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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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General Comments

The authors present a new concept how to establish a statistical permafrost distribution model for the European Alps. The benefit of an Alpine-wide consistent permafrost model is of importance, for example for areas where the permafrost distribution is still unknown. In addition a distribution map of this region would be of high scientific interest (e.g. studies focusing on the impact of permafrost on climate change).

This study combines two sub-models (a rock- and a debris model) that accord to the surface types. The rock model uses a linear regression with outcome variables on the continuous scale, whereas the debris model applies the Generalized Linear Model with

a probit link function on a binary scale. Thus, as the tolerance distribution the normal distribution is used. To calibrate the model a set of various rock glacier inventories and rock surface temperatures were used. The application of the statistical method is adequate and correspond to the available data set in the European Alps. In contrast to the well-known ordinary Least Square Regression the Probit Model is not as common and has some drawbacks (no comparable statistics to regression models).

Unfortunately, in its current form, the MS lacks a clear scientific finding and a firm conclusion. References on recent permafrost modeling studies are given abundant and are up to date whereas that on the applied statistical methods are either sparse or absence. The units for almost all tables are missing systematically. In addition at several places the same text is repeated or text describing ordinary facts is found. From this point the actual MS can be shorten significantly. The reviewer has the impression that this manuscript is only a part of a study. A presentation of the expected result of this study (a permafrost distribution map for the European Alps) and it's interpretation would significantly increase the impact of this study and it's benefit for other research groups.

The major critics are as follows:

(1) Title and scientific findings: The title of the article could be more precise to represent the content of this work and to avoid a misunderstanding as the reader would expect a permafrost distribution map as result. For the MS in its current stage an adequate title would be 'A statistical approach to model the permafrost distribution in the European Alps' or "A pilot study to establish a statistical permafrost distribution model for the European Alps '. The scientific question of this work is clear: to provide a permafrost distribution model that is consistent over the Alps. Further the authors write to focus on the analysis of the explanatory variables, the development of statistical submodels and their combination. Since the application of this statistical methods with permafrost data could be done in a Msc thesis I expected firm scientific findings and aims that top the scientific request of the applied statistical methods. For the reviewer

the scientific findings of this study are sparse and are not even stated in the conclusion. Although observations are used for the model a result of the model (e.g. permafrost distribution map for the European Alps) is not presented. If the authors want to focus on the establishment of a consistent Alpine-wide permafrost distribution model, empirical data should not necessarily be used. Instead the formulation and the theory of the models and their real combination should be the focus of such a study. However, a potential scientific aim would be the estimation of the permafrost distribution in the European Alps, or at least the investigation of the sensitivity of the major quantities that parameterize the model. This include the assessment of the model sensitivity with regard to uncertainties of the model input parameters (standard deviation and spatial resolution!).

(2) At the first view the structure of the MS looks quite well, but after reading particular sections it is found that a concept is missing. This makes it difficult to follow the MS. I suggest to rename section 2 (Background) as section 1 (Introduction) and to move most of the text from section 1 (Introduction) to a new section 2 (Concept). The section 'concept' has to explain which models are used (debris-, rock-), which method is applied (GLMM/nonlinear probability model, linear regression/linear probability model), and which is the outcome variable (binary variable, continuous variable). Further it has to define the statistical terminology (response variable, explanatory variable), to explain why scaling issues are necessary, to introduce the methodology how the model is parameterized and calibrated, and to declare how the model output is tested. This would help the reader to follow the MS and to clearly separate between the two different models. Then it is no longer necessary to repeat in each section how the models work.

(3) References and theory of the statistical methods In the current stage the MS include no information on the Generalized Linear Model, the Generalized Mixed-Effect Model (Dobson 2001, Crawley 2005), and contain only one reference for the Probit Model. It is unclear who apply such models and for which applications it is usually used for.

To make this work comprehensible for the reader it would be necessary to establish an appendix where the major approach/theory of both the GLMM /GLM and the Probit Model is explained. State what are the differences to the well-known ordinary Least Square Regression.

(4) Debris model: The idea to use a rock glacier inventory for the debris model seem to be a good choice if the distribution of rock glacier should be predicted. I suspect that this input parameter not only leads to an too optimistic permafrost distribution (as stated by the authors), but gives an incomplete and poor permafrost distribution (at the wrong locations). Can you show that the same input parameters (MAAT, PISR, PRECIP) that discriminate intact from active rock glaciers can discriminate permafrost from non-permafrost sites that were found by other methods as the rock glacier inventory (see methods in next sentence)? I do not understand why the authors neglect important information that is provided by other methods such as borehole temperature (BH), ground surface temperature (GST), geophysical prospecting (GP), other indirect evidence (OIE), and maybe surface movements (SM) (Cremonese et al., 2011). This information should definitely be implemented in the model and used as calibration data since they give a better delineation of the real permafrost distribution. You easily can use weights in your inversion to consider different accuracies caused by the various methods.

(5) Rock model: The established rock model might be based on a too sophisticated approach to satisfy the main objective of this study (a permafrost distribution model that is consistent over the Alps). I am sure that the authors already have tried a more simple approach for this task. In general, the reviewer favours an approach that avoid the need of scaling issues. The simplest approach would only consider the parameters ELEVATION, EXPOSTION, and maybe PRECIP. However this problem might not be solved in this study, or?

(6) Model combination and scaling issue: For the reviewer it seems that the two independent models are simply connected to together and that no conjoint parameter is

found that is inverted from the combined model. For example, the parameter 'delta' is derived separately from each model (delta_r, delta_d) . Thus I deduce that the model combination is not obligatory and that this would easily allow to use standard methods for the interpolation. This methods could either be geometrical (Minimum Curvature Gridding) or geostatistical (Kriging) where the correlation length is determined in a variogram. The use of the mean value for the scaling issue is adequate if the spatially distributed points show a large scatter (short correlation length in a variogram, random distribution). But if this happens I assume that the explanatory variables (PISR, MAAT, PRECIP) have less contribution to the model. In the other case (large correlation length) geostatistical methods (primarily Kriging) that use variograms seem to be more practical.

(7) Discussion - use and limitation of the model: I recommend that the authors rewrite this section as the current text is not adequate for a scientific discussion (e.g PSIR calculated from a finer DEM is more accurate than from a courser one). The authors should discuss the impact of the adjustment offsets 'delta' on the model result (state values) or to asses, how accurate a DEM must be for the two individual models to obtain a satisfying result for the prediction of the permafrost distribution. This should be done for the lateral sample interval (grid cell size) and the accuracy of the grid value itself (e.g. the elevation, the MAAT) for all explanatory variables.

(8) Conclusion: This section reads such as a summary and lacks in scientific findings. I recommend to completely rewrite this section and to address the aims of the paper. In the current form the authors emphasize that this work is a first empirical approach to establish an Alpine-wide permafrost distribution model, that the influence of precipitation need further investigations, and that this is the first study were the spatial distribution of rock glacier is analyzed in relation to precipitation for an Alpine-wide data set. This is not a firm scientific conclusion for a paper to be published in "The Cryosphere". For example, conclude which parameters are the most important in your model (e.g the model calibration), how accurate they must be (spatial resolution and the absolute

value itself) and how sensitive their respond is to the predicted probability. I suggest to simply state that you have found the relation regarding the influence of precipitation on the rock glacier activity. Thus you can describe a conclusion for Figures 4,5,6. If you are not confident in this result it is better to omit this explanatory variable for the debris model. The listing of steps that are needed to use the established approach for a map based product is rather an outlook as a conclusion, are obvious, and should not be a part of the conclusion.

Altogether in the current stage of the manuscript I recommend to publish this study in 'The Cryosphere' with major revisions. I encourage the authors to do that effort and revise the manuscript accordingly.

The language of the MS isn't evaluated because its not the native language of the reviewer.

Particular comments Abstract: 12: I suggest to use model specific terminology such as 'explanatory variable' later. You can write 'parameters' or 'input parameters' here. 15: Delete the parenthesis - this are too much details. 17: Is the rock model also a GLMM? 19: The root mean square error (RMS) is well-known. Thus you can write '..a RMS of ..' 21: Here I would expect the statistics for the Alpine-wide permafrost distribution.

1. Introduction p1421 5-25: This text is rather a description of the model or a concept than an introduction. Move this text to a section 2. ('Concept') 13: You have to define somewhere which slope angles are related to the term 'steep rock walls'. 2. Background The text in this section rather belong to the section 1 ('Introduction') p1423 5-8: Can be deleted 8-11: Should be moved to new section ('Concept') 11: I miss the mention of the occurrence of the different types of permafrost found in the Alps. In which geomorphologic units does permafrost occur (bedrock/fissures, unconsolidated sediments/talus slopes, rock glacier, etc.) and what are typical textures (fine-grained) for permafrost in sediments? 15-18: Use for example the term 'creep behavior ' instead of movement feature. For the overestimation of the permafrost distribution only

the creep behavior is crucial, not the cooling effect. 22-24: rewrite this text section; e.g. Since 2004 near-surface temperatures in steep rock walls were observed in the Swiss Alps (citation). The result of this studies is that due to the absence... 27: Do you mean the lateral variability of the rock surface temperature? 27-29: The first part of the sentences is obvious and can be neglected. Is the second part really relevant for this study? p1424 1: Rewrite; It has been shown that MARST values can indicate ... (Nötzli et al,?) 2-3: Delete this sentence if it is not relevant for the next sentence. If yes, shorten this two sentences into one. At present this text section is very confusing.

3. Data 3.1 Response Variables p1424 15: The different sources of the data need to be presented in a seperate table. I suggest the following pattern:

response variable—country—region–number of data—author/source rock glacier activity—CH———Ticino–1500————-Frauenfelder et al., /.. MARST————————-CH———Ticino–1500————-Hasler et al., 2011/..

p1425 5-10: The MARST values are computed from your observations and are then corrected by using a function of the MAT. Since a binary variable (permafrost found/not found) would satisfy the input parameter of the statistical model why do you use such a complicated way instead of using only the MARST? In the present approach you are interested to predict lateral variations of the permafrost, and not in vertical direction.

3.2 Topographic and climate variables p1425 The heading should be renamed to '3.2 Explanatory variables'. Otherwise you have to rename section 3.1 to be conform. 12: leave 'in our statistical analyses'

p1426 1: Why do you center PRECIP? Do you need small values for the inversion, or do you need this for your initial model? 8: Specify the term local horizons. Do you mean joints? 10: I assume that you use the ZAMG-MAAT for the predictions. So, how do you estimate the adjustment value for the predictions? 11: This sentence need to be moved to an earlier line - 5. 15-16: You write 'can be used', do you have used data from this techniques? Rewrite this sentence, and specify how accurate the DEM

data have to be to allow reasonable calculation of PISR. It is obvious that if one DEM is too course that you need a better one. 4. Statistical Methods I suggest to use the singular and use either 'Statistic Model', 'Statistical Method' or 'Statistical Modeling' for the heading. 4.1 Theoretical framework The section 4.1 "Theoretical framework" seems to be rather a concept than a theoretical framework. Here I would expect information on the Generalized Linear Model, the generalized mixed-effect model (Dobson 2001, Crawley 2005), and the probit model in general. Explain who normally apply such models and for which applications they were usually used for (references!).

19-27: Move this text to the introduced section "Concept" and rewrite this section according to it's title. 4.2 Model formulation p1427 This section lacks in several issues such as clear description of the used symbols or the number of used symbols which makes it very difficult for the reader to follow the MS. This might be a result of the missing concept at the beginning of the MS. Explain what quantities where a 'tilde' appear does mean. Further clearly formulate which equations are used for the rock- (eq. 3?) and the debris model (eq .7?). You might also introduce sub-sections such as 'Rock model' and 'Debris model'. 1: I would prefer the use of a permafrost definition where the formation of ice is possible (p=P(theta<0). 2-5: Remove the quantities 'theta_max/mean' since they appear only at this point. Instead define 'theta' after the term '..of annual ground temperatures'. 6-7: Why do you write 'may'? Remove this term. Remove 'theta_mean=theta' 9. Use '...that.. corresponds to the normal distribution' instead of '..being normally distributed'. 10: Explain if you use different symbols or the same one for measurements and predictions in your equations. I would prefer to make the text more comprehensible and to omit the term 'prediction' or 'model prediction' at this point. You could introduce this term in line 19. The used symbols for the mean and the standard deviation are very confusing (sigma**2_theta & theta dash). I suggest to use 'mu' & 'sigma**2' for the statistical moments of the ground temperature. To help the reader to understand your quantities I recommend to show the whole (well-known) formulation of cumulative distribution function.

Equation 2: Leave the subscripts and add the formulation of the cumulative distribution function p=...=int((1/sigma*sqr(2*pi))*exp(-(x-mu)**2/(2*sigma**2)))dx.

Equation3: Define the quantity 'k' - number of observations? Are 'alpha' and 'delta tilde' scalars? If yes, the inversion algorithms might only be able to determine the sum of both quantities. How do you separate the value of the sum on the two quantities? The same problem might occur in equation 6. 17: Is the mean of your residuals indeed zero, or is this a request of the least square model? You can simply check this. You might better use 'random error' or 'disturbance term' to define 'epsilon tilde'. 18: Define exactly which quantity is the explanatory variable and which one is the coefficient. 19: State in which section the mentioned explanations will be found. 19-21: The term 'predictive situation' is not a statistical/mathematical formulation. Use 'to make predictions .. to estimate/determine...'. You have to replace this at several places. 21: Rewrite this sentence, e.g 'In this case the permafrost probability p is therefore p=phi()'. Equation 4: It is unclear how this equation is derived. You might refer to other equations or introduce more details. Further, the equation seems to be non-conform with your permafrost definition (try theta=0°C). You might use '>= ' here.

p1428 1-6: This text rather fit to the section 4.1(Theoretical framework) or section 2. ('concept'). 12-13: Leave 'traditional', the reader could expect the probit model shown in Eq. 5 and a second one - the traditional probit model. 20: This sentence is confusing, you might use 'The relationship between the observed? temperature and the presence or absence of permafrost allows ..' 22-24: If you use a section 'Concept' you can leave this sentence. However, in any case you can leave 'which will later be specified'.

4.3 Integration of continuous- and binary-response models Heading: Use consistent terms for your models; rock/debris models or continuous/binary models. I seems that the model coefficients are also estimated within this section. Thus you might change the heading to 'Integration and parameter estimation of the rock- and debris model'. 5-9: Use 'For ..' instead of 'In our case..'. Avoid to use that much quantities. Write '..two models Mr (rock surface) and Md (debris surface) that are fitted separately.' and

leave the rest of the sentence. If the two models are fitted separately, is there at least one parameter that is inverted from both data sets? If no, why do you need to integrate these two models into one? You could then use two separate models and could easily use standard geostatistical methods (e.g. Kriging). 13-15: I did not understand what implication the parameter 'delta_d' has on the model. Do you use rock glacier velocities as discriminator for the presence of permafrost? I further expect that you are an expert for applying this statistical models. If 'delta_d' is not used in your study it should be omitted from all equations. However leave '..but represent an expert-defined adjustment term'. 17: Which are the same explanatory variables? 4.4 Scaling Issues p1430 8: Do you mean resolution or the grid cell size? 15: Use quotation marks for the term 'change of support'. 16: Why do you not apply well-known statistical methods such as Kriging? p1431 1: see comment on p1427 19-21. Leave 'ie. at locations..sites' 6-9: What do you mean with 'possible'? - 2x 15: How do you determine N? - Bian and Butler (1999) suggest to sum up to the range of spatial autocorrelation to reduce errors by averaging dissimilar units. Equation 13: The average might be representative if the spatial correlation length is low (high scatter, random behavior). In the second case I suggest to use either the median or Kriging. 1432 1: Why is this a 'conservative choice'? 5: Does your approach consider the Gaussian error propagation law? 14: see comment on p1427 19-21 4.5 Surface Types Do you have used one of this approaches. If not, you can omit this section. If yes, this should be explained in the 'data' section. 4.6 Model fitting and Assessment The heading should be more precise. At present the text involve a heading such as 'Model implementation and accuracy'. I suggest to add information on how to parameterize the model and to use as potential heading 'Model parameterization and evaluation'. The systematic order would be implementation, parameterization, and accuracy. p1433 1-4: This kind of sentence appears on various places of the MS, but is not necessary here. 4-6: How does the GLMM takes into account random inventory effects? Do you use weights for your input parameters? That rock glacier samples were taken randomly is also mentioned earlier and don't need to state twice. 7: Do you mean '..the penalized

quasi-likelihood method ..'? 8-9: see comment 1-4. 10-12: Use only one, consistent, term for the 'debris model'. I do not understand how the AUROC is computed or what it does represent (reference!). 'ROC curve' can be omitted if the explanation of AUROC is clear. 13-14: Do you also present results or AUROC's without the random inventory effect? I assume that you again mean the random samples taken from the rock glaciers. - In this case, the term 'effect' seem to be not adequate. 14: If the abbreviation 'cv' is not used again, leave it. For me it would be more important to see how the model adjust if all data are used. Did you also try this, and which values have your residuals. 15: You might write 'adapt', 'adjust', or 'focuses' instead of 'generalizes'. 21: see comment 10-12. 22: Simply write RMS, this term is well-known. 5. Alpine-wide permafrost model The heading should be more precise. The section mainly contains the model calibration but also an interpretation of results. I suggest to use the heading 'Model calibration' and an additional chapter 'Result' or 'Interpretation'. 5.1 Debris model 1-20: This text include information on the model calibration/parameterization and should be moved to such a section. 21-p1435-line 2: This text correspond to the analyses of coefficients of the final model and should be moved to an adequate section, e.g. 'Result' or 'Interpretation'. p1434 5: If the input data should be consistent use the centroids for all rock glacier. It would be still possible to take random values along this line. Table 1: The statistical values are not well aligned so that the reader can not easily follow the statistics. I suggest to use an additional row with the names of the statistical parameters and to remove them from the caption. If you show the quantiles it is also adequate to show the median value. e.g.

——————intact rockglacier–()—-relict rockglacier—() ——————mean–median–Q25–Q75 —mean–median–Q25—Q75 Altitude (m)—999—-999—999–999—-999—-999—-999—999

11: Table 2 shows three different models that were not introduced up to now. This could be done in the section 'concept' or at the beginning of section 5. Why do you introduce exactly three models and how do you constrain your final model? Therefore

you have to use a consistent methodology. 12: Provide a reference for the Wald-test or briefly explain how it is derived. 14: Better is 'for' instead of 'from'. 15: I do not understand this sentence.

22: Leave 'empirical'.

Table 2: Put the term 'in parenthesis' in parenthesis. Are the three models potential debris model? I prefer to simply write 'residuals' or 'errors' instead of 'goodness of fit'. State the units for the explanatory variables. I do not understand the term 'inventory-level? standard deviation'. You might use 'sd' or 'std' as abbreviations for ' standard deviation'.

Table 3: Use 'Summary of statistic parameters and ..'. The values are not well readable and should be aligned to columns - see comment on table 1.

5.2 Rock model 4: 'top left' instead of 'left' 5: 'top right' instead of 'right' 7: It is not clear what the AIC criterion is. Rewrite this sentence and give a reference for this criterion. Further, which parameter shows the insignificance? For me the AIC seems to be no good criterion since it shows almost the same values for the three models. In general there seems to be no effect to the residuals in the 3 models although Intercept, MAAT and SEASONAL changes significantly. Why do you fix PISR? You write at the beginning that this parameter is import to know (this parameter causes the scaling issue).

5.3 Scaling model and model combination 20: The sentence should end here. 24-25: leave the '-' in the parenthesis. p1436 1:Which model do you mean? - The linear regression in Eq.16, or the rock model? 5: State the unit for the deviation. Table 5: State again the units for your quantities.

6. Discussion 6.1 Use and limitation of the model 23: Do I need an alpine-wide permafrost model for regional application? 25-26: Note also other methods such as the Kriging or the Minimum Curvature Gridding. p1437 6: Explain why a large data set that

is used for model calibration is not effected by variations in the elevation data. 7-9: This is previously stated and is obvious. Leave it. 10-13: This is previously stated, leave it. 15-17: I expected that this was done in this study. If it is not possible to do this you should discuss the impact of the adjustment offsets delta on the model result.

6.2 Influence of precipitation p1438 15-19: This should be explained earlier. How large are the uncertainties in the PRECIP data? - 30%, or more? How does this uncertainty effect your result?

7. Conclusion p1439 This section reads such as a summary and lacks in scientific findings. I recommend to completely rewrite this section and address the aims of the paper. 11-20: see comment above. 14-18: I suggest that the model is also applicable with other data. Thus this information is not relevant for a conclusion. In contrast it is important to emphasize how sensitive your parameters are and to know which parameters are used for the two models. 21-22: This seem to be one of your results and should not be questioned. Simply state that you have found this relation. 24: Leave the parenthesis, this should be mentioned in an introduction. 25-p1440/9: see comment on line 11-20. In a scientific paper it is not common to give instructions how to run a model. p1440 10-18: This text reads itself like an outlook. Points 2,4,5 are obvious and should be omitted. 21-25: see comment above.

Figures Fig.1: Use a projection to avoid such large distortions. You might use UTM - zone 32. It is also necessary to add the names of the countries, optional the names of large cities.

Fig.2: I don't understand your comment on the random effects.

Fig.3: Add a plot for the relation: residuals vs. PSIR. For the figures in the lower panel draw in or state the mean and the standard deviation.

Figures 1,3,4,6 can be show up in grey colors.

10. References Bian, L., and Butler, R. 1999. Comparing Effects of Aggregation Meth-

ods on Statistical and Spatial Properties of Simulated Spatial Data, Photogrammetric Engineering & Remote Sensing, 65, 73–84.

Crawley M. J. 2005. Statistics: An Introduction using R, Wiley; 1st ed.,

Dobson, Annette J., 2001. An introduction to generalized linear models, 2nd ed.p. cm.âĂŤ (Chapman & Hall/CRC texts in statistical science series).ISBN 1-58488-165-8

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