

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

Anonymous Referee #2

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General Comments:

This paper examines an important aspect in mountain permafrost modelling which includes the expanding of results and techniques to regional models. Regional models are critical in developing big picture ideas which are much more likely to get the attention of people outside the permafrost community. The paper builds on a study previously published in the cryosphere and although the two papers must almost be read one after the other to fully understand the concept, the manuscript represents an important step forward in modelling permafrost in the European Alps especially with respect to scale. As a result I would recommend the manuscript for publication pending

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ing minor (but critical) revisions, which I would consider essential. The most important of these revisions would include the addition of a figure showing the entire study with results applied to it. The entire time I was reading this paper I was anticipating this map and then saw that the model was only applied to a subsection of the area (Figure 4). If the authors are saying this procedure works through the Alps a map showing it must be presented. 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. The authors raise a good point about calibration data not being available and bring it to the forefront. I believe that this is a strength of the paper, however, I think that the authors should also stress that this is a model and point out the uniqueness of the end product (Map which need to appear as a figure).

Specific comments include:

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.

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.

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.

855 – line 25, what program was used to compute PISR? How long of a period of the

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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.

856 – line 4, evidence needs to be provided that shows that a Surface Lapse Rate (SLR) of $-6.5^{\circ}\text{C}/\text{km}$ 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 $-6.5^{\circ}\text{C}/\text{km}$ 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 Surface 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.

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.

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.

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