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:

- In this document the reviewer can find:
  - 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 annex responses when relevant.
  - Long responses to some of the major issues proposed in the annotated annex. These answers are referenced by the respective annotation by numbers in square brackets, e.g. [1]
  - New figures and tables, when major changes are requested by the reviewer
- In the annotated annex the reviewer can find:
  - Response to the annotation. The annotations' color code has been changed to plain green, as we used it to mark annotations we responded to
  - When necessary, the annotation refer to answers presented in this document, as described above.

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 will help 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 will correct 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 is a lot of confusion in the text at the moment, we apologize about that, and thanks to reviewer for marking in the text all these mistakes. We agree that there is a number of places in the text that would benefit from more precise referencing to the figures (a,b,c…); we will improve referencing.

- 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.
- Yes, we agree that this aspect will improve the text and we will implement the reviewer's suggestions.

Structural Aspects

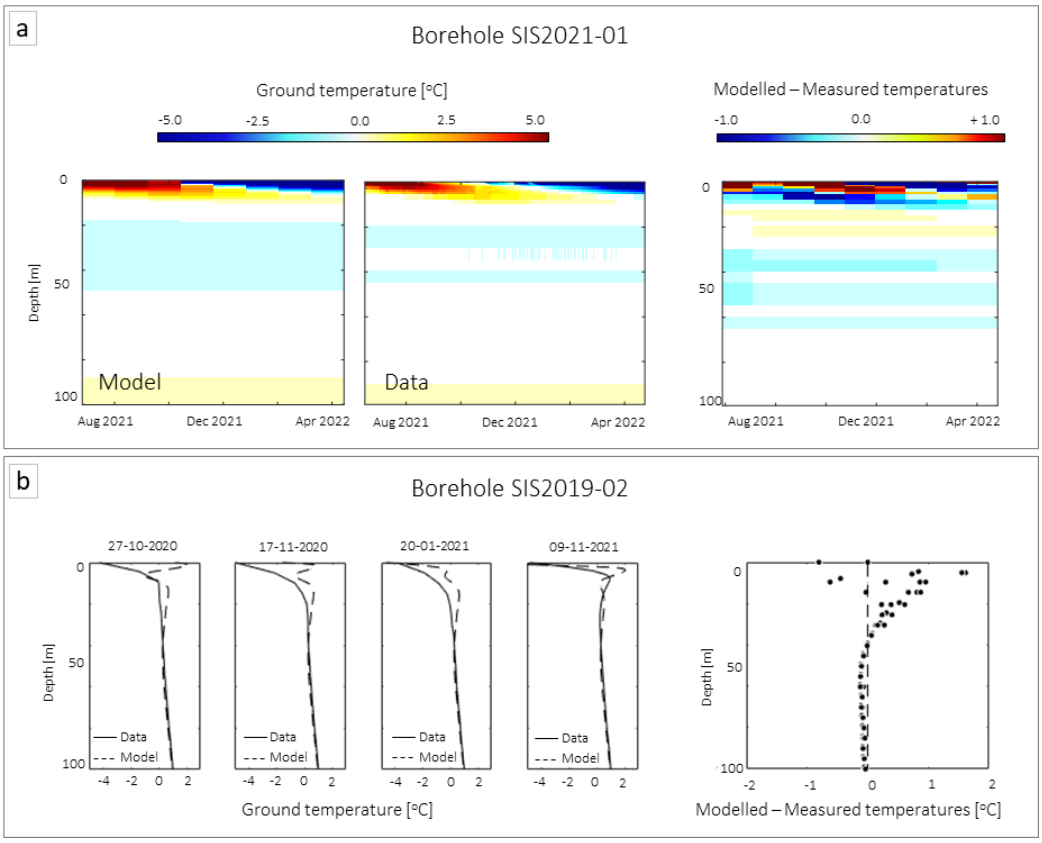
- [M1] Generally speaking, the manuscript could be structured more stringently. It should be examined whether one could separate more clearly between methodological background, results and the discussion or merge chapters. For example, climate data, temperature measurements, and geophysics provide a 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 should be re-structured. 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 loosing the main point of the manuscript fade into details.
  Concerning the restructuring, we do not believe that merging results and discussion section would help us to achieve a clearer text. This is because a large share of the discussion compares results from different methodologies.

- 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 will adapt the chapter naming to match the actual content.

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 should appear earlier in the text, before figure 3. 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 will add 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 "Permafrozen".
- [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 will improve the text in this sense.
  We agree that "Permanently frozen" is better than "Permafrozen", and will change 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 will develop 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. 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.
- 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.

This is not well developed in the original text and we agree that we have to put more emphasis on these points while producing the new version of the manuscript.

[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.

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.

All these points are rather foggy through the text, and we understand the reviewer's concerns. We are going to define more precisely the two different models (GST model and heat transfer model) already in the method section, and refer properly to them through the text. We are also going to precisely define the improvements of our proposed modeling approach compared to previous models in the region.

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, and we are glad that, thanks to this work, we will be able to improve our manuscript.

Comments in the annex

[1] 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.

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.

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 agree to restructure the introduction and discussion in the revised version of the text.

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. In this sense, we will specify in the text how this study only gives an outlook on this area.

[2] L 31: 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, the difference is that we have relatively large amount of data for a small area. In particular:

- Network of GST measurements that represent the mountain terrain in its complexity
- High elevation ERT profile that gives an idea of deep temperature distribution in 2D

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. The idea we would like to convoy is that this additional source of data, plus the fact that we use a known methodology which has been validated by other studies, give more confidence to the results. We believe that our critic on the amount of borehole data is not followed by an in-depth constructive description of the strength of this study. We will modify the text accordingly.

[3] 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.

Yes good points. So, we will add the suggested information that we obtain by treating the data in Cappelen et al, 2021. In particular:

- Mean annual air temperature increased from -3.51 ^{o}C in 1961-1981 to -1.81 ^{o}C in 2000-2020
- Mean annual precipitation decreased from 509 mm in 1961-1981 to 422 mm in 1984-2004. The period mismatch is due to the removal of the rain gauge by the weather station in 2004
- Mean monthly precipitation in January-April decreased from 28 mm in 1961-1981 to 25 mm in 1984-2004.
- Mean monthly precipitation in June-September decreased from 58 mm in 1961-1981 to 49 mm in 1984-2004.

[4] L96, Table 1: 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 will be modified to:

	Nb	Brand	Type	Sensor	Resolution [C]	Accuracy [C]	Logging interval [hr]	Sensor(s) depth [m]	Terrain
Ground Surface Temperature	28	Maxim integrated	iButton	DS1922L	0.063	0.5	4	0.05	Soil, Bedrock, Steep Bedrock
	5	Geoprecision	MLog5W-Rock	PT1000	0.001	0.1	1	0.3	Steep bedrock
	4	Geoprecision	MLog5W-STRING	Tnode	0.01	0.1	1	0.3, 0.9	Steep bedrock
Deep boreholes	1	Geoprecision	MLog5W-STRING	Tnode	0.01	0.1	1	[0.1,0.5,1.0,1.5,2.0,3.0,4.0,5.0,7.5,10.0,12.5,15.0, 17.5,20.0,25.0,30.0,35.0,40.0,45.0,50.0,55.0,60.0, 65.0,70.0,75.0,80.0,90.0,99.0]	SIS2021-01
	1	HOBO	5-inch Probe	U12-015-02	0.03	0.25	0.00028	[1,5,10,15,20,25,30,35,40,45,50,55,60,65,70,75,80 ,85,90,95,97.5]	SIS2019-02

[5] L149, Table 2: 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.

Thank you for the suggestions, we agree that the table can be improved. Following the comments, we modified the table as follows:

Dataset reference	Text Reference	Period Available	Period used	Variables	Data type	Location
From datasets f* and c	a	1784-2021	1850-1958	Air tempertaure	Interpolation from datasets f1 and f2	Sisimiut
				Solar radiation	Extrapolated from dataset c	
Cappelen et 2021b	b	1958-2021	1958-1979	Air temperature	Weather station	Sisimiut
				Solar radiation	Extrapolated from dataset c	
Herbasch et al 2019	c	1979-present	1979-2022	Air temperature, solar radiation, cloud cover, dew point, wind speed and direction, total precipitation	Reanalysis	Global, Gridded 0.5 degs
Hofer et al 2020, RCP2.6	d	2006-2100	2022-2100	Air temperature, solar radiation	Model	Global, Gridded 0.5 degs
Hofer et al 2020, RCP8.5	e	2006-2100	2022-2100	Air temperature, solar radiation	Model	Global, Gridded 0.5 degs
*Used to generate air temperature of dataset a						
Cappelen et 2021a	f1	1784-2021		Air temperature	Weather station	Nuuk
Cappelen et 2021a	f2	1784-2021		Air temperature	Weather station	Ilulissat

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.

[6] Points addressed here refer to the picture shown in [M2]

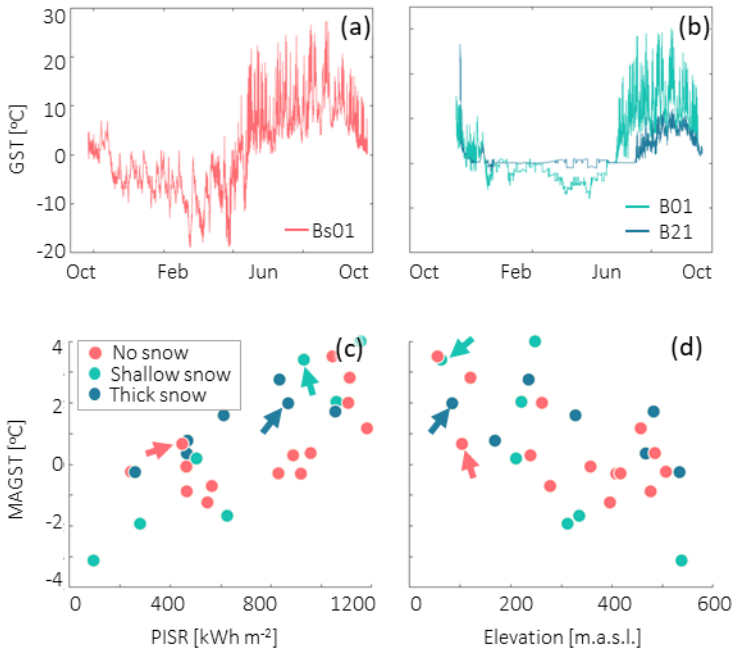
[7] This chapter is primarily concerned with the correction of MAGST data as a function of snow cover. The presentation of the actual surface temperatures comes somewhat short. Furthermore, a distinction is made with respect to the snow cover thickness, but not with respect to the snow cover duration. 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 will need to separate GST results and evaluation of snow cover influence on the GST time series. We will do this already in the method section, and successively in the results.
- 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 evaluate the quality of this method by comparing data model at our target: temperatures below the depth of annual amplitude.
- We are not sure what is meant by “independent interpretation”; we assume this means “model validation”. If this is the case, we agree with the reviewer, as pointed out in all comments regarding paper structure. We will improve our article structure (introduction, methods, and discussion) to highlight how data are used for both training and validation. In particular, we use cross validation on the GST modeling. We also compare numerical model results to deep ground temperatures and ERT transect.

[8] 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 will instead describe as “permafrost can be found in all aspects”.
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, borderline embarrassing. Thank you for pointing it out. It will be changed to “Snow cover can rise the lower limit of negative MGT20 from 200 to 450 m.a.s.l. on south facing slopes”.
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?
Yes, as discussed in previous answers, we will dedicate proper space to the snow cover issue in the introduction.
The evidence here is not quite clear, in areas with thick snow cover permafrost does not exist?
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. We will clarify this.
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).
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

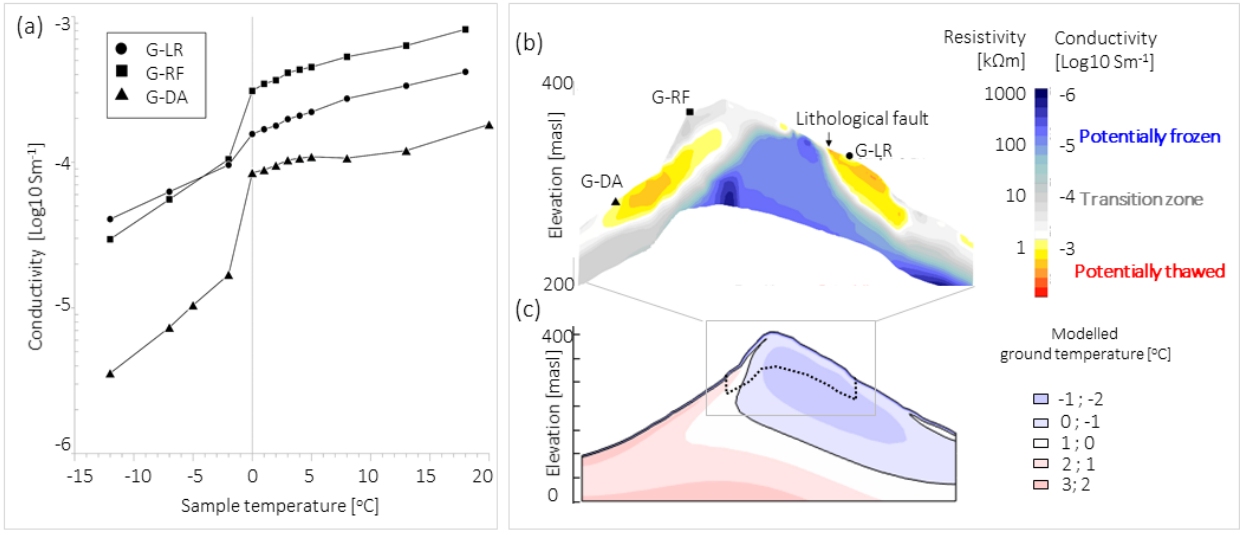
the average effects of snow cover on deep ground temperatures. This is going to be made clearer in the introduction, as suggested by the reviewer.

[9] New figure 3:



Caption: Summary of GST recorded by the loggers during 2020-2021. On top, examples of GST time series are for loggers not covered by snow (a) and covered by thick and shallow snow cover (b). On bottom, the MAGST in relation to topographical predictors Elevation (c) and Potential Incoming Solar Radiation (d). The data points are colored depending on local snow cover conditions. The arrows highlight the loggers presented in (a) and (b).

[10] New figure 4:



[11] 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°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°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.