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The Cryosphere Discussions

Interactive comment on "Permafrost distribution in the European Alps: calculation and evaluation of an index map and summary statistics" *by* L. Boeckli et al.

L. Boeckli et al.

lorenz.boeckli@geo.uzh.ch

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We thank reviewer #1 for his valuable comments, which helped us to improve the manuscript. The referee comments ('RC') are extracted and answered by the authors ('AC') below.

General comments

RC: The most important revision would include the addition of a figure showing the entire study with results applied to it.

AC: As reviewer #2 suggested we inserted a new figure in the revised manuscript showing the entire European Alps (see Fig.1 below).

RC: In general I find the paper is generally a little too long and could benefit from an introduction, which tells the reader to read the original paper first. I think if this is done the authors might be able to cut certain parts of the paper in order to shorten it and get to the results faster.

AC: According to this comment and to shorten the manuscript we removed subsection 3.1, which explains the derivation of the topographic and climatic variables and refer to the first paper (Boeckli et al. 2012).

Specific comments RC: In the introduction when speaking about regional permafrost models Bonnaventure et al. 2012, must be mentioned. Even though the geographic area is different the spirit of the reach is similar and this is worth noting.

AC: Done.

RC: 853 – line 10-15, the authors must compare this model to others that use probabilities (e.g. Lewkowicz and Ednie, 2004; Bonnaventure and Lewkowicz, 2008; Bonnaventure et al., 2012). I believe this is important that the authors have explained why the results can not be viewed as true probabilities however, the above empirical statistical models do so the question of how are they different must to brought out and explained.

AC: As mentioned in section 2.1 from the original manuscript we believe that the term 'probability' is misleading in the chosen modelling approach because of the underlying uncertainties and assumptions that are integrated in APIM. Further we think that 'true probabilities' such as used by the papers mentioned should be used with care

because there is no equivalent of 'true probabilities' in nature. Lewkowicz and Ednie 2004 relate the calculated probabilities to areal extent but this linkage is not obvious and would need further theoretical concepts. These generic problems are addressed in the section 2.

RC: 853 – line 25, also it should be mentioned that these models are equilibrium models of present permafrost conditions. A comment as to if they could be used for scenario based climate change might also be appropriate as the presented model focuses on 'big picture' ideas and could be brought to people outside the permafrost community who are likely to ask such a question.

AC: We agree with reviewer #2 that this is an important point and added the following phrase: '[...] and/or the thickness of the permafrost body for current climatic conditions.' The fact that our approach is not suitable for future prediction based climatic scenarios is already mentioned in section 8.3 of the original manuscript.

RC: 855 – line 25, what program was used to compute PISR? How long of a period of the C479 year was PISR computed for and with what level of cloud cover? Snow free period? Since the area spans more than one degree of latitude were several models created and pasted together? The Alps is a very diverse area and these are important attributes in the PISR calculation, this information must be provided.

AC: PISR is calculated using RSAGA (Brenning 2008) and the algorithm of Wilson and Gallant (2000). The calculation is done for one year with an hourly temporal resolution and clear sky conditions (100 % atmospheric transmittance). We used a maximal latitudinal extent of 1° (6 bands for the entire study area). The corresponding section was however deleted in the revised manuscript and replaced by referring to Boeckli et al. (2012).

RC: 856 – line 4, evidence needs to be provided that shows that a Surface Lapse Rate (SLR) of -6.5?C/km is appropriate. In addition the reference of Lewkowicz and Bonnaventure (2011) should be added in speaking about SLR above treeline. I expect that the value of -6.5?C/km is correct since the permafrost area being modeled is above treeline which would be consistent with the findings of Lewkowicz and Bonnaventure (2011). In addition the term 'urface Lapse Rate (SLR) should be used in literature of this context, as changes in temperature with elevation on the ground are different than free-air lapse rates. I believe this should be added in this paper as an attempt to encourage the use of this nomenclature throughout mountain permafrost modelling.

AC: Lewkowicz and Bonnaventure (2011) are using a method (equivalent elevation) to calculate variable surface lapse rates for low elevation areas that is below the tree line. Because in the European Alps, permafrost is expected to be absent below the tree line (except of some spots with isolated and extrazonal permafrost) the approach of Lewkowicz and Bonnaventure (2011) cannot be applied. In order to address the remarks on the term 'surface lapse rate', we decided to use the term 'surface temperature lapse rate' in the revised manuscript because we think this is most appropriate.

RC: 857 – line 21, Here I think something needs to be added saying that the model is not set up to examine permafrost which occurs in high vegetation or forested areas. In the Alps permafrost of this type is rare, but possible. It is however, very important in other geographic areas (Yukon, Alaska, northern Norway and Mongolia). I understand that this is not the case in the Alps where it is not a modelling priority however, please briefly explain why.

AC: We agree that this is an important point that should be addressed. Instead of mentioning this in the subsection 'Surface type' as suggested be reviewer #2 we added the following sentence in the subsection 'Uncertainties and limitations of APMOD': 'Isolated permafrost patches in densely vegetated areas and/or below tree (cf. Gruber

and Haeberli, 2009) are not considered in APMOD, but are of minor importance for an Alpine permafrost distribution map.'

RC: 862 – line 10, I do not believe that the classification system of used in the map (figure 4) and in A1 to overly easy to follow. The term 'permafrost only in very cold conditions' is a little strange and difficult to interpret. What exactly constitutes 'very cold conditions' I think this needs to be better explained earlier in the text and possibly consider revising the classification.

AC: We agree that the term 'permafrost only in very cold conditions' might be misleading and therefore changed this term to 'permafrost only in very favourable conditions'. Additionally we added the following sentence in the subsection 'Interpretation key for the permafrost index': 'The term 'very favorable conditions' refers to a situation (topography and ground characteristics) that locally modifies favorably conditions for permafrost presence.'

References

Brenning, A.: Statistical geocomputing combining R and SAGA: The Example of Landslide susceptibility Analysis with generalized additive Models, SAGASeconds Out, 19, 23–32, 2008.

Gruber, S. and Haeberli, W.: Mountain permafrost, In: Permafrost Soils, edited by: Margesin, R., Biology Series Vol. 16, Springer, 3344, doi:10.1007/978-3-540-69371-0 3, 2009.

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Interactive comment on The Cryosphere Discuss., 6, 849, 2012.

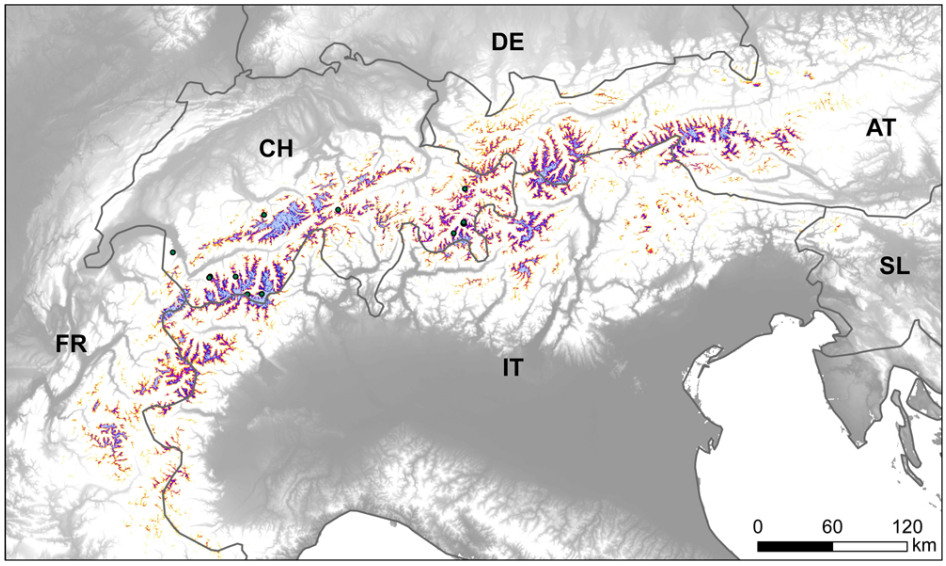

Fig. 1. Alpine Permafrost Index Map (APIM) shown for the European Alps (AT: Austria, CH: Switzerland, DE: Germany, FR: France, IT: Italy, SL: Slovenia). The map should be interpreted together with the legend