

***Interactive comment on “Monitoring of active layer dynamics at a permafrost site on Svalbard using multi-channel ground-penetrating radar” by S. Westermann et al.***

**S. Westermann et al.**

swestermann@awi.de

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We thank Håvard Juliussen for his detailed and constructive review of our discussion paper. In the following, we address all issues raised. The comments are given in bold face, while the author's response is given in normal font. Passages from the revised version of the paper are given in italics.

**GENERAL COMMENTS**

**The paper describes the use of multi-channel ground-penetrating radar (GPR) to monitor summer thaw depth progression and soil water content over permafrost**

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**in Svalbard. Thaw depth mapping using multi-channel GPR has been described by Gerhards et al. (2008) and Wollschläger et al. (2009), but in contrast to these papers the present paper includes the time aspect through repeating surveys. The results are used further in an interesting discussion on the active layer latent heat content and identification of spots where the permafrost is particularly vulnerable to degradation. The paper is well written and should be of interest for the readers of The Cryosphere. I recommend acceptance of the manuscript after some changes have been made.**

**The weaknesses in this manuscript are in my opinion:**

**Thaw depth vs. active layer thickness monitoring. In the introduction chapter the active layer thickness and its role as an “early warning system” is presented along with monitoring programs such as the CALM program. The active layer is defined as the end-of-season thaw depth. The rest of the paper, however, shows how the thaw front propagates downwards in the late thaw season. Thus the monitoring aspect here relates to seasonal thaw depth progression and not interannual variations in active layer thicknesses as may be anticipated from the introduction chapter. The difference between the two should be made clearer in the manuscript.**

We are thankful for this comment as our wording has been inaccurate in some places in the manuscript. In the revised version, we make a clearer distinction between active layer thickness and thaw depth. The changes are marked in bold in the revised version. However, we do think that the presented study has implications for the suitability of multi-channel GPR as a tool for active layer thickness monitoring. Firstly, a rigorous determination of the end-of the season thaw depth requires a time series of thaw depths, as it is not a priori clear when the maximum thaw depth is reached. This work presents such a time series. Secondly, our study shows that the results

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of multi-channel GPR are sufficiently accurate to follow the evolution of the thaw depth within about 0.1 m. Therefore, the study suggests that it is a suitable technique to monitor the active layer thickness over consecutive years, not only over the five week period presented here. We have added a clarifying sentence at the end of the introduction:

*At the chosen study site, rocky soil and thaw depths exceeding 1.5 m effectively prevent the use of manual probing methods. The study is conducted at the end of the thaw season, so that the late-summer maximum thaw depth of the active layer can be inferred from the measurements.*

**A discussion on the accuracy of the GPR method in relation to reported inter-annual variations in active layer thicknesses (e.g. the Permafrost and Periglacial Processes 15(2) Special Issue “Circumpolar-Active-Layer-Monitoring (CALM) Workshop” and the Polar Geography 28 Issue 4) could be included to bridge this gap between the introduction and the rest of the manuscript.**

We have added a sentence to the discussion relating the estimated accuracy of the GPR method to observed interannual variations of the thaw depth and decadal variations of the active layer thickness: *The method would be capable to resolve observed interannual differences of the late-summer thaw depth on the order of 0.3 m (Christiansen et al., 2010), or decadal trends of the active layer thickness of about 0.2 to 0.4 m (Åkerman and Johansson, 2008).*

**[2] Monitoring period.**

**The monitoring period can not easily be extended in the present case, but for future monitoring projects a period covering the entire thaw season would be beneficial. An increase in thaw depth of about 0.2 m was found over the five weeks of monitoring in Aug.-Sep. 2008. This is relatively small compared to the**

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**measurement accuracy of 5-10 cm and inhibits calculation of thaw rates.**

We agree that it would have been desirable to cover the full thaw cycle. However, it is important that the multi-channel GPR method has been capable to deliver thaw depths over transect lengths which yield a consistent picture over a period of five weeks and fits well with validation data. The calculation of exact thaw rates is obviously not possible, but we can at least estimate a thaw rate of 0.2 m with a non-invasive technique (at a thaw depth of more than 1.5 m). As detailed in the paper, this would be extremely arduous to achieve by using manual methods. Furthermore, we suspect that the accuracy of thaw rates inferred from measurements with a frost probe would be in the same range.

**[3] Validation.**

**Validation of the thaw depths is limited to one point where temperature is monitored in a profile through most of the active layer. The fit at this point is however good. At least another 2-3 independent validation points would have been beneficial (1-2 points for each transect). Frost tubes is a cheap alternative to thermistors. Validation of the soil water content is also lacking except for the calculation of a simplified water budget.**

We agree that it would have been desirable to have more validation points for the thaw depth available. However, we think that all provided validation information taken together is a convincing evidence for the potential of the multi-channel GPR method.

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## **-SPECIFIC COMMENTS**

### **Title**

**Perhaps use “thermal dynamics” instead of “dynamics”, to avoid confusion with studies of cryoturbation etc.**

The presented multi-channel GPR study does not only include the thermal dynamics, but also the dynamics of the soil water content in the active layer. We believe that it is important to account for both in the title. As it is not practical to create a much longer title explicitly accounting for both, we would like to leave the title unchanged.

### **Abstract**

**Line 6. What is the material type? That is relevant in explaining deep thaw**

We have added the information that the “*surveys have been conducted in gravelly soil*”, and that the recorded thaw depths “*are among the deepest thaw depths recorded in sediment on Svalbard so far*”.

### **Introduction**

**Reported active layer thicknesses in Svalbard could be referred to here (Åkerman 2005, starting in 1972, and Christiansen et al. 2008).**

We are thankful for this valuable suggestion. The suggested publications are cited in the revised version. The paragraph in the revised version is:

*On Svalbard, considerable interannual variations of the active layer thickness are common (Christiansen and Humlum 2008, Christiansen et al. 2010) which might*

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*obscure long-term trends. In the Kap Linné area, Åkermann (2005) reports an increase of the active layer thickness since the 1980s in conjunction with increasing air temperatures. Isaksen et al. (2007) report increasing permafrost temperatures in a borehole in Nordenskiöldland during the last decade, which is accompanied by a moderate increase in active layer thickness by 10 to 30 cm.*

**Page 289 Line 20. misspelling: “Nordenskiöld Land”**

corrected

**Page 291 line 17 “...five weeks”. Time period?**

We have added the information that the study was conducted “*from mid of August until mid of September*”.

### **Methods**

**Page 292 line 20. misspelling: “MALÅ”**

corrected

### **Results**

**Page 296 line 8. How were the antenna separations chosen?**

An additional sentence has been added to explain the reasoning for the chosen antenna separations.

*The corresponding antenna separations of the 4-channel setup are summarized*

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in Table 1. With the chosen separations, the reflected wave can be picked for all channels, while the ground wave and the reflected signal are clear separated. All radargrams are recorded using a time window of 102 ns, 1024 samples and 4 stacks per trace, and a spatial trace increment of 0.1 m which is triggered by a survey wheel. A more detailed description of the factors contributing to the choice of the antenna separation is included in the response to the comment of John Moore. As the presented data set does not systematically investigate the issue of choosing optimal antenna separations, we do not want to give a detailed account of this problem in the present paper.

**Page 297 line 22. This is obviously a potentially large error source and should be elaborated further. How large was the path deviation? How can future surveys be planned to minimize this source of error? This could be included in a new section called “method evaluation” or similar in the discussion chapter.**

**Page 299 line 5. Referring to the above comment (page 297 line 22)**

The transect paths were marked on the ground, which effectively prevents a too strong deviation of the paths used for the single surveys. However, the ground features a microtopography with elevation differences on the order of 0.1 m. It is not practical to achieve exactly the same path in each survey, so that these small elevation differences will appear as noise in the surveys. In addition, a few small “valleys” incised by more than 0.1 m have been present along the radar transects, where path deviations of 0.5 m to 1.0 m between consecutive surveys (such as the one referred to on Page 299 line 5) can cause stronger deviations. At these few sites, more care should be paid in future studies to achieve exactly the same transect paths.

We have included a section on the suitability of the method and the uncertainties in the discussion. See below.

**Page 300 line 5-7. Saturated conditions at 28% vol. water content points to a**

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**lower porosity than assumed in page 294 line 14 and page 296 line 27.**

We agree with this point. The porosity of 0.4, which we used for our calculation, has been selected according to typical literature values for gravelly soils. However, lower or higher porosities are well conceivable for such soils. If our interpretation of the temporal evolution of the soil water content is correct, this would indeed mean, that the porosity is much lower, which could even be used as a non-invasive technique to calibrate the porosity. However, applying such a procedure without additional validation, e.g. a water level gauge in the soil at the relevant points, seems problematic to us. In addition, the porosity can vary in space which in our situation cannot be evaluated further with this method. We therefore use the original porosity of 0.4 and stress that the influence of the porosity on the obtained soil water contents is small. Furthermore, an uncertainty of 0.1 on the porosity is explicitly assumed in the error calculation of the soil water contents.

We have added a short paragraph to clarify this issue:

*If the interpretation given here is correct, then the saturated conditions at a volumetric soil water content of approximately 0.28, as found in case of transect T2 (Fig. 8), point to a lower porosity than assumed in the calculation of the soil water content (i.e.  $\phi = 0.4$ ). We emphasize that the potentially lower porosity is accounted for in our error analysis (see Sect. 3).*

## **Discussion**

**As also stated above, I suggest a short section on method evaluation. Here the uncertainty of the method could be evaluated and discussed in the light of reported variation in active layer thicknesses.**

**A short note on the efficiency of multichannel GPR compared to “traditional” CMP-surveys could also be given.**

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We have added a short paragraph and renamed the first subsection of the discussion to “Thaw depth and active layer dynamics”. The corresponding text passage reads as: *Multi-channel GPR has been capable to deliver the evolution of the thaw depth and the average soil water content between the surface and the freeze-thaw interface for a period of five weeks. For the conditions encountered in the study area (gravelly soil, thaw depths of more than 1.5 m), the efficiency of multi-channel GPR must be considered far superior to both manual probing methods and traditional GPR methods such as “Common Mid Point” (CMP) surveys. The accuracy of the obtained thaw depths, which is estimated to be around 5 to 10 cm, is sufficient to secure spatial differences in the thaw depth of about 0.2 to 0.3 m. The main factors limiting the accuracy in the present study are: (1) the zero-offset calibration, (2) the short antenna separation which was required due to site-specific conditions, and (3) the lack of information on the microtopography. The latter could be accounted for by using automated laser tracking of the radar antennas, as it has been employed by Wollschläger et al. (2010). Despite of such issues, the accuracy of the obtained thaw depths is sufficient to make multi-channel GPR a viable alternative to manual probing in permafrost monitoring.*

**Page 300 line 17. The TSP Norway data should be cited as “Permafrost Observatory Project: A Contribution to the Thermal State of Permafrost in Norway and Svalbard. Year of dataset release. Borehole/ minilogger name, borehole/minilogger ID. The Norwegian Permafrost Database, Geological Survey of Norway (NGU), Trondheim, Norway. Date at which you accessed the dataset” (cf. the NORPERM database at [www.ngu.no/norperm](http://www.ngu.no/norperm))**

**Page 300 line 15. See Christiansen et al. 2010 table 2 for recently published active layer thicknesses from some of the TSP Norway boreholes in Svalbard, including one site in Ny-Ålesund.**

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As the data on the active layer thickness are more readily accessible in Christiansen et al. (2010) (which had not been available at the time of publication of this work on TCD), we use this citation instead.

**Page 300 line 19-22. ...and in agreement with an increase in active layer thickness (Åkerman 1972).**

We have included the citation Åkerman (2005).

**Page 301 line 2: When air temperatures drop in the autumn and the meltwater contribution to streamflow in the Bayelva river ceases, is it possible that the river drains the near-river active layer before significant ground freezing? That would give more similar active layer ice content along the transects in winter and explain a more uniform thaw in the summer (my speculations). Extending the monitoring period in the autumn could shed light on that.**

This interpretation is well possible. However, we are reluctant to give speculations which we cannot support by data too much room in the paper as we think that probably a whole range of factors must be taken into account to explaining the thaw depths. As suggested, multi-channel GPR studies extending well into the autumn season hold great potential for investigating the interplay between the thermal dynamics and the water cycle in permafrost areas. The presented study highlights these prospects, we hope to stimulate such efforts in the future.

**Page 303 line 6. If my reasoning in the previous point is reasonable, some runoff should be expected at least for parts of transect T2**

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In the simplified water budget, we have omitted subsurface runoff as we have no data available. It is highly likely, that subsurface runoff occurs in a gravelly area adjacent to a glacial stream, where the transects are located. However, it is possible that the subsurface in- and outflow is roughly balanced for most of the monitoring period. It would have been interesting to continue the survey for a few more weeks to see whether the soil water content decreases to the summer values after the termination of the rain falls. If that had occurred, subsurface runoff would have been the only possibility to explain this given the low evapotranspiration rates in fall.

**Page 303 line 16-19. How would you calibrate your heat flow model with only non-invasive methods?**

Calibrating the heat flow model cannot be rigorously achieved by using only non-invasive methods. However, the knowledge of the average soil water content from multi-channel GPR strongly reduces the uncertainty of the calibration, so that it seems promising to employ estimates (or values determined from surface soil samples) for the missing parameters. The following quantities must be known for the heat flow model:

The specific heat capacity can be determined from the volumetric fractions of the constituents. Hereby, the knowledge of the soil water content strongly reduces the uncertainty.

The thermal conductivity can in principle be determined from the volumetric fractions of the soil constituents by using parameterizations, such as the deVries-model (overview is given in Farouki (1981)). The uncertainties hereby are large, though, and the knowledge of e.g. the organic fraction is critical. However, the knowledge of the soil water content again reduces the uncertainties.

The suggested scheme finally aims at reproducing the temperature distribution in the active layer to calculate the sensible heat content. The heat flow model

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would be employed to improve the simple linear interpolation between the surface and the 0 °C-interface. Therefore, even if the missing soil parameters are only estimated and the resulting values of the heat capacity and the thermal conductivity are biased to some extent, one would expect an enhanced performance of the scheme.

**Figures**

**Figure 1. It is difficult to read the figure, but I assume it will appear in a larger version for the final paper.**

In the final version, the figure will appear on one page, so it should be much better to read the figure.

**The figure should also include a small inset map to show the position of the site in Svalbard. A terrestrial photo of the ground conditions along the transects (material type, obstacles, etc.) would be informative.**

We have added an inset with the location of Ny-Ålesund to the orthophoto.

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