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The Cryosphere Discussions

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Interactive Comment

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

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We thank both referees for their thorough reviews and valuable comments, which helped to improve the quality of the paper. In the resubmitted version, we have dealt with every comment raised and our responses as well as resulting changes to the paper are described below.

Reply to Reviewer 1 (M. Luethi)
General Comments

1. Permafrost – as well as ground temperatures in general – are often used as indicators or essential variables of climate change (e.g., IPCC, GCOS). However, in order to avoid a discussion on ideal climate change indicators – which we find interesting but far beyond the scope of the paper – we have changed the text in the abstract as follows: *In high mountain areas, permafrost is important because ... and because it sensitively reacts to climate change.*

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2. In our opinion, this comment addresses two different aspects: The first concerns the definition of permafrost, and the second is related to the question of a melting point depression at depth in the interior of mountains.

We have used (and now included in the introduction) the official definition of permafrost after, e.g., Brown and Pewe (1973) or Washburn (1979) (cf. also NSIDC: http://nsidc.org/sotc/permafrost.html). Permafrost is defined purely on the basis of temperature, irrespective of the presence or absence of ice. This is not necessarily congruent with frozen ground. What the reviewer probably addresses, is the fact that the difference of permafrost to non-frozen ground, and eventually the practical relevance of permafrost, comes with the ice contained in the underground. Further, a certain amount of unfrozen water can be present below the freezing point (and in permafrost), which is important for phase changes and geotechnical properties. We argue that it is important use the official definition, but agree the latter two aspects have to be considered in the modeling and the interpretation of the results (see Chapter 6).

The second aspect addresses the question of a freezing point depression due to overburden pressure. It can be assumed for bedrock permafrost that ice is mainly contained in the pore spaces and the overburden pressure is not directly affecting the ice at depth, but is absorbed by the rock matrix.

3. In order to double-check our results, we have compared them to independent simulations for the same geometries and settings using the model FRACTure (cf. Noetzli et al., 2007, Kohl and Hopkirk, 2001). In addition, our results are congruent with Mottaghy and Rath (2006) (cf. Figure 9). We have found two main aspects, why our results indicate a smaller long term effect of latent heat than shown by Luethi and Funk (2001).

The first concerns the geometry and distance to the surface: We modeled a situation in steeper topography, which results in a) shorter distance to the surface (i.e., ca 500 m in our simulations vs. ca. 1500 m), b) a smaller influence of the geothermal heat flux (Noetzli et al. 2007), and c) an acceleration of the pace of a warming signal penetrating

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into the subsurface (Figure 9). These factors reduce the effect of latent heat compared to the situation modeled by Luethi and Funk (2001). Further, we did not compare heat fluxes, but temperature fields. The latent heat effect visible in the temperature profiles by Luethi and Funk (2001) is not large, either.

Secondly, the surface temperature history considered for initialization is different. The exact temperature history is not given in their paper and it is therefore not clear, which surface temperature has been assumed at what time. However, Figures 9a and 9b in Luethi and Funk (2001) indicate 10 °C colder surface temperatures at 10 ky BP than at present. Based on available climate reconstruction studies, there is no indication to assume such low temperatures at the beginning of the Holocene. We assumed more or less stable climate conditions during the past 10 ky (Chapter 3.3, Figure 2). The results based on our initialization are therefore closer to steady state conditions and effects of latent heat (together with the transient effects) are smaller.

For these reasons, we did not change our text as suggested by the reviewer. But we have stressed that these results are valid for the depth scales and initialization procedure considered in our experiments. E.g.:
Energy consumption due to latent heat is of minor importance for low porosity material and for the time (i.e., millennia) and depth (i.e., ca. 500 m from the surface) scales considered in the experiments (Fig. 5).

4. We used to the commercial FE modeling package COMSOL Multiphysics (COMSOL AB, Stockholm, Sweden, see Chapter 3.2) for our simulations. Intensive verification of the heat conduction scheme has been performed by the software developers.

Concerning time stepping and mesh resolution we have performed sensitivity tests to assess their influence (see Chapter 3.2, lines 68211;9 on p.193 of the original manuscript). No significant influence on the results has been revealed. The validation of the model has been demonstrated by Noetzli et al. (2008) and Noetzli (2008). References and a short description of the validation are now included in the manuscript

(Chapter 3.2, cf. Comment 2 to Reviewer 2).

5. For the experimentation with synthetic geometries, the use of the TEBAL model is not preferable to an analytic expression for GST. For complex geometries and for studies in real topography, however, a thorough determination of surface temperatures is fundamental, since their strong spatial variability is the primary factor leading to lateral heat fluxes and 3D temperature distribution patterns in the subsurface. Here, an analytical expression is not sufficient. For consistency and in order to demonstrate the entire modeling procedure we have calculated GST based on TEBAL for all simulations presented. We have included this motivation in the text (Section 2.2).

6. It has been shown in several studies that the correlation of climate variables is significantly higher between sites of similar elevation than for sites with shorter horizontal but substantial vertical distance (e.g., Suter et al., 2002). Since the inner Alpine climate at the Corvatsch station is similar to that of the Zermatt region and the station is about 1000 m higher than St. Bernard, we have chosen this station. Further, the station at Grand St. Bernhard is located on a pass, which is not an ideal topographic setting to extrapolate to the high mountains and creates problems with horizon shading of solar radiation. We have explained this in the revised manuscript:
Surface temperatures were modeled using climate time series from the Corvatsch 8230; This station was chosen because it was shown in previous studies (e.g., Suter, 2002) that horizontal extrapolation of climate variables between high-elevation sites in mountain areas leads to smaller deviations than vertical extrapolation.

7. The effect of latent heat is handled based on an apparent heat capacity. We used the approach published by Mottaghy and Rath (2006), which is written on p. 192, lines 13–16 in the original text. Since the detailed description and derivation of the specific equations are published and accessible, we prefer not to repeat them in our paper. The parameter w is the temperature interval over which freezing occurs, as explained on p.

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192, lines 14–17 in the original manuscript. Since Mottaghy and Rath (2006) named the variable *w* and in order to be clear to what we refer, we prefer not to change the name. The values of *w* have been corrected to be in units of K.

8. A rectangle box of 2000 m height was added below the geometries to avoid effects from model boundaries (see p. 192, line 29 in the original text). That is, the lower boundary condition is set at 500 m a.s.l. for ridge geometries of 1000 m height (p. 190, line 27). Tests with lower model boundaries did not significantly change any results (i.e., resulting difference < 0.1 °C, cf. p. 190, line 27).

9. Model runs are started from a steady state and initialized according to the prescribed temperature history for the upper boundary GST (cf. Chapter 3.3). To be more clear, this is now explicitly mentioned in the revised manuscript (Chapter 2):
To account for the evolution of GST in the past, we initialize the subsurface temperature field based on a prescribed GST history using a steady state solution for the GST conditions at the start time.

10. In our idealized simulations we considered steep rock without any surface cover such as debris, snow, or glacier. In this case, projected warming of atmospheric conditions leads to a warming of the subsurface and to permafrost degradation. The simplification and possible effects from neglecting a possible snow cover are discussed in Section 6. We refer to Comment 4 to Reviewer 2 for further explanations.

Minor Comments

The manuscript has been changed according to the reviewer8217;s suggestions for all minor comments not specifically addressed below.

p.186, l.15: *Warming on shorter time scales* relates to the time scales of variations in surface temperatures during the past millennia (as stated in the previous sentence).

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Larger temperature variations on shorter time scales lead to a subsurface temperature field that more strongly deviates from stationary conditions (i.e., a higher rate of warming). According to climate scenarios, this can be expected for future temperature fields in high mountains and is, in our opinion, important and demonstrated in this paper.

p.187, l.20: We have included the reference to Wegmann et al. (1998) at this place. This paper explicitly treats thermal conditions in an Alpine ridge, was published before Wegmann and Gudmundsson (1999), and does not extend the already long reference list.

p.189, l.6: The term temperature depression is used to describe the transient signal of actual subsurface temperatures compared to a stationary field, which originates from former cold periods. We adapted the manuscript in order to make this more clear:
Many studies point to significant temperature depressions in the deeper subsurface, which are caused by past cold climate conditions (e.g., Haeberli et al., 1984, Safanda and Rajver, 2001). That is, actual subsurface temperatures are colder than for a stationary temperature field that corresponds to current climate conditions.

p.197, l.13/20: Plain is the correct expression for the landform, whereas plane is more often used in a mathematical sense. When speaking of the geometric form used (in the same way as *pyramid* for a peak), we have changed *plain* to *plane*.

p.198, l.19: We give more detail and the anisotropy factor used. Because the latter is clearly defined from the text we do without writing the thermal conductivity tensor.
In a first run, the thermal conductivity was set to 2 W K8211;1 m8211;1 in x-direction and to 3 W K8211;1 m8211;1 in the perpendicular z-direction direction. For the second run, the vertical and horizontal thermal conductivities were swapped (i.e., the anisotropy factor was set to 0.66 in the first and to 1.5 in the second run).

Reply to Reviewer 2 (anonymous)
General Comments

1. We generally agree with this comment. The intention is not to make actual predictions of permafrost evolution in the next 200 years, but to investigate and describe the dimensions, principal effects, and general patterns how subsurface temperature fields in steep mountains react to changes in surface temperatures in the dimension/scale that can be expected for the coming centuries. We have rephrased corresponding paragraphs in order to reduce this connotation (cf. also specific comments).

2. A short section on the validation of the modeling procedure is included at the end of Section 3.2. Because this validation is discussed in detail in other publications, we have summarized the most important points: *The modeling procedure bases on the coupling of the distributed energy balance model TEBAL with subsurface heat conduction simulated in COMSOL. Because measurements of entire three-dimensional temperature fields in mountains are hardly feasible, three different validation steps have been conducted in order to gain confidence in the modeling results (Noetzli 2008). They include: (1) comparison with field data measured at or near the surface, (2) comparison with temperature profiles from boreholes, and (2) sensitivity studies. For (1a) and (1b) we refer to the detailed descriptions and results given in Gruber et al. (2004b), Noetzli et al. (2007), Noetzli (2008), and Noetzli et al. (2008). Sensitivity studies to assess the uncertainties and influence related to the lack of information on subsurface properties and the surface temperature history are part of this paper.*

3. Yes, the main difference of the Matterhorn section to the numerical experiments with idealized topography is the use of real topography. However, we think that an application to real topography adds the following two aspects to the paper:
A) It demonstrates that the principal pattern and effects of the idealized geometries presented are the same for real topographies and that they are not fundamentally different. This is important when results from numerical experimentation are interpreted

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and transferred to nature. Further, we consider an application to real topography very helpful for the intuitive understanding of the dimensions of changes and the implications of results.
B) Based on the validation of the modeling procedure (see Section 3.2), which also includes comparison with field data, we trust that the modeling results reasonably represent the characteristic of the subsurface temperature field of the Matterhorn for the areas not influenced by glacier or significant snow cover (i.e., the upper and southern part). The modeling procedure used helps to understand transient 3D temperature fields in mountains without having very detailed information, which is only available for a very limited number of sites. Such an application, of course, constitutes a first guess and is not intended to be an accurate prediction. It must be interpreted with great care. Corresponding text passages (Section 5) have been reworked to make the above two points more clear and to stress the limitations of this application (cf. also specific comments).

4. As mentioned above (Comment 1), we intend to systematically investigate the principal effects in transient 3D temperature fields in mountain permafrost. That is, we simulate (2) idealized bedrock temperature response. We have rephrased corresponding paragraphs and tried to be more cautious concerning the interpretation and the transfer of idealized conditions to nature. For example, we added adjectives such as *schematic* or *idealized*, or quoted expressions to make clear that we do not speak of a real features or real permafrost bodies.

Specific Comments

The manuscript has been changed according to the reviewer8217;s suggestions for all specific comments not specifically addressed below.

p.186, l.21–23: Cf. Comment 3.

p.187ff: For the preparation of the manuscript we used the EndNote-Style for Coperni-

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cus Publications from the Website, where references by the same author are listed by the names of co-authors.

p. 188, l.5 *The results of such idealized simulations can be used to identify the dominant processes and their impacts on the subsurface temperature field and will contribute to our understanding of the three-dimensional distribution of mountain permafrost, its thermal state today, and its possible evolution in the future. They should be seen as a step towards assessing natural and more complicated situations. Results will also be useful to decide on the initialization procedure required for the modeling of permafrost temperatures in high-mountains. At the end, the model is applied to the topographic setting of the Matterhorn (Switzerland). Results from idealized geometries are compared to this first example of real topography, and possibilities as well as important limitations of the model application are discussed.*

p.189, l.3: The reference has been deleted.

p.190, l.10: We inserted two sentences describing the main features of the model. In order not to repeat text further below, we refer to Sections 3.1 and 3.2, where more details and citations on the model are given.

p.190, l.14: *We applied different GST histories, which we compiled based on published changes in air temperatures and the simplifying assumption that GST follow these changes closely. That is, effects from changes in other components of the surface energy balance, snow cover, or surface characteristics are neglected.*

p.190, l.17: We intend to give an impression of the depth scale of the seasonal variations in bedrock rather than an exact number. We concretized the expression to solid and dry bedrock, and give a range for the ZAA rather than one value:
In this study, we ignore seasonal temperature variations, which may penetrate down to about 10–15 m in solid and dry bedrock (Gruber et al., 2004a), and only...

p.191, l.18–19: As suggested by Reviewer 1, we have included a table providing the values for surface and subsurface characteristics and corresponding references. Values were chosen based on published literature and typical values for rock.

p.192, l.13–16: The effect of latent heat was handled in the model based on the widely known apparent heat capacity approach. In our opinion this information is sufficient for the understanding of the paper. Cf. Comment 7 to Reviewer 1 for a more detailed explanation.

p.195, l 8–9: The scenario of +3 °C/100 y for rock surface temperatures has been chosen based on a study by Salzmann et al. (2007). In their study, a possible range of surface temperature changes and corresponding uncertainties were analyzed for steep Alpine rock walls, based on scenario climate time series downscaled from RCM results and the distributed energy balance model TEBAL. We have rephrased this text section: *Further, to asses the transient response of the subsurface temperature field to future warming, we used a warming of the rock surface of +3 °C/100y. This value has been calculated as a mean warming of Alpine rock surfaces from 19828211;2002 to 20718211;2091, based on output from different Regional Climate Models, different emission scenarios, downscaling methods, and varying topographic settings by Salzmann et al. (2007). Since no information on the form of the increase (e.g., exponential) was deducted and uncertainties are high, we chose a linear increase. Based on this simple scenario, the principal effects can be demonstrated.*
In addition, we come back to this point in the discussion section.

p.197, l.1–5: We have rephrased this part in a more cautious way and speak of a schematic permafrost boundary to make more clear that idealized situations are considered: *... but the position of the 0 °C isotherm – which can be interpreted as the schematic permafrost boundary – varies (Fig. 5). The corresponding difference in permafrost thickness in the simplified ridge (which we consider vertical to the surface) between...*

p.198, l.13–17: The corresponding sentences have been changed according to the reviewer8217;s remarks and an additional reference (Scherler 2006) has been added. *However, other processes than conduction and phase change are not considered and are likely to play an important role in such a weathered layer (e.g., Scherler, 2006).*

p.199, l.25–26: *For the idealized ridge geometries considered and for all elevations, no below zero temperatures, or "permafrost", remain at the surface on the southern slope. Nevertheless, a significant "permafrost body" remains below the surface for a long time, especially for higher elevations. For lower elevations a "permafrost body" remains only on the colder side.*

Section 5 Is now entitled: Application to the topographic setting of the Matterhorn

p.201, l.22–24: *For present-day conditions and all the simplifications assumed for the simulation, the entire mountain is within permafrost, except for the lower parts of the South side. Considering the calculated scenario, in contrast, surface temperatures on nearly the entire South side would be positive after 200 years. On the North side, the 0 °C isotherm at the surface would have risen to an elevation of about 3500 m a.s.l.*

p.202, l.27: *...This mainly concerns glacier coverage and the influence of the snow cover.*

p.204, l.6–7: *For the idealized present-day temperature field of the Matterhorn...*

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