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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 3 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

The authors present a new statistical model, or, better, a combination of models to predict permafrost distribution of the European Alps. The main aims of the paper are not really well presented. The authors stated that the focus of the paper is the analysis of the explanatory variables, the development of the statistical sub-models and their combination but, both in the structure of the paper and in the title these aims are not truly developed in a clear way. The title, for example, seems more to suggest that the novelty of the paper is a new statistical permafrost distribution model for the whole European Alps, implicitly suggesting the presentation of one map with a calibration of the results.

AC: The title was changed to better reflect the aim of the paper: "A statistical approach to model the permafrost distribution in the European Alps or similar mountain ranges".

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. A combined manuscript would be too large and the scope of the two papers, both in terms of methods and geographic transferability, is different.*

Unfortunately this map is not presented and also the calibration of this permafrost distribution is practically absent.

AC: See above: A sample map is shown now (Fig. 1) and a second paper addressing the application of the model has been to be submitted (Boeckli et al., to be submitted). The calibration, however, is described in detail and we do not understand the second part of the above comment.

The manuscript is not easily readable and in general it needs a shortening to avoid several repetitions especially on how the models work. In particular, the paragraphs Introduction and Background can be shortened and included in a single paragraph (Introduction).

AC: The sections "Introduction", "Background" and "Conclusion" were restructured and partly rewritten. Section "Background" was renamed to "Conceptual background" as suggested by reviewer 1. Several repetitions were removed (see specific comments of reviewer 1).

The main part of the statistical methods could be moved in an appendix.

AC: We think this is not possible, because the development of a new statistical approach and the way we address the main challenges to model permafrost distribution over a large region (see also response to comment 1 by reviewer 1) are the main motivation for this publication.

The references are not complete both regarding the methods as well as the permafrost

models and generally the self-citations are too abundant and redundant.

AC: We added references concerning the statistical models (Hosmer and Lemeshow, 2000; Gelman and Hall, 2007; Hand, 1997; see also comments of reviewer 1). Regarding the permafrost models we do not know what kind of references are missing.

However, the most important points regard the data set that the authors used for the models and on their calibration that require, at least, a critical discussion and not a simplistic assumption.

AC: It is not clear from the reviewer's comment which particular assumption is referred to as "simplistic", but we assume that we addressed this point in the following detailed comments.

Specific comments

1) The choice to neglect GST, BTS, geophysical data and borehole temperatures is difficult to understand, also because the number of these data is quite high and useful to calibrate permafrost distribution.

AC: See answer to general comment 4 of reviewer 1.

2) It is not clear why the authors joined active and inactive rock glaciers in the category of the intact rock glacier. In many inventories (and may be also some inventories used by the authors) inactive rock glaciers are considered indicators of past permafrost distribution as the relict rock glaciers. Therefore the authors should at least explain the difference between inactive and relict rock glaciers and why the inactive rock glaciers should be included in the intact rock glacier.

AC: We added the following sentences to make this more clear to the reader: "Rock *glaciers can be classified into intact (active and inactive) and relict forms (e.g., Haeberli, 1985) and are a well visible geomorphological feature, which can be mapped relatively easily from, for example, aerial photographs. Active and inactive rock glaciers are grouped as intact rock glaciers because of their existing ice content (cf. Haeberli, 1985; Ikeda and Matsuoka, 2002; Roer and Nyenhuis, 2007; Lilleoren and EtzelmÃĺuller, 2011) and the reliable indication of permafrost they offer. Relict rock glaciers do no longer contain ice, show a collapsed surface due to melting of the ice, and they often present a vegetation cover (Roer and Nyenhuis, 2007)."*

3) Rock glaciers should be used with care to calibrate permafrost distribution because is well known (as stated also by the authors) that their surface conditions can produce local perturbation of the thermal state of the ground, with important cooling, and therefore with a overestimation of the permafrost distribution.

AC: We agree with reviewer 3 that this is an important point. We therefore added the following sentence in the conclusion: "By using intact and relict rock glaciers as calibration data, the prediction of the debris model is biased towards and overestimation of the permafrost distribution."

4) It is not surprisingly that the probability of a rock glacier being intact is positively associated with increasing PRECIP because this reflects the possibility that they are debris rock glaciers. Indeed, debris rock glacier are very widespread in several parts of the world, including European Alps. Despite of their origin, debris rock glacier are mostly developed in recently deglaciated areas or in the wetter areas of the Alps. Personally, I think that the authors should test their models using only talus rock glaciers.

AC: The rock glacier inventories assembled by Cremonese et al. (2011) do not differentiate "debris rock glaciers" and "talus rock glaciers" and the distinction of those forms may also not always be simple in the field. We added the following sentence to

our manuscript: "Debris rock glaciers, referring to rock glaciers developed in strong relation to a glacier (e.g. Barsch 1996, Hughes 2003), may offer an explanation for the positive coefficient of PRECIP in the debris model. However, their precise definition is difficult and as a consequence their influence on the debris model can not be assessed."

5) Regarding the MARST the authors should show more details of the data considered. Infact the depth of the sensors, their aspect and type of rock is not described as the year of the measurement. It is important to know these characteristics because the number of sensor is not so high and their distribution is localized only in some particular areas of the Alps and therefore this data set does not seem to be enough complete and robust to test all the investigated area.

AC: The calibration data for the rock model is indeed sparse as stated by reviewer 3 but it represents all available measurements in steep bedrock in the Alps. We have now included more information: "The data originate from eight areas (Fig. 1) within which a wide range of aspects and elevations has been sampled. Measurement depths are on the order of 10 cm. The rock types sampled vary between areas and include limestone, granite and gneiss." Details regarding MARST data are not included because this information is contained in the cited literature. We have also added more explanation regarding the low number of measurements: "In comparison with the high number of rock glaciers available, 57 measurement points are few. They are, however, used for describing a system that is much less complicated than rock glaciers because the influence of snow, phase change, a mixed-media active layer and the down-slope displacement of ice-rich material is minimal or non-existent."

6) It is also not clear why the authors need to adjust the MARST to longer term measurement and how they did this. Why they used Piz Corvatsch site and especially why they used the period 1961-1990? The period of 1961-1990 is surely not appropriate if

the authors want to give an actual permafrost distribution because the warming trend was much more pronounced since 1990 to now.

AC: We need to homogenize MARST values to a common temporal level, because the MARST values of the different locations correspond to different years. By doing so, we tried to minimize the influence of single years (especially important for anomalies such as in 2003), which would bias the regression model. By using only the station of Piz Corvatsch we assume, that inter-annual variation of one specific year to its long-term mean is equal for all stations in the Alps. The period of 1961–1990 was chosen, because it is a commonly used reference period for climate normal"s. The concept of an "actual permafrost distribution" is difficult because it neglects the delay inherent in thermal phenomena at depth and their transient nature. For the determination of current areas in which permafrost may exist, the choice of a base-period with high quality data (such as the normal period 1961–1990) is more important than updated data reflecting the latest changes.

By using this period for manual correction we use an optimistic (biased toward an overestimation of permafrost) approach that is in line with the debris model. In the manuscript we added: "By using the period of 1961–1990 as reference, the air temperature warming especially in the past decade due to climate change is neglected. This leads to an optimistic estimation (biased towards an overestimation of permafrost distribution) of MARST, but is in line with the debris model that also follows an optimistic approach."

7) The assumption that MARST follow MAAT is quite simplistic considering that is well known the effect of the radiation on the steep rock face.

AC: Reviewer 3 refers to p1425, line10, where we stated, that we assume that MARST follows MAAT. This sentence may be miss-leading, but here we refer with ÂńfollowÂż to changes and not to absolute temperature values. We have replaced the sentence:

"The underlying assumption is that the difference of MAAT to its longer-term mean and the difference of MARST to its longer term mean are equal." (cf. Figure 3.1 of PERMOS 2010)

8) The PISR was calculated for which year?

AC: Neglecting longer-term effects such as Milankovitch cycles, PISR has no inter-annual variation. (PISR does not account for actual cloud cover in a particular year, among other transient effects.)

9) The Lapse rate of 0.65 C/100 m could be not appropriate in several areas of the Alps and for several months in the year, please specify why they decided this lapse rate.

AC: See answer to Comment 8 of reviewer 2.

10) The size of the model for the precipitation 15 km seems not really appropriate considering the strong local variability of the precipitation in the Alps. There are some areas in which you can pass from 1500 mm/year to only 900mm/year in less than 15 km!

AC: As far as the authors know, there is no better precipitation data available for the Alps. Further, this variable has a regional character as stated on p1439 line2.

11) It is not clear which is the method used to distinguish the different surface types and which is the accuracy of this method, please specify.

AC: In Sect. 4.5 we proposed three different types of methods to distinguish between different surface types. The assessment of their accuracy is required when the model is applied (that is not in this manuscript).

12) The three subset representing the different climatic conditions (drier-wet) should be clearly defined. In conclusion, I think that the manuscript is not acceptable in this form and that the authors could try to rewrite the paper with more care on the used data set and on the organization of the paper.

AC: The three subsets representing the different precipitation conditions are defined in the caption of Fig. 5 and are now included in the revised manuscript in the text.

As stated above, we improved the organization of the paper and restructured large parts of it. Several repetitions were removed, especially regarding the different models. The data that is used is now described more in detail and the statistical models are introduced more carefully in the section "Conceptual background" and at the beginning of the section "Statistical Methods".

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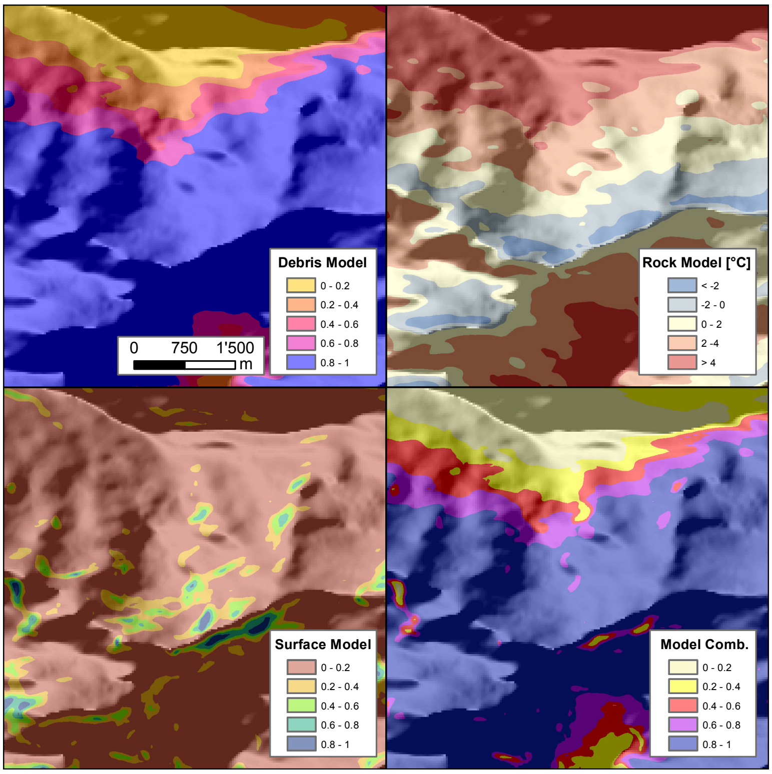

Fig. 1. Example of the application of the different models. Top left: Prediction of the debris model showing probabilities of permafrost occurrence. Top right: Predicted MARST values of the rock model. ...