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Reply to Reviewers:
Past climate changes and permafrost depth at the Lake El'gygytgyn site: implications from data and thermal modelling

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First of all we want to thank both reviewers for his detailed and knowledgeable reviews. Below our detailed replies to the items raised in this review.

Reviewer #1

General comments

The authors try to estimate how an uncertainty in the thermal conductivity might effect the steady-state solution, see figure 5 in the presented study. At the same time, there is no mentioning of potential errors in the transient solution due to uncertainty in the thermal conductivity. Besides the thermal conductivity, the transient solution also depends on the porosity and unfrozen water content curves. It is suggested to conduct some appropriate sensitivity studies. It would be very interesting to see how the uncertainty in the porosity, thermal conductivity, heat capacity and unfrozen water content might influence the results on the presented Monte Carlo simulations.

In a revised version of this manuscript we have added a new figure showing the sensitivity to the choice of porosity in layers . In a sense this also determines the other parameters, as with porosities $>.25$, the properties of the water content strongly affects all other properties mentioned.

Specific comments

p 2610, line 24, "...the temperature distribution even in the upper surface." Probably you mean "in the upper layer of the ground material." How thick this layer would be?

In our opinion it makes no sense to speak about a particular layer. We have reformulated this sentence to improve clarity: "...temperature distribution at all depths up to the surface."

p 2613, line 26, "... the freezing range can be adapted ..." Which unfrozen water content curves were used in the present modeling? A plot might be beneficial. Do you take into account salinity of the ground material? Do the modeling results depend on the assumed values?

For this study we used the simple smooth curve $\Theta(T)$ proposed by Lunardini (1981, 1988), with a liquidus temperature T_{liq} of 0. °C, and a half width of 14 K as shown in Figure 1. The particular form of this curve does not play a role here because it mainly affects the width of the domains where freezing or thawing occurs. This width is overestimated using this particular function compared to most of the functions commonly applied in active layer studies (e.g., Nicolsky et al., 2009; Galushkin, 1997, also shown in Figure 1). In order to minimize the influence of the particular form of the unfrozen water content function on the position of the boundary of the permafrost layer, our histogram of permafrost depths is based on the liquidus temperature T_f, and thus represents the upper limit. There is no freezing or thawing in the uppermost subsurface on the time scale studied, due to the pertinent subzero annual air temperature for the time period of interest. However, if we need to simulate the annual wave in the top 20 m, a better representation (e.g., Nicolsky et al., 2009) would be required. Salinity was not included due to lack of available information.

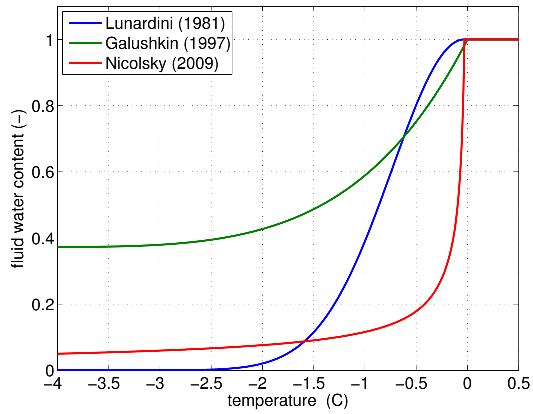

Fig. 1. Smooth functions describing the fluid water content in the freezing range. For the particular form of $\Theta(T)$ used in this study (Lunardini, 1981), we assumed ω = 1 for the width of the freezing/thawing zone, and a liquidus temperature T_{liq} = -0.04 °C. In addition, we plotted the functions proposed by Galushkin (1997) for clay-rich material, and by Nicolsky et al. (2009) for silt. The temperature derivative of the latter formulation shows a singular behavior near the liquidus temperature, and is thus not well suited for our implementation of the enthalpy method (see also Nicolsky et al., 2009).

p 2614, line 9, "... as well as literature data." Could you please provide some references?

We have added the references in the revised manuscript (Popov et al., 2003; Mayr et al., 2008). This refers to the impact-affected rocks.

p 2615, line 1-3. As I understand values of the thermal properties are estimated either from the laboratory experiments or literature. Here, you state that you adapt the values later in order to obtain a good fit. Do you simultaneously try to estimate the thermal properties and the temperature history (specifically during the LIA and later)? Please clarify.

This sentence was indeed not clear and even had a wrong reference to section 5: we have added an explanation and refer to the correct paragraph, thermal conductivity was slightly modified in order to fit the measurements in the deepest part of borehole 5011-1 using steady state simulations and not transient simulations.

p 2617, line 19. "... from the literature." The reference is missing.

We have added the references in the revised manuscript (Popov et al., 2003; Mayr et al., 2008). This refers to the impact-affected rocks.

p 2618, line 14. "... is estimated to be 14K, which is in agreement with the results of other studies." Could you please clarify how, the value of 14K is obtained? The references are missing. Is it agreement with Melles et al., (2012).

Here we refer to the general results of Miller et al. (2010). We have clarified this in the text. The Results of Melles et al. (2012) are not directly comparable, as they only refer to maximum summer temperatures, while our estimates would be relevant to mean annual temperatures. However, the value of 14 K determined from our modeling is considerably higher than the ones determined by other authors (Schneider von Deimling et al., 2006; Schmittner et al., 2011; Annan and Hargreaves, 2012). There are some caveats related to the the results obtained in this study. What can be determined from the observations in 5011-3 is the temperature gradient at the deepest part of the borehole. This gradient, however, depends on the basal heat flow assumed, and the thermal conductivities. As has been shown by Rath et al. (2012), at shallow depths the effect of postglacial warming can be approximates by a linear behaviour of the temperatures, i. e., an additional contribution to heat flow. From borehole 5011-3 we can not differentiate between the climate and non-climate contributions to the temperature gradient, and heat flow as well as thermal conductivities are uncertain. Can we constrain the problem otherwise? Assuming that the Lake has been isolating the subsurface rocks from the atmosphere during the period of interest, we have tried to model the temperature measured in 5011-1 below the lake as a steady state profile. The assumptions on heat flow density and thermal conductivity made in the text led to the best fit of these date. Though not rendering the solutions unique, this is an argument for the assumptions made in this study. It has to be kept in mind that we simply do not have better constraints because of the lack of independent observations. We have clarified this in the revised manuscript.

Figure 6, What type of the solution is shown? Is it the present day temperature distribution?

The results in this figure are based on the steady-state simulations, we have modified the caption accordingly.

Reviewer #2

General comments

Major comment: The authors use a 2D-model to describe a 3D-situation. For this to be appropriate, the heat flux in the third dimension must be negligible on the considered timescales. This is fulfilled if the 2D-section with the prescribed thermal properties continues unchanged in the third dimension for a distance much larger than the extent of the modeling domain in the two other dimensions (in this case the lake would be an elongated trench or channel). However, the lake surface is more something like a circle, so that the required translational symmetry in the third dimension is clearly not given. Judging from the form of the lake surface, one should rather make use of the cylindrical symmetry of the problem and numerically solve the cylindrical form of the heat flow equation for a thin ”cake slice”. In such a work, the detailed stratigraphy of the different layers, as given in Fig. 2 can not be as easily incorporated as in the 2D-section presented in the manuscript, since it is clearly not cylindrically symmetric. But this could only be incorporated in a full 3D-simulation, which is computationally prohibitive. One could argue that the lake surface is not too far from a square, so that the translational symmetry assumed by the authors is not too much violated. This, however, will strongly depend on the bathymetry and the distribution of thermal parameters, and the authors would have to present evidence for this. I am not sure about how much the presented results would change if the cylindrical form is used, since this will depend on the exact domain and time period. In any case, the authors must present additional analysis to show that their results are valid under these geometrical considerations. For instance, simulations in both symmetries could be presented as two constraining cases for the problem.

We agree that the issue about the differences between 2D and 3D model results has to be discussed. However, in order to estimate the influence of 3D geometry we did not compare with a 2D model of cylindrical symmetry, mainly because this would have required serious changes in the modeling code. Instead, we set up both, a simplified 3D model and a corresponding simplified 2D model and added a paragraph describing these additional simulations as an appendix. From these we conclude that our 2D approach is justified for the aims of this study. It must be mentioned here that figures 3, 7, and 9 are greatly exaggerated in the vertical direction, suggesting different ratio between lake depth and lake extension. Thus, we added an explanation in the captions.

Specific comments

Abstract: The abbreviations GST, LIA, etc. should not be introduced in the Abstract

Done.

P. 2612, l. 21ff: The authors should explicitly state that the material was either fully frozen or fully thawed during the heated-needle-probe experiments. Partial thawing even at subzero temperatures directly around the needle would introduce large errors.

The measurements were performed ensuring that the material was fully frozen, in order to avoid the errors as mentioned by the reviewer.

P. 2613, l. 17ff: Was the thermal conductivity also determined for the frozen state?

No, these measurements were only performed at ambient temperatures. These rocks feature a low porosity ($< 10\%$), therefore the influence of the phase transition of the pore fluid is low. However, it is taken into account in the modeling code by the latent heat effect as described in section 3.

P. 2614, l. 23ff: What are the values for internal heat generation based upon?

They are based on the measured gamma-ray log and literature Rybach and Čermak (1982); Rybach (1988); Čermák et al. (1990); Beardsmore and Cull (2001). However, as mentioned and tested, this parameter is of minor importance in the shallow regime which is studied.

P. 2615, l. 6: Use "lower bound" or "lower limit" instead of "lower boundary condition"

Done.

Table1 + 2: Define all abbreviations in the table header.

Done.

Fig. 4: T5 instead of Temp_5

Done. Caption and figure now consistent.

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