In their revised manuscript "Modelling present and future bedrock permafrost distribution in the Sisimiut mountain area, West Greenland", originally "Characteristics and evolution of bedrock permafrost in the Sisimiut mountain area, West Greenland", the authors use a minimalist approach to modelling spatial permafrost distribution and its future evolution. In my opinion, the revision of the manuscript has massively improved its quality, although some points still remain open. Since the changes were very extensive, a few more questions arose during the re-review of the manuscript, which are addressed in the following.

Formal Apects:

The reviewers' points of criticism regarding formal aspects have been incorporated very well for the most part. Nevertheless, there are some sentences, especially in the newly written sections, whose wording should be revised. I have made some suggestions here, but would recommend that the manuscript be critically proofread once again.

The illustrations have also been significantly improved. A few suggestions, or points of criticism, are given below in the line-by-line comments.

General Comments and Fundamental Aspects:

In the first review, I raised a few points that I consider fundamental, which were taken up by the authors but were not clarified in a way that was completely comprehensible to me. This concerns the discussion conducted under [M4], where I would like to take up a few points again.

[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.*

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

(You can also find some comments on this point in the Line-by-Line Comments).

- *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).*

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

- *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).*

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

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

Line-by-Line Comments

P1/L4 (…) towards the characterisation of bedrock permafrost (…)

As you changed the title with regard to the characterisation of permafrost, does is still make sense to focus on characterisation here?

P1/L12-13

Large parts of the abstract focus on background and methods. Only the last sentence is on results. Maybe you could give a little more information on results and maybe the fundamental conclusions?

P1/L20-21 (…) and debris permafrost creep rates increase

Maybe change to: (…) and an increase in permafrost creep rates?

P2/L24-26 Sentence

Suggestion: For Greenland, a precise quantification of mountain permafrost distribution is still not available. Models are based on numerical simulations at kilometer scale (Daanen et al., 2011), are not calibrated with in-situ data (Gruber, 2012), or are valid for sedimentary terrain only (Obu et al., 2019).

P2/L28-31 This knowledge gap challenges our understanding (…)

This sentence focuses on the same point as main parts of the first paragraph in the introduction. Maybe add the information and references dealing with hazard assessment and affected population to the first paragraph to avoid redundancies?

P2/L32 “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).”

Sentence is a little difficult. Suggestion: The fact that ground temperature data in Greenland are limited to a few low-land sedimentary boreholes that are not representative for high mountain bedrock permafrost in complex terrain, is a major challenge for modelling mountain permafrost in this region.

P2/L43-44 “This approach (…)”

Sentence; suggestion: This approach has the advantage that good results are obtained while only basic climate data are needed, i.e. air temperature and solar radiation

P2/L45-54 Paragraph on the influence of snow on permafrost

This paragraph focuses mostly on the spatial distribution of snow and its influence on the ground thermal regime. Anyway, the temporal aspect also has a great effect, with the timing of the onset of snow cover and snowmelt. Can this somehow be included in the model or would it be helpful to add some information here?

P2/L55 (...) aim of this study is to move a first step towards a high resolution regional characterization of mountain permafrost in Greenland

As already asked above, the characterization of permafrost aspect has been removed from the title, as it is not the main focus of the manuscript. I still think it would be nice to go a little bit more into detail regarding the aims of this study, especially with regard to the complex approaches you mention in the following sentences.

P2/L56-58 “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.”

To me this sounds a little bit confusing. Were the data available for use or did you install the loggers for the project? Maybe you could rephrase and make clear what data were available and what has been installed?

P3/L81 “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)”

Two sentences? I do not see why the “and” should connect the two parts of the sentence?

P3/L78-80 and P5/L117 “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.” and “such as flat bedrock (6), soil (11) and easy-access rockwalls (2)”

In the chapter 3.1 you mention that iButtons were installed in soil. Anyhow, you did not mention the soil in the site description. As the thermal regime of soil most probably differs significantly from the

other substrates it would be nice to have some information on the soils, maybe thickness and distribution? Or do you consider it as insignificant?

P3/L89-90 “The recent climatic change on the other hand is believed to have caused significant glacial retreat in the coastal glaciers in the area, which lost about a fourth of their volume in the past three decades (Marcer et al., 2017).”

Maybe you could rephrase the sentence, as there is no “on the one hand” in the paragraph it does not make sense to introduce the other hand. Suggestion: “Within the last three decades coastal glaciers in this area experienced a loss of about a fourth of their volume”?

P4/L115 “These loggers were placed by drilling a 10 x 300 mm hole and sealing (…)”

I suppose you drilled holes for each logger, maybe you could rephrase?

P5/L114-119 Description of logger positions

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

P5/L120 “Two 100 m deep boreholes, SIS2019-02 and SIS2021-01, are drilled in bedrock outcrops in relatively flat terrain at 50 and 70 m.a.s.l. (see Fig.1) and similar conditions of exposure to solar radiation, while snow conditions are different.

Maybe two sentences? Suggestion: Two 100 m deep boreholes, SIS2019-02 and SIS2021-01, are drilled in bedrock outcrops in relatively flat terrain at 50 and 70 m.a.s.l. (see Fig.1). While conditions are similar with regard to exposure (exposition?) and solar radiation, snow conditions are different.

P5/L127 “(…) and it was logged manually three times since it was drilled.”

Could you be a little bit more specific here? Does this mean these three datasets are part of your study? Is there a general measurement interval for the borehole? Furthermore, in Figure 4 you show 4 datasets measured at that site. Could you give a little background on this?

P6/L136-137 Could you provide a cross-reference to Figure 1, showing the location of the ERT-profile?

P6 and 7/L135-154 Structure of the paragraphs

I think it was really helpful that you added some background information on ERT in this chapter. However, to me, the structure is a little confusing. You start with the setup of the measurement, followed by basics on ERT, back to the field-setup followed by data processing. Could you maybe rearrange this in a more straightforward way?

P6/L141 “(…) yields only qualitative information on the thermal state of materials (…)”

The wording here could be somewhat misleading and imply that one can directly derive information on thermal conditions from ERT. Even if only qualitatively. Here I would find it helpful to formulate the whole thing a little more precisely. Perhaps a clear introduction to the chapter would be helpful, in which it is made clear at the beginning that the information on the thermal status is derived from the ERT measurement in combination with the laboratory calibration.

P7/L157 “The three granite cubic core samples (…)

In chapter 2 you write, that the bedrock consists of amphibolitic gneiss. Is this information correct? If so, could you explain why you used granite for the calibration? And if there is granite and gneiss in the study area, could you take this into account in the site description?

P7/L158-220 In this paragraph values were changed compared to the first version of the manuscript. The changes are tracked, but – far as I can see – without explanation. Changes include porosity, conductivity and information on TF and downward/upward. Could you please explain the reasons for the changes and how they affect the data?

P7/L169 "(..) and freeze-thaw conditions of the ERT transect."

Freeze-thaw indicates a process, I think something like frozen/unfrozen might fit better in this context?

P8/L190 "(…) 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)"

Repetition of "good performance". Please rephrase

P9/L205 "In our study, we use a conceptually identical approach, based on the following hypothesis (Magnin et al., 2019): the snow-free GST can be predicted by an empirical model trained using available forcing variables that dominate GST distribution on steep bedrock."

I do not fully understand the citation within this sentence. Does it mean the Hypothesis is to be found in Magnin et al. 2019? If so, maybe rephrase to: …based on the hypothesis given in Magnin et al. 2019, stating that…?

P9/L215-228 - Chapter Snow Cover Modeling

If I understand correctly, the chapter focuses more on the distribution of snow cover, rather than the snow cover in general? Maybe you could rename the chapter?

Furthermore, I must admit, that the chapter was a little hard for me to follow, as you tell the story backwards. You start with temperature offset you calculated that you multiply with SnowP, then explain what SnowP is and what it is based on, then how the SAGA dataset was created and so on. Even though I do not think this approach is wrong, maybe you could think about changing this paragraph and turn the line of argument around?

P10/L235-237 Here you argue, that varying the rock-type parameters results in temperature differences smaller than 0.1 °C. Could you add some information on the values you tested to get a little more context on the errors?

P10/L241 2D "…models, as it not known…"

here is an "is" missing, please change to "…model, as it is not known…"

P11/L250 Could you change 0.015°C to K?

P12/L279 "…we compute 2D model for two location…"

please change to: "…we compute 2D models for two locations"

P12/L281 Please provide reference for QGIS

P12/L281 "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"

Suggestion: Elevation data along transects from north to south were extracted from the DEM using QGIS (Quantum GIS)

Chapter 4.1 P12/L290-306 In my opinion, the language of the largely newly written chapter 4.1 should be improved. A few suggestions or specific points:

> P12/L290 “GST data are measured during one full year, as loggers were installed in September-October 2020, and retrieved one year later.”
>
> Maybe change to something like: GST data are measured between September 2020 and 2021? Did you retrieve the loggers or do they still measure?
>
> P12/L291-292 “Fifteen loggers present snow-free GST data (Fig. 3a), seven present thick snow cover and six present intermediate characteristics”
>
> Repetition of “prevent” maybe change to: Snow-free conditions prevailed at 15 logger sites, a thick snow cover was observed at 3 sites, 6 sites were continuously snow-free
>
> P12/293 “in general, snow cover onsets in early November and lasts until mid-June, although this depends on the specific logger location”
>
> “in general, there is a continuous snow cover from early November to mid-June, with topographical differences.”
>
> Please also check the rest of the chapter.

P13/L308 "As shown in Fig.5b, the electrical conductivity tomograms ~~acquired~~ show ~~a~~ vertical and also lateral variations distribution of the conductivities with low conductivity values (< 10−3.5 Sm−1) below the north and south face and high values inside the mountain (> 10−4.4 Sm−1).”

Please rephrase. Difficult sentence and repetition of conductivity. Where is the difference between “values inside the mountain” and “below north and south face”? Also inside the mountain? Maybe you could provide a more precise description of the area you discuss?

P13/L310 “…from frozen to thawed conditions…”

Thawed implies, that the subsurface has been frozen. Maybe write “from frozen to unfrozen conditions”? as there is no data on the temporal evolution?

Chapter 4.2/Figure 5b It would be helpful if you could add an indication in Figure 5b on where North and South is. I have a question about the lithological fault shown in Figure 5a. Firstly, could you provide additional information on this in the text? Secondly, this lithological fault is located directly in the border area between relatively low and very high resistivities. I could imagine that in the vicinity of fault zones, the rock may be more weathered or fractured, which could potentially have an influence on the resistivity distribution. Partly you discuss this a little in Chapter 5.1 (L401-404) coming to the conclusion, that “a direct comparison between model and geophysics is not meaningful” I think this might need a little more background information.

Figure 4b – could you add a vertical line at 0°C as you did in the modelled-measured graph?? this would make it easier to see when the temperature falls below 0°C and where the zero amplitude is.

Figure 5 b/c – could you add distance values on the x-axis? Could you add a clear Elevation axis in Fig. 5b?

4.3.2 Heat Transfer Model & Figure 4 & Discussion

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

P15/L333 "The permafrost map is represented by the MGT20, i.e. the average temperature at 20 m depth (below the dpeth of zero annual amplitude) during the period 2012-2022 (Fig.7a and b)

Could you please rephrase this sentence? Is it correct, that you defined the MGT20 as permafrost?

Figure 7 "(outer radius is sea level, increasing to 800 masl at the center. "

")" is missing

Is there an exaggeration in the 3D model of Figure 7b? If so, could you add the info?

Chapter 4.4.1 Comparison between 2D model and ERT profile

As you compare and discuss results, would it make sense to make this part of the discussion chapter?

P16/L338 "Snow cover plays an important role, as snow covered areas can increase of 250 m the elevation of the MGT20 0 °C isotherm."

Sentence is difficult to understand. Could you please rephrase?

P16/L342 "In Fig.5c is presented the 2D model simulation at the geophysical profile location."

Suggestion: The 2D model simulation at the geophysical profile location is presented in Fig. 5c

P16/L346 "indicate the presence of sporadic permafrost on the summit"

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

P16/L347 "Both datasets indicate a mostly unfrozen south face, and a colder north face"

This information is already given in this paragraph (Lines 344-345)

P16/L347 "However, the ERT data indicate a large unfrozen section at the extremity of the north face."

As mentioned above, I think a little more background on the ERT-dataset would be helpful. Especially with view to the fault zone and potential effects on the resistivity distribution. One problem I see here is, that there are a few redundancies, as the ERT data are already presented in chapter 4.2. In contrast, there is no discussion on the ERT data in chapter 5.

P17/L356-363 General Comment on the use of "MGT20" and parts of the discussion, where you state, that the model is suitable for evaluating ground temperatures below the depth of zero annual amplitude (P20/L403)

This might be a relatively fundamental comment, but could you provide reasons, why you refer to MGT20 as a relevant depth with regard to ground temperatures? I can see, that the reliability of your

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

P17/364 "(Fig.9).For"

Please add a space before For.

P18/Figure 8 (b) Could it be helpful to add the mean Temperatures of SIS2021-01 as recorded in 2021 as a reference?

P18/Figure 8 (c) Please check the readability of all numbers in the figure. The Degree-Values are hard to read

P18/L383 "A more reliable assessment…"

The section starts by stating that reliable data have been collected and ends by stating that the model is not suitable for short-term changes above zero-annual-amplitude. Then it continues with the statement that a higher reliability is achieved for depths below the zero-annual amplitude. Here it would be nice if this contradiction could be derived a bit more clearly.

P18/L384-385 "Although our model seems to provide good results (mean error of 0.14 °C from 0 to 100 m depth, 385 Fig.4) (…)"

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

P19/L391 "(…) we believe that this performance difference is likely imputable to our climatic database"

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

P19/L397-398 "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"

I have trouble following the reasoning at this point. Can you please revise this section?

P20/L400-402 “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.”

With regard to Figure 5 I would like to ask if this observation is really correct. It looks like resistivity data indicate “Transition Zone” or “Potentially thawed” in areas where the model indicates negative temperatures. (As there are no distances or North/South included in the Figure, I refer to the area between peak and G-RF. Is this correct?

P20/L404-406 “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. This is achieved with using forcing data available at the Greenland scale, making an significant step towards a comprehensive assessment of mountain permafrost in the region.”

Here you provide a conclusion at the end of one discussion chapter. Anyway, I would suggest, to either delete or strongly rephrase this statement with regard to results and possibilities.

P20/L409 “temperatures in rockwalls seems comparable”

Think it should be “seem to be”

P20/Chapter 5.3 Discussion?

In my opinion, this chapter does not really correspond to a discussion. Rather, results are repeated. Also, at the end of the chapter, where Krautblatter et al. and Magnin et al. are cited, nothing is really discussed. In my opinion, this chapter is not really purposeful as long as the only result of the discussion is that climate change leads to permafrost degradation. Also, the reference to rockfall compared to Krautblatter et al. cannot really be derived from the discussion in this chapter.

P21/Figure 10 - I consider the information in this figure to be somewhat misleading. Basically, the comparison is very interesting, but it seems that the data is not really representative for the regions listed. It appears that meta-studies are cited, but as far as I could understand based on the literature mentioned, they are mostly case studies referring to specific locations. While the studies in Norway include one northern and one southern site, for the European Alps there is one study that refers to an area in the Mont Blanc massif. Is this correct? How representative are the studies listed for the regions? Can the graphic be optimized and made more concrete?

P21/L433 “quantification”

What is meant by quantification of bedrock permafrost? Far as I can see, a real quantification is not given within the manuscript?

P21/L434-435 “The modeling approach produces results that are consistent with available data from deep boreholes and geophysical investigations”

In my view, this conclusion is somewhat imprecisely worded. It sounds as if reliable results have been obtained, both in comparison to borehole data and to the geoelectrical measurement, which does not correspond to the statements in chapter 5.1.

P21/L435 One “Permafrozen” is left.

P21/L441-447 This section focuses on the relationship between permafrost degradation and rockfall activity. First, I would note that this is an outlook rather than a conclusion, as there is no data on these points within the manuscript. Secondly, I am not sure how relevant the results of the study are to establish a link between permafrost development and rockfall. Of particular importance to rockfall

activity are the near-surface processes. However, the manuscript primarily cites temperature development at 20 m depth. The connection between permafrost degradation and mass movements is definitely extremely relevant, but I do not see any points in the manuscript that allow conclusions that go beyond the numerous existing publications. It could be made clearer here what is conclusion and what is outlook.

P21/L446 “(…) and crack networks (…)”

This is a point I raised earlier; apparently there is a "lithological fault" in the area of the ERT array. This point is raised but not comprehensively discussed. Can you add a few points in the manuscript that makes a conclusion on this issue a bit clearer? Or that shows how the lithological fault mentioned in the manuscript affects the results?

P21 Conclusions in General

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

P23/L464 Wrong DOI for Allen et al. 2009