

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.

Anonymous Referee #1

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General comments:

The authors present an applied study of multi-channel ground-penetrating radar to explore soil moisture and active layer depth at a permafrost site on the Qinghai-Tibet plateau. The study clearly demonstrates the very high potential of multi-channel GPR for effective mapping of thaw depth and soil water content in permafrost regions. The paper is very well written and organised, the illustrations are clear and of very good quality, and the method is well described. Apart from my general comments concerning the set-up of the study (see below), the paper was a real pleasure to read!

The method itself was already described in Gerhards et al. 2008 (by almost the same set of authors), which the authors cite many times within their methods section. As this former study was also conducted on the Qinghai-Tibet plateau, the new aspect of the methodological analysis is not clear to me, apart from the detailed analysis of the spatial heterogeneity. Concerning the latter, the lack of ground truth, temperature, energy balance, precipitation and snow cover data severely restrict the interpretation of the data and the validation of the hypotheses presented.

In the Discussion section (p.933-935), only unproven hypotheses of very complex interactions are presented. Neither additional data (even temperature data from the region, modelled radiation balance or satellite data concerning the snow cover evolution would have been helpful), nor comparisons with former surveys (e.g. the data from the survey of Gerhards et al. 2008), nor comparisons with own or other model studies, nor any references to previous work at all are given. Without that, the presented hypotheses remain purely speculative.

In my opinion 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.

In its present form the paper makes the reader ask oneself, why a second study in Tibet was necessary (after Gerhards et al. 2008) to only demonstrate that the method works. I assume that the effort for organising and conducting the fieldwork on the Qinghai-Tibet plateau was very high with respect to the comparatively very small survey grid shown in Figure 2. To analyse the potential of the method to characterise the spatial heterogeneity of the subsurface, other, more accessible study areas would have been better suited.

More specific comments are given below.

Specific comments:

INTRODUCTION:

The introduction is very well written and focuses on both, active layer processes and the multi-channel GPR technique. The objective of the study is given as: "the application of multi-channel GPR at a continuous permafrost site in order to efficiently infer spatial variations in thaw depth and average volumetric soil moisture content of the active layer." This objective is fulfilled, but the resulting hypotheses presented in section 4.3 (Discussion) were not directly related to the processes mentioned in the Introduction. For example, not a single reference to other work is given in 4.3 - the processes and references included in the Introduction should be used or at least better linked to the processes described in the Discussion.

SITE DESCRIPTION:

p.924, l.21: here, the site is described as "extensive discontinuous permafrost" (Chinese classification), but as "continuous permafrost" in the Introduction (p.924, l.3). Is this the same and/or what is the physical background behind these classifications ?

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 ?

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 ?

MATERIALS and METHODS

p.925, l.26: Without additional data: How did you assess that the thaw depth of the active layer was close to its deepest position on August 31st 2007 ? Besides, 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 to me! Please provide references, data from previous years

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or from comparable locations to support or explain this statement.

p.929, l.14: epsilon_a is not used in equation (3)

p.929, l.19: I do not understand, why the porosity was assumed constant in equation (3). Later in the discussion, the different soil types are analysed in the context of spatially varying soil moisture and thaw depth. Across these heterogeneities, the porosity will not be constant. 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.

RESULTS and DISCUSSION

p.930, l.10: "ground surface AND the longer..."

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.

p.932, l.22: unclear wording: what phenomenon ("which") is not able to "burn" into the frost table ?

p.932, l.26: uncomplete sentence: "Similar to slightly shallower frost table depths were...". Similarly, slightly shallower... ?

SECTION 4.3: DISCUSSION...

The first paragraph of this section is very true, and correctly describes the following section. However, if no additional data such as meteorological variables, soil moisture and snow cover are available, the question may be permitted why the survey was conducted at this site in the first place ? The task to analyse the causes for spatial variability of thaw depth and soil moisture without the above data is very difficult, so

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why was it attempted ?

This whole section is highly speculative and additional data and/or references from other work have to be added to support the hypotheses presented by the authors. In addition, the 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). Furthermore, dividing the discussion into sections with explanation of thaw depth variability and soil moisture variability would be helpful.

Concerning the interpretation I have the following additional questions:

- 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 ? - and finally: 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 ?

SUMMARY and CONCLUSIONS

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:..."

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

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uncertainties involved in the hypotheses presented have to be clearly stated in the Summary section.

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