Final response to

tc-2010-60

Modelling past and future permafrost conditions in Svalbard

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We want first of all thank all the reviewers for relevant and constructive comments. Our reply is cumulative in this document, covering all three referees. Obvious grammatical or syntax errors are changed without further comment.

Reply to referee #1 (Haeberli)

Comment on title ("The title could be more precise").

We change the title to

"Modeling the temperature evolution of Svalbard permafrost during the 20th and 21st century"

Comment: The final paragraph on possible consequences is an (unavoidably somewhat vague) outlook rather than an integrative part of the modeling exercise. It could be more clearly marked as such and the recommendation for more in-depth investigation of the involved complex processes could be strengthened.

The paragraph is not absolutely necessary for the overall message of the paper. However, it relates the presented results of the study to their major use, addressing possible geomorphological responses to permafrost temperature changes. We insert a new heading, "Geomorphological implications" to separate the paragraph within the discussion.

Abstract, line 5: better write ". . .project possible future ground temperatures".

Done

Page 1878, line 17: better use a term like "expression", "effect", "consequence" or so. Changes in permafrost temperatures are not straightforward "indications" of climate change but the result of highly complex atmosphere/ ground interactions, strongly influenced by a variety of environmental aspects, buffer effects and feedbacks at the surface and in the active layer.

We used "consequences"

Page 1879, lines 1 to 3: Vegetation can have a predominant effect, even though perhaps not in Svalbard, and should be mentioned.

We added the term "vegetation". Vegetation is shortly discussed later in the paper.

Page 1880, line 16: What is the annual precipitation at Isfjord Radio?

Inserted: “480 mm in average between 1961 and 1990”.

Line 9: Better write “ . . . and the beginning of the 21st . . .”.

Line 10: Better write “ . . . empirical-statistical downscaling . . .”.

The paragraph about the down-scaling routine is extended as response to the comment by review #2, see below..

Page 1882, lines 1 to 3: how about the influence of grain size in the active layer (this has an extreme effect in rock glaciers or mountain-top detritus, etc.)?

Yes, this is of course correct, we assume that “land form types” cover implicitly the grain-size distribution. We changed to “Landform/sediment types”, to keep things short, but more informative.

Page 1883, line 14: Add "(cf. explanation on page 1885)" where a more precise definition is provided about the depth scale considered.

Done

Page 1886, The sentence under 3. has no end (eliminate “for” and make full stop?).

The word “melting” was missing, included.

Page 1887, line 9: as only temperatures are calculated it may be more careful to write something like “ . . . permafrost conditions continue to exist until 2100.”

Accepted.

Page 1888, line 28: The authors are, of course, free to believe whatever they want. But what scientific argument exists for assuming that “the median of the . . . is the most reliable indication”? Would it be more appropriate to say something like "the scenario with the highest probability" or "the most realistic estimate" or so? Or perhaps just leave it as it is – the median is the median and everybody can have his/her own interpretation about probabilities?

Changed into the following sentence:

“We suggest, that the median of the resultant temperature distribution is a realistic indication of the future evolution given the used temperature scenarios.”

Page 1889Line 12: This may be oversimplified – a thinner snow cover also has a tendency to disappear earlier in springtime and thereby to let the subsurface warm earlier and during a longer time interval.

We included the sentence:

“On the other hand, – a thinner snow cover also has a tendency to disappear earlier in spring time, possibly leading to subsurface warming earlier and during a longer time interval.”

Line 20: Is the (correct) idea that spatial heterogeneities are strongly smoothed at depth, because lateral heat exchange tends to have the effect that temperatures at greater depths integrate over larger surface areas?

Yes, this is what we meant. New sentence here:

“Deeper ground temperatures in turn will tend to be more similar because of lateral heat transfer which have the effect that temperatures at greater depths integrate over larger surface areas.”

Page 1891, line18: The relation of the last sentence in this paragraph to the explanations in the preceding sentences is not clear. What exactly is meant with “This” at the beginning?

Thanks for pointing this out. We have moved this sentence to line 1, where it probably should have been in the first place

Table 1: The values for the water content are very low, do they relate to the active layer only?

No, these values are estimated during the calibration process, and refers to total volumetric percentage of water/ice. Most sites in this study are in bedrock or have only a low and coarse-grained cover, where water/ice content is relatively low (except the Endalen site). The values in the tables refer to the layers defined in depth in column 2 of table 1.

Reply to referee #2 (anonymous)

We have made the minor corrections and included elevation of the official met-stations (all close to sea level).

In section 3.2 please give a bit more information on the empirical statistical downscaling of the GCM's by Benestad (2008). I think this will provide context that is needed to make the reading of the paper better.

We enlarged the description of the down-scaling procedure considerably, to make it more informative for the reader (see manuscript, paragraph 3.2.)

In section 3.3 the authors should indicate what the current measured lapse rate in the areas is in C/km.

We put in a sentence about lapse rates at page 1880, l. 18, as follows:

"The mean annual lapse rates between the Longyear valley bottom (app. 60 m a.s.l,) and the Gruvefjellet meteorological station (464 m a.s.l) was 0.0062 \,$^\circ$Cm$^{\rm -1}$ (2001-2010). There are major inter-seasonal differences, with lapse rates ranging from +0.01 \,$^\circ$Cm$^{\rm -1}$ during end of winter/early spring (May-June) to -0.01 \,$^\circ$Cm$^{\rm -1}$ during winter inversions."

In the discussion there are more comments on lapse rates on page 1890 line 2, at this point a reference should be added. Also is there any data on lapse rate differences between winter and summer months? Also please comment on how you believe lapse rates have changed through time, and what impact will the changing of lapse rate have on ground conditions.

Se above comment about lapse rates in section 3.1.

We believe that lapse rates are depending on sea ice cover and changes with it. We state in the text (page 1980):

"Finally, the SAT-GST relationships employed here assume a constant lapse rate between Svalbard Airport and the study sites of 0.0065 Cm^{-1}. The mean annual lapse rates during our measurement period in this study was 0.006--0.007 \,$^\circ$Cm$^{\rm -1}$ between Svalbard Airport and the stations on Janssonhaugen and Gruvefjellet (average 2000-2010 was 0.0062 \,$^\circ$Cm$^{\rm -1}$) , respectively. However, lapse rates are not constant over time and especially at high Arctic coastal sites highly depending on sea ice conditions and the local air flow conditions. Svalbard Airport is situated at the coast, and its temperature especially during winter is largely affected by sea ice cover. The station is highly sensitive to the coupled sea ice-ocean-atmosphere system (Benestad et al., 2002) and recently observed shrinkage in Arctic sea-ice cover (Vinje, 2001; Stroeve et al., 2007) suggests that larger differences may be expected further inland e.g.~at Endalen, Gruvefjellet and Janssonhaugen today than previously (O.~Humlum, personal communication, 2010)".

Reply to referee #3 (anonymous)

Obvious grammatical or syntax errors (technical comments) are changed without further comment.

It would be interesting to include in section 3.3 Ground Temperature, a more thorough description on the N-factors employed in this study. It would be valuable to see the period of time over which the ground surface temperature and air temperatures were collected on which the N-factors used in this study were calculated. If they have been calculated using more than one year of data what is the range of values?

We added the following text (pg. 1882, l. 18):

"We calculated the ratios of annual sums of freezing or thawing degree days of GST to those of SAT, referred to as n-factors $n_{\rm T}$ and $n_{\rm F}$, respectively (e.g.~Smith and Riseborough, 2002). The n-factors were calculated over the same period as the ground temperature measurements, which is >10 years for the Janssonhaugen site (2000-2010) and >2 years for the other sites (2008-2010). For Janssonhaugen the n-factors were practically constant, with variations of 1.05 to 1.07 during freezing and 1.12 to 1.19 during thawing. For Gruvefjellet and Kapp Linné a similar near constant value was calculated (Kapp Linn\'{e}~1.00, Gruvefjell~1.18 for the thawing factor, $n_{\rm F}$ was between 0.95 and 1). For Endalen, the freezing factor in 2009/2010 was slightly lower (0.78 to 0.74), while the thawing factor increased from 0.83 to 0.93. Hence, for the non-vegetated sites, the thawing n-factor $n_{\rm T}\ge1$, characteristic for equal or warmer summer conditions at the ground surface than at screen level (2\,m above ground). At the Endalen site, $n_{\rm T}$ was considerable lower, equivalent to lower summer GST than summer SAT. As the freezing n-factor ($n_{\rm F}$) depends mainly on snow cover, $n_{\rm F}<1$ indicates a weak coupling between GST and SAT due to the insulating effect of the snow cover. Here, the observed inter-annual variation of the freezing factor will of course depend on the snow cover timing, duration and quantity. For the more open wind-exposed sites, little changes in the future might be expected".

Comments on figures: The majority of the figures are well drawn and easy to understand. The only figure that requires any modification is figure 1. The general layout of the map is good but the titles and the symbols are tightly packed together making them difficult to understand. In addition, some of the place names are printed below these symbols; it would increase the value of the map to spread out the place names.

New Figure 1 is inserted into the manuscript, taking into account the above comments.

Pg 1883 line 14; is there a reference for the geothermal heat flux value?

Done

Pg 1885 line 14; instead of using "Past Development" as a section heading 4.1 a more specific title could be "Historical ground thermal regime" or something similar.

Accepted

Pg 1886 line 3; section 4.2 could use a slightly more focused title.

Changed to “Future ground thermal regime”

Pg 1887 line 14; a more specific heading could be “Sensitivity to changes in seasonal air temperature”.

Accepted

Pg 1891 line 7; does temperature variability refer to air temperature?

Yes, added.