophysical profile. This anomaly occurs near a large lithological fault, visible in the field. Overall, the observations indicate that ground characteristics at this location are not isotropic and a direct comparison between model and geophysics is not meaningful.", P20L400-403

[P14 L306] Please review the use of the term "permafrost" in this context. Possibly simply speak of frozen ground?

Sentence rephrased as following:

"The model indicates, as of October 2020, the presence of negative temperatures below the depth of annual amplitude on the Nattoralinnguaq summit, suggesting the presence of permafrost.", P16L342-344

For more detail, see answer [M3]

[P14 L306] Is there an ERT2? Can you refer to data/images early on?

Sentence rephrased as following:

"In Fig.5c is presented the 2D model simulation at the geophysical profile location.", P16L342

[P14 L308] this is speculative and interpreted. Maybe something like "the model indicates temperatures are positive down to depths of 20-40m"?

This is a pure description of the model results. We further clarify this aspect now, see answer P14L305

[P14 L308] Simply frozen? Or is there any information that temperatures have been below 0 °C for more than 2 years?

See figure 9: the model shows negative temperatures since 1979.

[P14 L308] What figure do you refer to here?

Proper reference is Figure 5b. Now corrected, P16L345

[P14 L309] frozen ground?

Here we talk about permafrost as we are evaluating deep ground temperatures. See [M3]

[P14 L310] interpretation

Sentence rephrased as following:

"Both datasets indicate a mostly unfrozen south face, and a colder north face. However, the ERT data indicate a large unfrozen section at the extremity of the north face.", P16L347-348

[P15 L316] 21st?

accepted, P17L352

[P15 L316] in the previous sentence you write, that permafrost will disappear by the end of the century.

Sentence rephrased as following:

"For scenario RCP 2.6, the ground seems in phase transition by 2100, being at 0.05 – 0.1 °C between 50 and 70 meters depth.", P17L352-353

[P15 L318] RCP 2.6

accepted, P17L354

[P15 L318] RCP 8.5

accepted, P17L354

[P15 L323] permanently frozen?

Please reconsider using "permanently frozen" instead of "permafrozen" throughout the text. To my knowledge, the term permafrozen is not clearly defined and is not commonly used.

accepted, P17L359

[P15 L327] please reconsider the use of sporadic and continuous for mountain permafrost.

We now use elevation/aspect to describe permafrost limits:

"For Nattoralinnguaq, the optimistic scenario (RCP 2.6) suggests an increase of ground temperatures of about 1 °C, causing permafrost retreat on the north face up to 200 m.a.s.l.. Scenario RCP 8.5 delineates a situation where permafrost is relict, i.e. below the reach of seasonal frost (Magnin et al., 2017a), at approximately 100 m depth on the north face. The model produce similar results for Nasaasaaq, as for scenario RCP 2.6 we observe permafrost retreat to 300 m.a.s.l. on the north face and to 500 m.a.s.l. on the south face. Scenario RCP 8.5 indicates that all permafrost on the mountain is relict, except for the summit's north face.", P17L364-369

[P15 L330] what do you mean by "relict permafrost" here?

Below the reach of seasonal frost. This term is used by Magnin et al, 2017. This is now specified in the text, P17L366

[P16 L331] Please increase the font size of the axis label in Figure 8a. If possible, use comparable font sizes within the figures.

Accepted, updated figure 8, P18

[P16 L334] difficult to understand, please consider rephrasing.

Sentence deleted, as this information is already conveyed through the methods

[P17 L339] You are citing the discussion paper from 2020 instead of the final version of 2021!
accepted, P17L376, P26L599

[P17 L341] CryoGrid 3
accepted, P17L378

[P17L344] Here I agree completely. Even though the content of the Conclusions should be clarified, to what extent the results based on this short series of measurements are accurate.

See answer [M4]. We now make clear that:

"On the other hand, our model is more reliable when describing ground temperatures below the depth of zero annual amplitude. Here, the model has a maximum error of 0.15 °C when compared to SIS2019-02, which is used only as validation dataset. SIS2019-02 has also different snow conditions than SIS2021-01, suggesting that our SnowP map and offset provide an acceptable boundary condition to model long term effect of recurrent snow cover induced by topographical patterns. The general agreement between model and geophysical data also indicates that the model is suitable for describing permafrost extents in a wide range of elevations, slopes and aspects.", P19L395-P20L400

[P18 L346] Sentence seems to be incomplete. What do you mean by this? Please consider rephrasing.
This sentence has been deleted, as not carrying relevant information for the discussion.

[P18 L353] Please rephrase. Far as I can tell, snow does not actively cool the ground, but affects the thermal regime, which can result in a cooling. Also, instead of "early heatwave" heatwaves early in the year?

This discussion on snow effects is too complex for the scope of the study and we decided to remove it. We now present the issue of snow cover and state of the arte of modeling in the introduction, P2L45-54

[P18 L348] This information could also be part of the introduction. Please formulate in such a way that a discussion of the results is made apparent.
Agreed. See answer P18L353

[P18 L359] How can you tell the boundary conditions are acceptable? Could you present data on this?

The sentence has been rephrased as following:

"Here, the model has a maximum error of 0.15 °C when compared to SIS2019-02, which is used only as validation dataset. SIS2019-02 has also different snow conditions than SIS2021-01, suggesting that our SnowP map and offset provide an acceptable boundary condition to model long term effect of recurrent snow cover induced by topographical patterns.", P19L396-P20L398

The data of SIS2019-02 are now presented in figure 4b, P14

[P18 L362] This section also sounds like it belongs to the introduction rather than to the discussion.

This is now shortened and integrated in the introduction:

"Although some numerical models are able to describe snow physics at hectometric resolution (Gisnås et al., 2014), it becomes extremely challenging to achieve a good knowledge of spatial characteristics of the snow in complex terrain, given the spatial variability of weather forcing, as wind and shading.", P2L48-51

[P19 L400] who use? / Where a similar approach is used?

accepted, P28L385

[P19 L402] please provide statistics.

"Significantly" has been removed now

[P19 L402] (xy °C at 10 m depth) - please add information about the average differences of your model

This is already presented in the results, P14L327-331

[P19 L404] no data on the quality of the climate data has been provided up to this point.

It is not our aim to provide an evaluation of these data, which can be found in the respective publications. In this study we observe how these data perform within our database and methodology. Here we mean to say that using reanalysis data may have an effect to the result quality near surface, which is lower than previous studies using weather station data.

[P19 L405] this is very fundamental criticism of the entire experimental setup, which is also completely justified. What is the justification that the results are nevertheless relevant and reliable.

See [M4]

[P20 L414] Here I find it difficult to follow the argumentation. Above, you criticize both the duration of the measurement series and the positioning of the boreholes. The boreholes from the shallow foreland were used to model frozen subsurface in the mountains and then you state that the results are satisfactory because the modeled temperatures could be validated with those of the neighboring borehole. Please consider optimizing the evidence a bit. I think the approach in the next sentence to include the ERT results is purposeful here.

See [M4]. Sentence rephrased as following:

"All this considered, the results indicate that the modeling approach is suitable for evaluating of ground temperatures below the depth of zero annual amplitude in complex terrain.", P20L403-404

[P20 L416] As noted above, I think the comments on the ERT results and the petrophysical data are a bit too brief. To lead this discussion, a somewhat more comprehensive data interpretation would be interesting.

The sentence was rephrased as following:

"Most of the disagreement between model and data is due to the electrical conductivity anomaly on the north face of the geophysical profile. This anomaly occurs near a large lithological fault, visible in the field. Overall, the observations indicate that ground characteristics at this location are not isotropic and a direct comparison between model and geophysics is not meaningful.", P20L410-403

[P20 L420] is it about permafrost characteristics or permafrost distribution?

It is about permafrost distribution. The sentence was rephrased as following:

"We can use our results to compare the distribution of bedrock permafrost in the Sisimiut area to other mountain ranges, as presented in Fig.10.", P20L408

[P20 L421] contents of the next chapter

Sentence rephrased accordingly:

"We can use our results to compare the distribution of bedrock permafrost in the Sisimiut area to other mountain ranges, as presented in Fig.10.", P20L408-409

[P20 L423] could you provide reference to the associated chapter or other sources? In Chapter 4.3.1 you write: "Aspect causes a MAGST offset of 2.2 °C " and "When snow covers the loggers, these quantifications do not hold" How are these statements as well as the discussion here related?

Yes thank you for noticing, this is confusing. The 2.2 C refers to actual measurements of two specific loggers, while the 2.4 C is an average value produced by the model. We rephrase to avoid confusion: "In Sisimiut, the solar radiation creates an average offset of 2.4 °C from north to south facing slopes, causing a rise of about 400 m of elevation in the permafrost 0 °C isotherm between this two aspects.", P20L410-412

[P20 L432] The content of this chapter relates primarily to the relationship between solar irradiance and permafrost distribution, rather than permafrost properties. In addition, the influence of the snow cover could also be included in the discussion.

Chapter renamed accordingly "Local permafrost distribution compared to other regions", P20L407. As specified in the introduction, previous studies mostly disregard snow influence by filtering steep terrain as valid for the model. Therefore, direct comparison here is difficult.

[P20 L438] (...) outlast the 21st century?

accepted, P20L425

[P21 L447] This section focuses heavily on the relationship between permafrost degradation and rockfall activity, a topic that is very present in the introduction. For this, the statements here are very vague and in part simply the state of research is presented. In order to remain consistent with the introduction, it would be nice to make more concrete statements here. Does permafrost degradation only affect rockfall activity? Does the potentially longer lasting low-lying, then relict permafrost affect mass movements?

See [M1]. This paragraph is now only briefly touched in the conclusion: "Although the correlation between permafrost degradation and rockfall activity is accepted within the scientific community (Ravel and Deline, 2011; Patton et al., 2019), the process chain linking the two phenomena is very complex. Therefore, future efforts in the area should focus on investigating slope stability characteristics, and their relation to permafrost distribution and degradation. Once (and if) problematic slopes are identified, site specific models integrating high resolution snow distribution (Haberkorn et al., 2016) and crack networks (Magnin et al., 2020) will provide a more detailed understanding of slope thermodynamics, overcoming the main uncertainties of our model.", P21L443-447

[P21 L461] until the end of the century?

accepted, P21L439-440

[P21 L462] ..that only relict permafrost will persist at a greater depth (quantification?)...

Sentence rephrased accordingly:

"For scenario RCP 8.5, i.e. with no mitigation on carbon emissions, our model predicts a reduction of 95% of the active permafrost area at the end of the century, meaning that permanently frozen ground will persist only as relict condition below 10-20 m depth, i.e. below the reach of the seasonal frost.", P21L438-441

[P21 L463] suggestst

accepted, P21L441

[P21 L464] permafrost distribution and degradation?

[accepted, P21L445](#)

[P21 L467] In my opinion, the preceding text does not really lead to this conclusion. The majority of the conclusions consists of summary and outlook. The circle to mass movements as a consequence of permafrost degradation is closed, but less based on the research results than on general knowledge. For me, a clearer derivation to this final conclusion would be desirable. Is there a rationale that your simple approach is really a good trade-off? Especially considering your own criticisms of the data basis, it would be interesting to derive the conclusion in a slightly more complex way and show that despite the criticisms, the approach has advantages over even simpler approaches, but comes close to the informativeness of more complex projects.

Yes good point, this is linked to [M4]. The main conclusion here is that we are satisfied with the model performance due to its ability to reproduce ground characteristics below the depth of zero annual amplitude. This is useful for permafrost zonation in a first assessment. This is now specified by:

"In this sense, our modeling approach based on weather parameters readily available for the whole region, downscalable with a simple topographical approach, provides a good first assessment for mountain permafrost zonation.", P22L448-450

[P21 L471] The link does not work yet.

The link is activated after paper acceptance