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The Cryosphere Discussions

Interactive comment on "A statistical permafrost distribution model for the European Alps" *by* L. Boeckli et al.

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Author's comments (AC

We thank reviewer 2 for the valuable comments, which helped to improve the manuscript, and have addressed all of them in the revised version. Our point-by-point response to all comments and the resulting changes in the manuscript are described below.

The main changes of the manuscript include the sections "Introduction", "Background" and "Conclusion", which have been restructured and partly rewritten. Further, the former section 3 "Data" was placed after the section "Statistical Method". To address

the main comment, i.e. that the model was not actually applied in this contribution a sample map showing permafrost probabilities is presented in an additional figure. It is further emphasized more clearly that additional calibration and validation steps based on external data sources are necessary prior to presenting a final alpine-wide permafrost map, which will be presented in an additional paper.

General comments

In the paper titled "A statistical permafrost distribution model for the European Alps" the authors are attempting to create a model of permafrost distribution for all of the mountainous terrain in the European Alps. I feel the concept is very good and exposes a very needed aspect of empirical permafrost modelling in Europe. Although I admit there is a need for this type of study, I believe the manuscript in its current form is somewhat incomplete. Going through this paper as a reviewer I was anticipating the end result however, it was not delivered. I however, understand that that authors are presenting a methodology in this paper and not the mapped results themselves however, I was left wondering if the manuscript would benefit from simply waiting until the final map and modelled results are presented. This being said I must add that I believe the approach of using the debris and rock based models is good. I think what is really missing in this manuscript is a map illustrating how the model will predict permafrost distribution across space. I understand that at this point it is likely not ready for the entire Alps however, a test section map would be interesting to see, and I believe is needed. I have recommended to publish this manuscript in The Cryosphere with minor revisions but believe the manuscript can be shortened almost to the point of a short communication rather than a full research paper so this is something the authors might want to consider.

AC: The objective of our contribution is to introduce a modelling approach that is capable to address the specific needs of permafrost distribution models for entire mountain regions. This objective is now expressed more clearly in section "Introduction" and

the conclusions better underline our contribution with respect to this. Further, we have better stressed as one of the main conclusions the insight that such a model cannot be directly applied to an entire landscape but require later subjective adjustments. For this, we have to be submitted a companion article (Boeckli et al., to be submitted) that describes the steps for and results of a model application in detail. But to illustrate a potential model application in this manuscript we have included a figure (Figure 1) showing an example map.

Given the detailed description and reasoning of the model derivation, the results and conclusions presented in this manuscript, which we have reworked to better underline our main points (cf. also response to comment 1 by reviewer 1), we believe this contribution to be a full paper, which cannot be compressed to a Short Communication.

Specific comments

For all specific comments of reviewer 2 not specifically addressed below, the manuscript has been changed following his suggestions.

2) In the introduction line 25 (pg. 1421), get rid of the term possible permafrost avoid using these very ambiguous terms. This model is exploring the idea of obtaining permafrost probabilities so in this case use numbers instead.

AC: The probabilities we model strictly hold only for rock glaciers being intact as opposed to relict or MART being <=0 °C as opposed to >0 °C. While this paper describes a statistical model that results in probabilities, its application to real landscapes with diverse surface types requires a re-interpretation into an index that cannot be regarded as a probability. We have rephrased this to "...a warning map providing a qualitative information of the probability of permafrost."

4) Line 10 (pg. 1422) I was under the impression Haeberli introduced BTS in 1973 not 1975 please check this reference.

AC: Done, changed reference to Haeberli 1973.

5) I really think a study areas section is needed even if it is just to define the lat/long areas that the model will be run for.

AC: We clarified what we mean with Alpine-wide: "(43–49 °N, 4–16 °E)". Describing a study area in detail is not necessary for this paper in our view.

6) Line 17 (pg. 1425) you mention that your PISR model uses "clear sky" conditions. What does this mean? Does this mean no cloud cover? Please explain this better in the text.

AC: We added the following parenthesis to clarify this: "100% transmittance."

7) PISR can be calculated using several programs however, usually only over 1 of latitude, please explain how this will be calculated and speak about the extent area.

AC: We added the following sentence to clarify this: "PISR values are calculated for a latitudinal extent of 1 degree (6 bands according to the total latitudinal extent of our study area)."

8) Line 23 (pg. 1425) you mention a constant lapse rate of 0.65 C/100m is used. This is a constant lapse rate however, these values can be very different from one mountain location to another and change with the season. Is this measured on an annual scale? I would like to see some kind of lapse rate comparison using sensors at different elevations showing that this assumption is justified.

AC: This is a usual standard value (cf. International Standard Atmosphere, International Organization for Standardization, Standard Atmosphere, ISO 2533:1975, 1975) and we use it on MAAT as stated in the respective sentence. While we agree that the mean annual lapse rate may differ and be spatially variable, the effect of this is negligible as we only use it to adjust MAAT from the level of a 1 km grid to that of a 30 m one and vertical extrapolation is therefore small.

9) In Section 4 (Statistical Methods) I find the presentation of the equations hard to follow and the terms are also not easy to locate the meanings. I think this section would benefit from a small table showing the meaning of the symbols.

AC: Some of the symbols could be avoided in the revised manuscript. It would be difficult to express the remaining symbols in a table without repeating the expressions defining them. We therefore decided not to include such a table.

10) I find the conclusions section just summarizes the topics in the paper and think it could be better organized to include what the papers scientific findings are.

AC: Done, see response to the general comments 1 and 8 from reviewer 1.

13) In figure 1 the country boundary lines should be thinner, in addition an inset map should be added and see if a better shadowing of the topography can be added.

AC: The country boundary lines were made thinner. Instead of inserting a small map, we added the abbreviations of the countries as suggested by reviewer 1. The colours of the topography were slightly changed.

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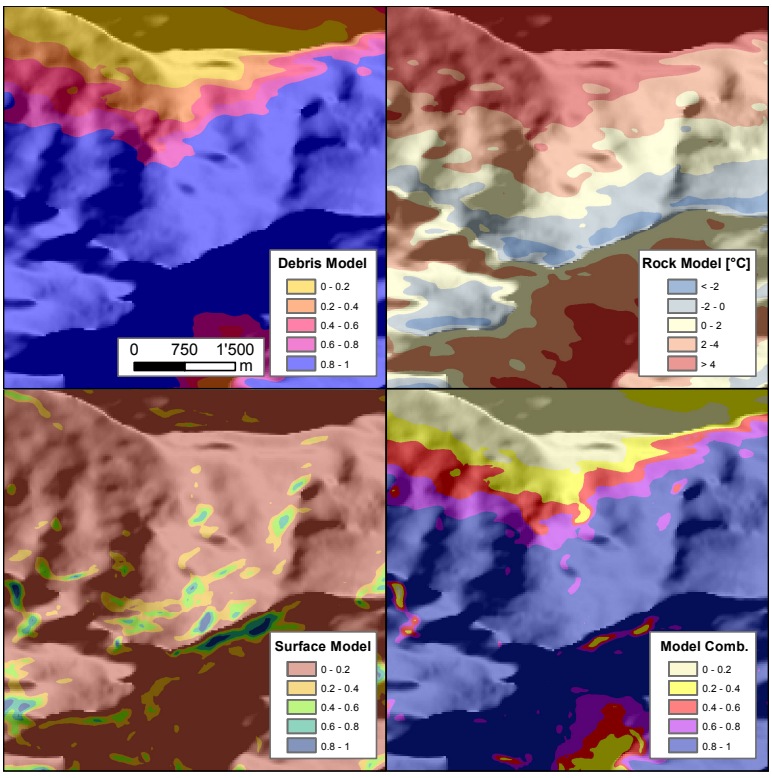

Fig. 1. Example of the application of the different models. Top left: Prediction of the debris model showing probabilities of permafrost occurrence. Top right: Predicted MARST values of the rock model. Bottom