We sincerely thank both reviewers for their constructive feedback, which helped a lot to improve the paper. It became clear to us that the paper was not convincingly written and we carefully checked the entire manuscript trying to clarify and precise it.

Answers to reviewer 1

The manuscript presents a new country-wide permafrost map over Switzerland, which is based on statistical analysis of the national borehole-data network. During the mapping process, they distinguish between ice-poor and ice-rich permafrost, where the latter normally is associated to rock glaciers or talus accumulation, spatially often detached from the more continuous permafrost zone above in elevation. They used a multiple regressions approach to map MAGT for these two different zones. The study is innovative as the permafrost types react differently on climate perturbations, and in alpine terrain ground ice has not been systematically mapped before. Thus, the study deserves attention. However, there are several issues which should be resolved before considering publication.

In general, the paper is wordy, and can be shortened substantially. Tell the reader what you have done and avoid reporting-style.

Answer: We agree

Changes: We shortened the paper and deleted some repetitions and unsubstantial phrases

There were several parts I really did not follow even reading the passages several times.

Answer: Considering the following comments, I guess this refers mainly to the method section

Changes: The methods section was completely restructured and a supporting figure was added (Figure A) See detailed remark points below.

1. Abstract. The abstract could be improved, what approach is chosen, what results are obtained which deserves attention?

Answer: We agree

Changes: We rewrote the abstract following your suggestion

2. Introduction: The introduction reads partly as a part of a discussion. An introduction should give the reader the background, not a discussion about what approach you have chosen. And it should end up in testable hypothesis, research questions or similar. This seems a bit mixed now, and should probably be re-formulated.

Answer: We agree

Changes: We rewrote large parts of the introduction considering your remarks and removed the last discussion-like paragraph

3. Methods: This chapter needs revision. To be honest, I still do not quite understand what the authors did in all details, and in which order.

Answer: We realized that it was a major problem to follow our analyses.

Changes: We completely restructured the method section, switching to a chronological explanation of the work steps, which are now easier to follow as well as better justified.

The description is full of report-style deviations in between substantial information. Sentences like "Attributing a MAGT to each thermistor is straightforward" have nothing to do in a scientific paper. Or what was "difficult" or not.

Answer: You are right

Changes: We deleted phrases like that.

I did not follow the handling of steeper slopes than 40 deg, in relation to PR calculations.

Answer: This was not explained well.

Changes: We rewrote this and supported the understanding with two new formulae.

Maybe a flow chart helps to describe the different steps.

Answer: We renounced on a flow chart here but added a new formula.

Changes: We added a new formula.

Give justification why you used different parameters, such like the selection of explanatory variables or why you consider points "5 times minimal distance of thermistors".

Answer: The distance threshold was optimized empirically. How, is explained in more detail now.

Changes: We gave reasons for our choice of the explanatory variables.

Eq.2: I understand you do a sort of interpolation, but I do not understand why and how you chose the factors. I do not follow p. 5, line 10 and following.

Answer & Changes: This was completely rewritten explaining better what we did and why we did it.

The sensitivity tests: Nice, but now you mentioned more parameters included in the calculation of solar radiation?

Answer & Changes: This is now coherent with the first part of the method section

Mapping of zone 2: Here you really need a flow chart, I cannot follow this, maybe some maps would help to illustrate the different steps, and a justification for e.g. the slope limits used, the size of the buffer zone around the runoff tracks etc.

Answer: The slope limit was found in (Kenner and Magnusson, 2017), which is referred to here

Changes: We insert a figure with 6 maps that represent the different work steps.

But as far as I could make out of it, you do an analysis identifying different types of mass wasting landforms (e.g. avalanche deposits, talus etc.), do some manual editing and comparison to existing data sets, and then you do what to say that the landform is part of zone 2?

Answer: We defined a narrow as possible zone in which the development of ice-rich permafrost is possible. Whether permafrost actually exists at a certain location in this zone is unknown and impossible to model as it depends strongly on the formation history of the talus ground. (Was there coverage of avalanche snow or glacier ice? Was it buried fast and deep enough to be sufficiently protected from melting? Did large scale rock fall occur which created blocky and porous ground layers which enable cooling by ventilation effects?) All this is of course unknown for the large scale.

Were there new regression for these areas, or you used only the regression values from zone 1?

Answer: We used no regression for zone 2. This is now stated more clearly.

I see you mentioned later that if including the boreholes in zone2, the performance goes down. Ok, but is there some calculation to identify zone 2permafrost or only the mapping? Please clarify these parts. It would help if you show maps of how you classified zone 2, this does not need to be in the main text, ok within an appendix if possible.

Answer: Only the mapping approach was used to identify zone 2.

Changes: As you suggested, we added a figure showing 6 maps which represent the different working steps for creating zone 2.

4. Validation: Ok, but could be part of the methods section, does not need an own chapter.

Answer: You are right.

Changes: We included the validation part in the methods

5. Results: Ok, show a map to present the results, this is a mapping exercise, and it is good for the reader to see a map then.

Answer: We agree

Changes: We moved figure 7 into the results section

The validation is good for zone 1, does not work on zone 2. I would prefer error matrix analysis instead of the histograms (or in addition) against real observations, which also would provide a classification performance measure in addition to the regression-R2s.

Answer: I am not sure if I understand this point. An elevation-dependent analysis of real data does not make so much sense as there is hardly data available for such an analysis and the existing data is biased in their distribution, depending on the motivation of their acquisition (an overrepresentation of north facing permafrost sites at elevations between 2500 and 3000 m asl.)

I am not sure if the comparison to other modelling results (Böckli, Gruber) is a "validation". I think, no, validation is against something we know is true or false, the other maps are also models with their biases. You also show this again in the discussion (now with maps), the comparison should be a result.

Answer: Well, this was never called a validation. We validated each of these maps using our set of validation records and the results were compared. It is a comparison of the performances of the different maps to show improvements achieved with the PGIM

6. Discussion: The first paragraph is more a result, as is the figure.

Answer & Changes: We slightly rewrote this part although we consider this paragraph as an interpretation of results

Paragraph p.14,line 10 is repetition, the same first paragraph in 5.2. Please avoid redundancies in the manuscript.

Answer: We agree.

Changes: We removed this.

Finally, the discussion is very close to the Swiss conditions, and the comparison to earlier mapping approaches. A discussion should highlight and discuss the results to general science, which turns such a study from a technical report to a scientific contribution. Therefore, I miss comparisons to other areas, a discussion of the transferability of your approach, the evaluation of the use of other statistical approaches like GLMs or similar (see e.g. Hjort et al, recently in Nature Geoscience, or Aalto et al in GRL), the comparison of your model with such approaches.

Answer & Changes: We added a section on the applicability of our approach to other regions and focused more on this issue in the entire paper. A comparison to lowland permafrost is however difficult as there are some considerable differences to (gravitational) processes taking place (or not taking place) in mountain permafrost due to the steep topography.

7. Conclusions: The conclusions states in several places that your approach is a real improvement, and this can be done in other regions. Probably, but then you need the same training data basis (which is exceptionally good in Switzerland), or are the regression coefficients universal? I do not think so. And if it so, why did you not test to transfer your approach, I guess it would be easy to transfer it at least into other areas in the Alps?

Answer & Changes: We added a section on this issue to the discussion.

What is the last conclusion point? I think you may consider reformulation of the conclusions.

Answer: We missed to discuss this point before the conclusions.

Changes: We discuss the last conclusion point before and rewrote some of the conclusions.

In summary, the paper needs major improvements to convince the reader that the presented approach includes a major step forward. I am sort of convinced that this is important, but the paper is hard to read and follow. Especially I am a bit puzzled around how zone 2 is treated in the final product. Distinguishing ground ice content is normal in Arctic permafrost regions, but little considered on mountain permafrost, and only related to clear landforms such like rock glaciers or frozen peat plateaus. This knowledge has be part of permafrost models, which is, as far as I see, the major message from this study.

Answer: You are right: we rewrote the paper giving clearer explanations on our methods and clearer information on the relevance and improvement of the permafrost mapping in the international context.

Further changes: Some reference boreholes used to set up the PGIM zone 1 regression model were used together with the validation points to validate the permafrost maps APIM, PPDM and PGIM. They were accidentally part of the validation dataset. Using these sites for the PGIM is critically if validation and reference sites are not properly distinguished in the results. We reworked the figures showing the validation results and highlighted all reference sites in figure 4, which shows the validation of the PGIM. We furthermore highlighted all reference sites in table 2 and adapted the manuscript section.

Answers to reviewer 2

The paper presents a new mapping approach for mountain permafrost in Switzerland accounting for ground temperature and ice content. The study is based on regression analysis using borehole temperature collected in the Swiss Alps. The overall interest of this study is to propose a statistical approach to distinguish ice poor and ice rich permafrost in a mapping exercise, and to provide a more detailed and more accurate map of mountain permafrost distribution in Switzerland, representing permafrost #gaps # in its altitudinal distribution resulting of the combination of topoclimatic factors and ground ice content. The approach and objectives of the study are sounds and well-suited for the journal, but it is very hard to provide a detailed and constructive review on the scientific content at the current stage. The writting misses dreadfully conciseness and precision. The paper can only be accepted after major revisions, notably rewritting of most sections to make it easier to understand and to follow the different steps.

Answer: We agree to this general comment and made considerable changes to the paper to improve its clarity and preciseness.

I have tried to formulate general comments to in order to guide the rewritting but I finally dropped many detailed comments as it was too messy. General : One strinking thing is the lack of references to the international research context : most references to previous work and knowledge focus on studies conducted in Switzerland, if not on the research team. Given that this study is submitted to an international journal of broad significance, one could expect that the international and broad significance of the paper is clearly stated and explained.

Answer: We added more international references. They are listed at the end of this document. You will find some Swiss authors there as well, these studies are however focused on different world regions. Mapping of mountain permafrost is not carried out in many countries outside of the Alps (e.g., Norway, Iceland, Canada) and Switzerland plays a pioneer role in this field. This additionally explains the high

number of Swiss references. We better embedded the study in the international context by including a section on the applicability of the method to other regions.

Changes: More international references, new section on generic use of the method

Abstract : it lacks of precise results about the predicted permafrost distribution and the improvments achieved using this innovative approach. Stating that allowed a clear improvement is inappropriate and would first need to be desbribed. In general, the abstract is rather coarse. I suggest to rewrite It based on the following outlines (or similar, this is just a suggestion) : 1. State about the overall context, relevance and objective/research gaps and research questions of the study, 2. Briefly explain the chosen method, 3. Provide key results (as quantitatively as possible) 4. Explain the main implications of the results and main answer to research question.

Answer & changes: We rewrote the abstract based on your suggestions

Introduction : Rather badly organised also. The study is introduced as early as L15with # The permafrost ground ice map.... #, but followed by decsription of the scientific background are given.

Answer & changes: We rewrote also the introduction and reordered its structure

This background is mainly based on studies from the 1st author and reference to the international context would be welcome.

Answer & changes: We substituted the mentioned references by other authors

However, the lines dedicated to introducing the study (p3 L10-15) are very poor. Try to be more straight-forward and precise in your description : what is the specific approach you choose ?Why (based on the background you described above) ? What are the main expected results to fill the research gap ?

Answer: We rewrote that considering your suggestions.

Changes: The aims and approach of the study is now introduced at the end of the section.

Methods : Here again, it is difficult to follow as the organisation of the method description is messy. Try to be more specific and more logical with the titles in order to ease the reader following the approach. It starts with # mapping #, then with a # sensitivity analysis #, # a testing of zone 1 for zone 2 # while zone 2 is presented only afterwards.

Answer: You are right, methods were messy, see also comments to reviewer 1.

Changes: we rewrote them following a chronological order of the work steps and explained better what we did and why

Similarly, the subsections are very unequal (2.3 is very short and one can wonder about its relevance). The short paragraph introducing Section 2 (p3 L 17-22) does not reflect the main outlines of the appraoch as it should, it just give general information about the maps. I wish I would have found a flow chart describing the methodological approach and this should be considered in the revised version. For example, illustrate the # buffer # (P3L25), the # mapped with blue colors # or # mapped in yellow #

Answer: We agree with this remark

Changes: We restructured all this. A supporting figure was added visualizing the different steps leading to the delineation of zone 2.

(L27).P5 L19 : I wonder if the regression coefficients would rather be in the results section while the method should rather describe the statistical approach. The meaning of the regression coefficients has to be briefly expanied (results).

Answer: The regression parameters have no special meaning, they are just technical values of the linear function describing the interaction of solar radiation and MAAT regarding their effect on ground temperatures. As these technical values give no further information to the reader than that what the reader can see in Figure 1 we have the feeling it is easier to give the values in the method section rather than starting an additional paragraph for that in the result section.

Your sensitivity analysis lacks of reference to common statistical methods. It is not clear whether this # bisected# sample is a common way to test model senstivity or if you have randomly decided it.

Answer: A common way to test the sensitivity of a model is to define critical input variables and to vary them based on a realistic assumption of their uncertainty. Bisecting the statistical population gives in our view a realistic indication of the uncertainty: The model result becomes more stable as more reference values are used to calibrate it. Stability grows however not linearly with the number of reference values but the stability decreases exponentially with decreasing reference values. This means doubling the number of reference values has a much smaller effect on model stability than bisecting the number of reference values. The bisecting method applied here will therefore rather underestimate model stability and can be considered as conservative estimate of the model sensitivity.

More technically, I think that there is a misuse of # PGIM # in Eq. 3 since the acronym refers to the map and Eq. 3 is the regression analysis.

Answer: Yes you are right

Changes: we changed “PGIM” to “MAGT(PGIM)”

Section 3. Isn’t it part of the methods?

Answer: Yes it is.

Changes: We moved it to the methods

Section 4 and 5 are better written. However, in my sense, description of map features (general and more detailed at selected areas) is lacking as this study is a mapping exercice. What do your results show in term of permafrost distribution ? What is the elevation belt without permafrost for example ? Giving such information will, in my opinion, strongly broaden the significance of the results.

Answer: The permafrost-free belt has no fix elevation values but its upper boundary depends on aspect, slope and ground characteristic and its lower boundary on terrain form. We show a histogram in the supplementary material showing the permafrost distribution over elevation, including the permafrost gap.

Statistics given in 5.4 could be merge with such results (map) and therefore moved in section 4. They would be easier to get in a Table.

Answer: You are right.

Changes: We moved this to the results

Unless I missed something, the data that you use in Figure1 and 2 are not very clear also : is it annual average ? multi-year average ? others? which measurmeent years ?

Answer: It is a multiyear average.

Changes: We added the years in table 1.

Title of 5.5 is not coherent with the content, even ifit deals with ground temperature and ice content, the focus is more on implication of such a map for its use.

Answer & Changes: We renamed the title to: Practical relevance of information on ground temperatures and ice content

Finally, as mentioned in the general comment, one expect that the authors place their study in an international context, at least in the discussion, and this terribly missing.

Answer: You are right

Changes: As mentioned above we inserted a section, which discusses the applicability of the methods to other regions.

Conclusions : they are poorly written. They are very general. They have to be written again with precise results and implications

Answer & Changes: We partly rewrote the conclusions

Further changes: Some reference boreholes used to set up the PGIM zone 1 regression model were used together with the validation points to validate the permafrost maps APIM, PPDM and PGIM. They were accidentally part of the validation dataset. Using these sites for the PGIM is critically if validation and reference sites are not properly distinguished in the results. We reworked the figures showing the validation results and highlighted all reference sites in figure 4 which shows the validation of the PGIM. We furthermore highlighted all reference sites in table 2 and adapted the manuscript section .

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