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The Cryosphere Discussions



# *Interactive comment on* "Transient thermal effects in Alpine permafrost" *by* J. Noetzli and S. Gruber

**Anonymous Referee #2**

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General comments:

The authors present a detailed study of idealised simulations of transient thermal and topographic effects on the ground temperature distribution in alpine permafrost. The paper is very well written and organised, the illustrations are clear and of very good quality, and the reasoning is sound. The topic of the paper is important and relevant not only for the mountain permafrost community, but also for researchers investigating the past, present and future ground thermal regimes in complex topography. The findings are very interesting and I thoroughly enjoyed reviewing this paper.

Apart from a series of specific comments (see below) and some minor technical corrections, there are some general remarks, which I would like to see addressed in a revised version of the manuscript:

1. In the abstract (l.6-9) and throughout the text the impression is given that the effects

of a future climate variability on the evolution of alpine permafrost is investigated. This is only partly true, as the simulations presented were made for very idealised conditions, and a simple linear increasing air temperature forcing is used for future climate simulations. I do not argue against the value of idealised simulations (on the contrary, they are extremely useful to investigate the dominant processes within a very complex system), but the scenario used may not permit to analyse "projected future ground surface temperature variations on the thermal state of Alpine permafrost...". There are many inherent uncertainties in this approach, which would have to be (but are not) addressed accordingly. I suggest to reword these paragraphs and reduce the connotation to actual predictions of permafrost distributions in the next 200 years (see also specific comments).

2. I miss a section on validation experiments or comparisons with ground truth data. Considering that you rely entirely on the capabilities of your model approach, evidence (and not only references) should be given that the model is generally capable to simulate e.g. present ground temperature profiles in alpine permafrost (you mention yourself that these ground truth data are available - why not present it here ?).

3. What is the reason for presenting the Matterhorn case study, even though (as far as I know) there are no ground truth data available on the permafrost distribution within the Matterhorn and there are no additional data on climate, surface and subsurface properties available ? I understand that the Matterhorn may be an intriguing "symbol", but in my opinion the results of this section do not add something significantly new to the findings already shown in the previous sections (the only difference is the usage of real topography). Besides, considering the uncertainties mentioned by the authors themselves and the lack of validation data, the results on the present and future permafrost distribution are quite speculative, but give the impression of being an accurate prediction.

4. You have to decide whether you are simulating (1) real alpine permafrost conditions or (2) idealised bedrock temperature response over long time scales. In the case of



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(1) it would be better to use only one temperature forcing and concentrate on realistic surface/subsurface characteristics, whereas in the case of (2) you may use idealised settings & detailed temperature forcings (as you did). But then, you should not overinterpret your results regarding the actual permafrost distribution and its evolution.

Specific comments:

Abstract: p.186, l.1-2: "...influences the occurrence of natural hazards...and has to be considered in construction practices..."

p.186, l-21-23: see general comment 3

Introduction: p.186, l.25 and throughout the text: consistent use of "Alpine" and/or "alpine".

p.186, l.26: regarding permafrost, a climate warming is not necessarily connected with a warming and degradation of permafrost, as the snow cover variability influences the thermal regime considerably. Rephrase (e.g. Its possible warming and degradation...)

p.187, l.17, 21 and throughout the text: denote references by the same author (Noetzli et al. 2007a,b, Gruber et al. 2004a,b,c) in the order of appearance (first 2007a, then 2007b).

p.188, l.15: "We first analyze..."

p.188, l.19-23: see general comments 1 and 3: rephrase. (e.g. "The results of these idealised simulations can be used to identify the dominant processes and their impacts on the subsurface temperature field. They should be seen as first step towards assessing natural..." and further "...we further present the modeled transient and three-dimensional permafrost distribution for the topographic data set of Matterhorn..."

Background and approach: p.189, l.3: Kohl & Gruber 2003 is only a 2 page abstract: is this the best reference to cite as "many studies..." ?

p.190, l.10: "The TEBAL model...calculates present mean ground surface tempera-



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tures (MGST) as a function of..." or "...by using..." - give slighly more details about TEBAL here.

p.190, l.14: the influence of snow is not mentioned here. You describe and simulate a very idealised case without snow, water infiltration through melting snow, surface wetting, surface heterogeneity etc,. I have no problems with that, but these simplifications have to be mentioned here.

p.190, l.17: "...down to about 12m bedrock...". Does that not depend on latent heat effects, water content, porosity, cracks etc ? This seems all too general statement (12m) for the whole variety of Alpine rock permafrost.

p.191, l.1-2: "...a typical value for rock slopes...": please include reference.

Energy balance and rock surface temperature

p.191, l.18-19: please provide the data here and the reasons for choosing them

p.191, l.20-21: a thin snow cover can change the thermal regime as well (e.g. albedo, melt water input...) ! Either state real reason (simplification purpose; "in a first step the snow cover was neglected...") or provide evidence that a thin snow cover has really only a small effect.

Heat conduction and subsurface temperature

p.192, l.13-16: please provide the equations for this approach

p.193, l.3: mW m-2 (per square metre) ?

p.193, l.6-7: "...as well as changing to smaller..." - rephrase

Evolution of surface temperature

p.193, l.12: "...Petit et al., 1999)

p.194, l.14: "...Ice Age ((1)/(2); numbers in brackets...)"



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p.195, l.8-9: What is the rationale for using this idealised scenario ? As this is the basis for all your simulations on future permafrost evolution, you have to explain in more detail why the approach is justified. (see also general comment 1)

Topography

p.197, l.1-5: see general comment 4: this is only an idealised permafrost simulation - it may not be justified to attribute the modelled 0° isotherm as permafrost boundary or referring to "permafrost thicknesses".

Subsurface properties

p.198, l.5-6: This sentence is strictly speaking only true for the idealised settings used for Fig. 5. Other processes connected with latent heat effects were not included, e.g. infiltration and refreezing, cracks, melting snow patches, latent heat processes at the surface (energy consumption) through wet surfaces. I suggest to rephrase: "For the idealised simulations shown in Fig. 5 energy consumption due to latent heat..."

p.198, l.13-17: Such detailed quantitative estimates are not given in Hauck et al. 2008 - besides, a weathered surface layer would also induce additional processes, as described in the previous comment (water fluxes). The effects of such a weathered layer would be better analysed with a high-resolution subsurface model.

p.198, l.28: "...respectively (Figure 7)." (Figure 7 is not referenced)

Effects of future warming

p.199, l.13: air temperature ?

p.199, l.14: "The results showed that after 200 yr the warming has penetrated..."

Topography

p.199, l.25-26: see general comments 1 and 4

Subsurface conditions



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p.200, l.21: indicate altitude of Schilthorn

p.200, l.23: "high ice-content": misleading; the ice content is not really high compared to other mountain permafrost sites - it may only be higher compared to 3% porosity bedrock!

Permafrost distribution and evolution in the Matterhorn

see general comments 1, 3 and 4! The title of this section is misleading - is this a prediction ? p. 201, l.22-24: see comment 1 and 4

Discussion

p.202, l.27: There are other variations of surface conditions that are neglected as well: snow patch distribution and associated meltwater transport, shading effects of microtopography, surface moisture...

p.203, l.27: "Idealised simulations of possible..."

p.204, l.6-7: "For the Matterhorn, for example, warm permafrost exists today in the middle of the southern side..." How do you know that ? Only because of your simulations with the inherent uncertainties mentioned by yourselves ? This statement is much too strong !

Conclusions

p.204, l.19: "...compared to stationary conditions..."

References

p.208, l.2: either include "PhD-thesis" or provide name of publication series.

p.208, l.21: "...geophysical..."

Figures:

Figure 1: in right grey box: "subsurface characteristics"



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Interactive Comment

Figure 10: solid line representing the no LH case cannot be identified regarding the modelled 0°-, -1°C etc isotherms.

Figure 11: in caption: "...cf. Fig. 10" ???

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Interactive comment on The Cryosphere Discuss., 2, 185, 2008.