The Cryosphere Discuss., 3, C593–C604, 2010
www.the-cryosphere-discuss.net/3/C593/2010/

The Cryosphere Discussions

Interactive comment on "Multi-channel ground-penetrating radar to explore spatial variations in thaw depth and moisture content in the active layer of a permafrost site" *by* U. Wollschläger et al.

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Received and published: 8 March 2010

Reply to the comments of both anonymous referees

We thank both reviewers for their thoughtful and constructive comments and valuable suggestions which helped us to improve the paper. In the following the reviewer's comments are summarized or abbreviated and printed in italics, followed by the author's replies printed in normal font.

Referee 1

(1) Difference to the study of Gerhards et al. (2008)

Gerhards et al. (2008) focused on the multi-channel GPR method and in particular demonstrated that estimation of water content and reflector depth demands multi-channel measurements. In the current paper, the focus is on the application of multi-channel GPR to explore the permafrost site and on deducing aspects of its hydrology. Still, methodological improvements are also presented for the GPR technique which we now emphasize explicitly in Sect. 3 of the revised manuscript:

(a) The survey was conducted using an eight-channel setup which provides twice as many data points for every single CMP gather compared to Gerhards et al. (2008). The availability of more channels is expected to stabilize the multi-channel evaluation accordingly and should be less sensitive to outliers.

(b) Spatial position and - even more importantly - topography were mapped in detail with the laser tachymeter which allows us (i) to efficiently conduct this kind of spatial survey and (ii) to spatially visualize the slope and variability in active layer thaw depth.

(c) In order to confine the air wave adaptation to a reasonable range, air wave travel times from the channels with short antenna separations were first picked and then fixed to constant values.

(2) The lack of ground truth, temperature, energy balance, precipitation and snow cover data severely restricts the interpretation of the data and the validation of the hypotheses. Use of additional data (even temperature data from the region, modelled radiation balance or satellite data concerning the snow cover evolution), comparisons with former surveys (e.g. Gerhards et al. 2008), comparisons with own or other model studies, references to previous work are missing.

We agree that the lack of additional data for interpretation is a disadvantage of this study. Unfortunately, further measurements of soil temperature and water content do not exist for this site. As microclimatic, microtopographic and soil textural differences may induce considerable variations in thermal properties, thaw depth and water content, which are also obvious from Fig. 6, information about soil temperature, water content and thaw depth measured at some place in the region may provide hints about general active layer conditions but cannot explain spatial differences like those observed at a plot of the investigated size. The same is true for satellite measurements which may be used to get a broad overview, of the whole region but whose spatial resolution is insufficient to reveal the small-scale variability provided by our GPR measurements. With respect to snow cover, we expect its influence to be negligible at our site since (i) precipitation is very low in this arid continental environment (see Sect. 2 of the original manuscript) and (ii) thin snow covers which may occur during single events will not be persistent due to the strong solar radiation at this low latitude and high elevation. This is an important difference to arctic permafrost sites. Still, we follow the reviewer's comment and will address it in Sect. 4.3 of the revised manuscript.

(3) The authors should either (a) extend the technical aspects of the paper (the multi-channel GPR) to a level not already covered by Gerhards et al. 2008 or (b) extend the discussion and validation of the case study on the Qinghai-Tibet plateau using additional data, such as temperature and snow cover data.
Please refer to our response to items (1), (2) and (4).

(4) The objective is fulfilled, but the resulting hypotheses presented in section 4.3 (Discussion) were not directly related to the processes mentioned in the Introduction.
We will completely revise Sect. 4.3 where we will exclusively discuss the observations from the measurement date in late summer. We will link our observations to the processes mentioned in the introduction and provide comparisons with other studies.

(5) p.924, l.21: The site is described as "extensive discontinuous permafrost" (Chinese classification), but as "continuous permafrost" in the Introduction.
The statement was indeed confusing. We have replaced it with information from Fig. 1

of Jin et al. (2000): "The site is situated within an area of continuous permafrost (Jin et al., 2000)".

(6) p.924, l.25: "earlier measurement campaign in summer 2006": in Gerhards et al. 2008 the authors state that the measurements were conducted in early October 2006... is this still summer or was a different survey considered?
The survey described in Gerhards et al. (2008) was in early October 2006. We aplogize for this mistake and corrected the text accordingly.

(7) p.924, l.27: was this soil profile excavated during the 2006 study or in 2007 ? Why was this kind of ground truth data not used as validation for the soil moisture & thaw depth data in 2007?
The profile was excavated during the 2007 study which we now state in the revised manuscript. It provided the information about the presence of groundwater at this site. We use this observation in the discussion. From the GPR measurements (Figs. 4 and 6) we observe that the thaw depth varies considerably across this site. Since the soil profile was not located in the measurement area, we cannot use this information to verify the GPR measurements.

(8) p.925, l.26: How did you assess that the thaw depth of the active layer was close to its deepest position on August 31st 2007 ? In Gerhards et al. 2008 the same authors wrote that "Measurements were done in early October 2006, when the permafrost table was near its deepest point", which seems like a clear contradiction! Please provide references, data from previous years or from comparable locations to support or explain this statement.
We have inspected soil temperature data (unpublished) from our nearby monitoring station located in the Tianshuihai Lake region (79°33.148'E, 35°24.177'N, 4739 m a.s.l.). The station was set up after the GPR survey at the beginning of September

2007. Data from summer 2008 indicate that from beginning of August until end of September, the 0°C isotherm in the inspected profile (bare surface, predominantly sandy sediments, similar atmospheric forcing) was almost constantly at about 1.6 m depth. Similar conditions were observed in September 2007 as well. Thus, the statements in both papers are presumed to be correct.

(9) p.929, l.14: epsilon_a is not used in equation (3)
We have rephrased this sentence to ..."where ε_c [-] is the composite dielectric permittivity of the soil, composed of the volume fractions of solid matrix ε_s [-], water ε_w [-], and air ε_a [-], respectively"... and added the CRIM formula with all its terms to the manuscript (now Eq. 3).

(10) p.929, l.19: I do not understand, why the porosity was assumed constant in equation (3). An analysis to what extent the calculated soil moisture/thaw depth variability could also be due to varying porosity (with similar absolute moisture content) would be very interesting.
The calculated thaw depth variability is not affected by the assumed porosity (cf. Eq. 1 in the manuscript). Porosity comes into play when transferring the inverted dielectric permittivities to water contents using the CRIM formula. For a better illustration of CRIM we now use Eqs. 3 and 4 as the new Eq. 3 better shows how the different phases and porosity compose ε_c. The uncertainty of the estimates for porosity and also dielectric permittivity of the solid matrix is now investigated by running the analysis with values ranging between 0.3 and 0.5 for porosity and 4 and 6 for ε_s. The uncertainty of both values on the water content estimates is discussed in the revised manuscript using the example transect in Sect. 4.1.

(11) p.930, l.10: "ground surface AND the longer..."
corrected

(12) p.931, l.10: Is it possible to quantify the level, where a signal is definitely above the (potentially) systematic noise level? One possibility would be to include an uncertainty range in terms of varying dielectric permittivity of the unfrozen active layer, or varying porosity, to indicate where the soil moisture/thaw depth signal is definitely above the noise level.
We have reformulated section 4.1 and now discuss explicitly the influence of statistical noise as well as the influence of porosity and dielectric permittivity of the solid matrix on the measured water contents.

(13) p.932, l.22: unclear wording: what phenomenon ("which") is not able to "burn" into the frost table?
We have reformulated this section to "In addition, a shallow frost table was observed along the roadside ditch which results from the lower topographic height compared to the adjacent measurement lines. Here, the frost table apparently is not able to penetrate deeper into the ground. This is possibly due to the soil's higher water content from enhanced infiltration."

(14) p.932, l.26: uncomplete sentence: "Similar to slightly shallower frost table depths were...". Similarly, slightly shallower...?
We have clarified this sentence to "For the transect along the gravel road and the two profiles further downslope, an intermediate thaw depth of about 1.5 m was detected."

(15) Discussion section could be better structured and/or a schematic illustration could be included to better visualise the various processes involved. In its present state, speculations about processes (runoff, infiltration, turbulence...) are mixed with surface/subsurface data, but also with speculations about the surface conditions (e.g.

increased albedo through salt precipitation). Dividing the discussion into sections with explanation of thaw depth variability and soil moisture variability would be helpful.
We will rewrite Sect. 4.3 which will be restricted to the discussion of the end-of-summer thaw depth and soil moisture content distribution. We will also add a figure for the visualisation of processes which we use to explain the observations.

(16) How long is the phase of runoff (do you mean runoff due to snow melt and/or rain?)? Can the relatively short water infiltration period (snow melt) indeed be responsible for all the observed variability of the thaw depth? What about spatial differences of thermal conductivity, initial water content/porosity as explanations for the observed data? How much snow melt is involved (what is the average snow depth in the region)? Could a spatially heterogeneous snow cover evolution during spring and early summer not also influence the thaw depth and soil moisture patterns in summer?
These are difficult questions which are hard to answer beacuse of the lack of additional data. Hence, we will restrict the discussion to the factors that were observed during the measurement campaign and skip the paragraph with the discussion of runoff. With respect to snow cover please refer to our response to item (2) above.

(17) To what extent could the described uncertainties in the method (described both in this paper, but also in Gerhards et al. 2008) be responsible for the observed variability?
Please refer to our response to item (12) above.

(18) p.936, l.15: the presented hypotheses are still speculative: it would be better to reformulate this paragraph, e.g. "...coarse-textured soil due to the presumed interactions:..."
We have removed this sentence from the manuscript.

(19) p.936, l.16-l.26: none of these processes/variables were actually measured !! A reader who only reads the summary would get the impression, that these statements were proven or at least data-supported results of the study, which is not the case. The uncertainties involved in the hypotheses presented have to be clearly stated in the Summary section.
We have skipped these points in the revised manuscript and will restrict the assumptions made in the conclusions to the measured values and field observations.

References:
Jin, H., Li, S., Cheng, G., Shaoling, W., and Li, X.: Permafrost and climatic change in China. Global Planet. Change, 26,387–404, 2000.

Referee 2

(1) The discussion is difficult to understand and should be rewritten. Especially p934 line 16 – p935 line 3 are confusing and seem to be speculative. Can the presence of vegetation also be an important reason for a reduced soil water content? Is the observed reflector mainly due to the thaw depth or can it be linked to the groundwater table which was observed at 0.76 m depth in a borehole some hundred meter away from the measurements (Why was this borehole not made within the survey area for ground truth?) Clearly missing are additional data and/or ground truth to confirm the hypotheses.
We will completely revise Sect. 4.3 and now discuss the observed late-summer moisture contents and thaw depths with respect to the observations available from the site. The effect of groundwater and vegetation on the measured soil moisture contents will be included as well. The observed reflection cannot be linked to the groundwater table since it shows significant topography. A groundwater table would be a straight surface, potentially slightly inclined with flowing water, according to the

hydraulic gradient, but it will not create mounds of some 0.5 m height as it is observed below the vegetation. We will comment on this in the revised manuscript.

(2) New data are measured without comparison with the older data and without discussing in detail what the differences are. The new data were acquired with 3 source-receiver antennas, whereas the old data were measured with 2 source-receiver antennas. It would be good to compare the new data with the old data and discuss in detail what the differences are. From the pictures, it seems like both datasets were measured at the same site or at least very close to each other.
With respect to the methodology, the availability of more channels should stabilize the multi-channel evaluation and should be less sensitive to single outliers. We have added a comment on this to the materials section. Concerning the measured thaw depths and water contents we now compare the data from the new study to the values measured by Gerhards et al. (2008) in Sect. 4.2.

(3) The conversion of permittivity to soil water content using a constant porosity of 0.4 is questionable since the porosity is most probably changing from the vegetated fine sand area (p. 933 line 16) and the low average pore size of the road bed (p. 934 line 9). How sensitive are the inversion results on the assumed value of 0.4 for the porosity?
Please refer to our response to item (10) raised by referee 1. Still, estimates of porosity, as proposed, could be used to refine the measurements.

(4) The presentation of the results could be improved by indicating line numbers in Figure 2 and 6. Include with dashed circles the areas of vegetation which were also shown in Figure 5.
We have added line numbers to Figs. 2 and 6. In Fig. 2, we now indicate the transect discussed in Sect. 4 separately by a dashed line and included markers for

the transitions between bare and vegetated areas. In Fig. 2 the vegetated area is well visible from the photograph, hence, we did not add a separate indication here. In Fig. 6 we added a dashed line which roughly marks the vegetated area upslope of the gravel road. We do not indicate the areas of vegetation shown in Fig. 5 separately as they are too small to be well visible in the figure.

(5) The summary and conclusions should describe the reliable results observed with GPR: shallow thaw depth and low permittivity were obtained below the sand-covered vegetation area, intermediate thaw depth was obtained along the gravel road and a deep thaw depth in the bare soil terrain.
We will revise the summary and conclusions section accordingly.

(6) p. 923 line 2 mention the strong dielectric contrast explicitly p. 923 line 2 change "an" into "and" I do not understand why the position of the reflector of the laser tachimeter was used as reference position. I assume that the data was resorted and the assumed reflection position of the CMP is used as reference position.
We have added values for the dielectric permittivities to the manuscript and corrected the typo. The term "reflector" of the laser tachymeter is indeed misleading at this point. In this context we refer to the prism which is mounted on top of the antenna which is the reference position for the resorting procedure to produce CMP gathers (reflection below the middle antenna in Fig. 3B). To avoid this confusion, we now call the reflector of the tachymeter "360° prism".

(7) p. 925-929: Split up the section "materials and methods" into two separate sections: Method and measurement setup, since both are mixed which results in repetition
Done.

(8) p. 926 line 25-26 and p. 927 line 9-10 discuss the same information: One could rewrite this sentence as "resorting the measured data such that they share the same common midpoint and reflections occur from a similar area of the reflector (see Figure 3c)."
We have changed this section to "For the multi-channel evaluation, measured data from the different channels have to be resorted such that they share the same common measurement point and reflections occur from a similar area of the reflector (Fig. 3C)." and dropped item (i) on P. 927 to avoid repetition.

(9) p. 927 line 11: do you mean with "clipping" time windowing?
Yes. To clarify this issue, we now write "clipping of all radargrams to a total time window of 80 ns".

(10) p. 929 line 3: It seems like actually 6 channels are used: 6 antennas are connected to the console (3 sources and 3 receivers) resulting in 9 possible transmitter-receiver combinations (see Figure 3).
With the term "channel" we refer to one out of the nine possible transmitter-receiver combinations. We have clarified this in the revised materials and methods section.

(11) p. 929 line 21: Why are you using a velocity of 0.1 m/ns for the topographic correction while the actual measured velocity is 0.08 m/ns? Describe more specifically which data are shown in Figure 4 and do not call them "example radargrams". Better is to indicate line numbers in Figures 2 and 6.
The velocity of 0.1 m/ns applied for plotting topography has no direct relation to the velocity of the electromagnetic signal travelling through the ground. It is only employed for transforming the topography measured along the transect (unit is meters) to the travel time scale (measured in ns) of the radargram and can be used to determine the exaggeration of the plotted transect. We now call Sect. 4.1 "Transect crossing bare

soil and vegetation" and refer to the illustration in Fig. 2. The figure caption of Fig. 4 was adapted accordingly. Line numbers were added in Figs. 2 and 6.

(12) p. 929-930: Repetition occurs in p. 929 line 24-25 and p. 930 lines 26-27.
It is not clear to us what the comment of the referee exactly refers to. The last paragraph on P. 929 refers to the interpolation algorithm employed for generating the contour plots. On P. 930 we describe the averaging of the line data displayed in Fig. 5. We would like to keep these sections as they are.

(13) p. 930 line 4: Is Figure 4 showing the data after the topographic correction? If so: mention this.
The radargrams shown in Fig. 4 were corrected for topography. We now mention this in the figure caption.

(14) P 930 line 10 change "an" into "and"
corrected

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