

Interactive comment on “Distinguishing ice-rich and ice-poor permafrost to map ground temperatures and -ice content in the Swiss Alps”

by Robert Kenner et al.

Anonymous Referee #1

Received and published: 11 February 2019

The manuscript presents a new country-wide permafrost map over Switzerland, which is based on statistical analysis of the national borehole-data network. During the mapping process, they distinguish between ice-poor and ice-rich permafrost, where the latter normally is associated to rock glaciers or talus accumulation, spatially often detached from the more continuous permafrost zone above in elevation. They used a multiple regressions approach to map MAGT for these two different zones.

The study is innovative as the permafrost types react differently on climate perturbations, and in alpine terrain ground ice has not been systematically mapped before. Thus, the study deserves attention. However, there are several issues which should be

[Printer-friendly version](#)

[Discussion paper](#)



resolved before considering publication.

TCD

In general, the paper is wordy, and can be shortened substantially. Tell the reader what you have done and avoid reporting-style. There were several parts I really did not follow even reading the passages several times. The following major points are identified:

1. Abstract. The abstract could be improved, what approach is chosen, what results are obtained which deserves attention?
2. Introduction: The introduction reads partly as a part of a discussion. An introduction should give the reader the background, not a discussion about what approach you have chosen. And it should end up in testable hypothesis, research questions or similar. This seems a bit mixed now, and should probably be re-formulated.
3. Methods: This chapter needs revision. To be honest, I still do not quite understand what the authors did in all details, and in which order. The description is full of report-style deviations in between substantial information. Sentences like "Attributing a MAGT to each thermistor is straightforward" have nothing to do in a scientific paper. Or what was "difficult" or not. I did not follow the handling of steeper slopes than 40 deg, in relation to PR calculations. Maybe a flow chart helps to describe the different steps. And: Give justification why you used different parameters, such like the selection of explanatory variables or why you consider points "5 times minimal distance of thermistors". Eq. 2: I understand you do a sort of interpolation, but I do not understand why and how you chose the factors. I do not follow p. 5, line 10 and following. The sensitivity tests: Nice, but now you mentioned more parameters included in the calculation of solar radiation?

Mapping of zone 2: Here you really need a flow chart, I cannot follow this, maybe some maps would help to illustrate the different steps, and a justification for e.g. the slope limits used, the size of the buffer zone around the runoff tracks etc. But as far as I could make out of it, you do an analysis identifying different types of mass wasting landforms (e.g. avalanche deposits, talus etc), do some manual editing and comparison to existing data sets, and then you do what to say that the landform is part

Interactive comment

[Printer-friendly version](#)

[Discussion paper](#)



of zone 2? Were there new regression for these areas, or you used only the regression values from zone 1? I see you mentioned later that if including the boreholes in zone 2, the performance goes down. Ok, but is there some calculation to identify zone 2 permafrost or only the mapping? Please clarify these parts. It would help if you show maps of how you classified zone 2, this does not need to be in the main text, ok within an appendix if possible.

4. Validation: Ok, but could be part of the methods section, does not need an own chapter.

5. Results: Ok, show a map to present the results, this is a mapping exercise, and it is good for the reader to see a map then. The validation is good for zone 1, does not work on zone 2. I would prefer error matrix analysis instead of the histograms (or in addition) against real observations, which also would provide a classification performance measure in addition to the regression-R2s. I am not sure if the comparison to other modelling results (Böckli, Gruber) is a “validation”. I think, no, validation is against something we know is true or false, the other maps are also models with their biases. You also show this again in the discussion (now with maps), the comparison should be a result.

6. Discussion: The first paragraph is more a result, as is the figure. Paragraph p.14, line 10 is repetition, the same first paragraph in 5.2. Please avoid redundancies in the manuscript. Finally, the discussion is very close to the Swiss conditions, and the comparison to earlier mapping approaches. A discussion should highlight and discuss the results to general science, which turns such a study from a technical report to a scientific contribution. Therefore, I miss comparisons to other areas, a discussion of the transferability of your approach, the evaluation of the use of other statistical approaches like GLMs or similar (see e.g. Hjort et al, recently in Nature Geoscience, or Aalto et al in GRL), the comparison of your model with such approaches.

7. Conclusions: The conclusions states in several places that your approach is a real

[Printer-friendly version](#)

[Discussion paper](#)



improvement, and this can be done in other regions. Probably, but then you need the same training data basis (which is exceptionally good in Switzerland), or are the regression coefficients universal? I do not think so. And if it is so, why did you not test to transfer your approach, I guess it would be easy to transfer it at least into other areas in the Alps? What is the last conclusion point? I think you may consider reformulation of the conclusions.

In summary, the paper needs major improvements to convince the reader that the presented approach includes a major step forward. I am sort of convinced that this is important, but the paper is hard to read and follow. Especially I am a bit puzzled around how zone 2 is treated in the final product.

Distinguishing ground ice content is normal in Arctic permafrost regions, but little considered on mountain permafrost, and only related to clear landforms such like rock glaciers or frozen peat plateaus. This knowledge has been part of permafrost models, which is, as far as I see, the major message from this study.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2018-235>, 2019.

[Printer-friendly version](#)

[Discussion paper](#)

