

Interactive comment on “Seasonal thaw settlement at drained thermokarst lake basins, Arctic Alaska” by L. Liu et al.

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We thank the reviewer for his/her constructive comments. We have addressed all of them and made the suggested changes in the new version of our manuscript. Our point-by-point replies (in black) to their critical comments (in blue) are listed below.

This linked pdf file is our revised manuscript with all changes highlighted in yellow
<http://www.the-cryosphere-discuss.net/7/C3360/2014/tcd-7-C3360-2014-supplement.pdf>

Please note that the page/line numbers are different in the original discussion paper and our revised paper. And we refer to the corresponding numbers in our revised paper in our replies.

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Since the GPR measurements are a major component of the study I would like to have some radargrams included showing the reflection of the ice table from the characteristic sections (see e.g. Fig. 6). Also the related frost probe measurements may be indicated therein. The authors may also have a look at the paper of Moorman et al. (2003), PPP 14: 319–329, who discuss the identification of massive ground ice bodies using GPR. Such a discussion of the radargrams may substantiate the ground ice theory for SAC basin.

AUTHORS: We added a radargram as a new figure (Fig 7) showing a profile that includes three units: i.e., basin center, basin margin, and outer basin. We also described this figure in the text (page 9, lines 290–294).

We did not identify massive ground ice using GPR in this study. We conducted GPR surveys at the end of the thaw season, aiming to measure ALT. The reflection at the interface between the thawed wet active layer and the frozen permafrost is so strong that it masks out reflectors within the active layer. We are also unable to image any structures beneath the permafrost table. By citing Moorman et al 2003, we point out that GPR surveys in the frozen season, however, are of great potential to detect excess ice bodies as their distinct geometry that results in specific reflection patterns. (page 11, lines 344–349)

The large textural heterogeneity of the area of investigation causes large uncertainties (20% for the basin area, 10% for the outer areas) in the estimates of volumetric water content using GPR even if ALT is known from additional frost probe measurements. Are there any options for reducing this uncertainty in future investigations, e.g. by measuring porosity from soil cores etc.? Please discuss.

AUTHORS: As pointed out in the text (see page 9, lines 274–276) and here by the reviewer, the uncertainties in our VWC estimates are due to compositional hetero-

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geneity and different empirical models used. Measuring porosity in the lab is a good suggestion. However, the point of estimating VWC in this study is to predict thaw strains and to compare with the thaw strains estimated based on InSAR and GPR measurements. Even though a VWC uncertainty of 20% seems large, it corresponds to a thaw strain uncertainty of only 2% (see page 9, lines 284-288). Therefore, for a basin scale study like ours, the top priority of further investigations is not to reduce VWC uncertainties, but to understand the physical mechanism of large thaw strain (>10%) we observed in the basin center.

On P 5806, L 17-18 and Fig. 6 the authors observe much larger subsidence in section “BC” than in “DE”. Please add some explanation for this to the discussion section as well.

AUTHORS: We added a general explanation of this difference in thaw strain (not in subsidence) between “BC” and “DE” in the discussion section (page 11, lines 339-343). We suggest that this reflects the heterogeneous distribution of ground ice. However, the exact mechanisms of such differences are related to soil composition and texture, which are unknown and will be investigated in future studies.

P 5794, L 3: Add explanation why knowing the dynamic state of DTLBs is important (cf P 5795, L 14-15).

AUTHORS: We added this in the abstract as suggested. Now it reads “Their dynamic states are seldom investigated, despite their importance for the long-term landscape stability, hydrology, nutrient fluxes, and carbon cycling.” (page 1, lines 2–3)

P 5798, L 8: Replace “a few” by absolute values. This is important since we are looking at thaw settlements of also “a few” centimeters only.

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AUTHORS: We now quantify the DEM accuracy as 0.1 m (page 4, line 109). The accuracy of InSAR deformation measurements is not of the same order of magnitude of the DEM accuracy. It is instead scaled by the height of ambiguity, which is on the order of 100 m for an ALOS interferogram with a perpendicular baseline shorter than 1000 m. Based on our calculation, a DEM error of 0.1 m only introduces deformation error of 0.01 cm. This is why we didn't solve for the DEM-induced error in this study (see page 5, lines 135-136).

P 5798, L 19: Replace “radar measurements” by “SAR measurements” to have a clear distinction to the GPR measurements discussed later on. Please check text for similar occurrences.

AUTHORS: As suggested, we replaced “radar measurements” by “SAR measurements” (page 4, lines 91, 117, 123).

P 5800, L 17: Suggestion: replace “the two radar transducers” by “transmitting and receiving antenna”.

AUTHORS: revised as suggested (page 6, line 164).

P 5801, L 19: Replace “unfrozen water” by “unfrozen water content”

AUTHORS: revised as suggested (page 6, line 194).

P 5802, L 8; P 5805, L 5 and whole section: Replace “radar speed” by “GPR wave speed”, check text for other occurrences.

AUTHORS: revised as suggested at following places: page 5, line 155; page 6, line 169; page 7, line 211; page 8, line 235; page 9, line 273; and Table 1 caption.

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P 5802, L 14: Suggestion: call it a . . . “site-specific” empirical model. . .

AUTHORS: revised as suggested (page 7, line 208)

P 5803, L 19-23: From my point of view, re-arranging eq. 10 is redundant at this place. Why don't the authors do their calculations using the dielectric permittivity values (eq. 10)?

AUTHORS: We chose to keep both equations. We rearranged eq. 10 to eq. 11 for continuity because we used GPR wave speed throughout the entire manuscript (sections 2.4, 3.2). Additionally, not all Cryosphere readers are familiar with these basic EM equations.

In addition, eq. 11 is only valid for saturated soils. The velocities given in Davis and Annan (1989) at least for dry silt are valid for the bulk soil (silt + air); see also comment below. In eq. 11, the velocity for the pure mineral grains would be required.

AUTHORS: We stated explicitly that the two-phase CRIM model applies to saturated soils in the original discussion paper (see page 7, line 226), which is consistent with our assumption based on field observations in basin center and basin margins (see below).

P 5805, section 3.2: The authors assume that the whole area (basin and margins) is fully saturated. I am wondering if this assumption is justified. From Fig. 1b we observe a jump in elevation of 1 to 2m between basin and margins. If the whole area was saturated the groundwater table would be a straight, maybe little inclined surface but it would not be able to follow the topography to have the complete area saturated. If the margin area would not be saturated, eq. 10 would not be valid and would have to be extended to the 3-phase CRIM formula for unsaturated soils including the air phase as well

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AUTHORS: The 3rd panel of the figure on the last page of this reply shows the elevation profile, along with the ALT and subsidence profiles as shown in Fig 6. The elevation jump that the reviewer referred to is located at the boundary between the outer basin and the basin margins. The sandy margins and the basin center are located at similar elevations. This can also be seen in Fig 1b. Fig. 6 is already a busy plot, so we did not add another elevation profile to it.

Sandy margins were saturated based on our field observation and this is also a reasonable assumption. It is therefore unnecessary to use the 3-phase GRIM equation and it introduces another parameter (saturation fraction) in addition to VWC, which is the major parameter we wanted to solve from GPR wave speed.

P 5805, L 13: please change: “The VWC of saturated pure mineral soil is. . .”

AUTHORS: ‘saturated’ is added as suggest (page 9, line 278).

P 5806: Replace “detected” by “estimated” (values are not confirmed by additional measurements and velocity may change with depth)

AUTHORS: revised as suggested (page 10, line 301).

P 5797, L 1: Reference Hinkel and Nelson, 2003 missing in reference list.

AUTHORS: reference has been added.

P 5816, Table A1: add reference to table in Text; check for completeness (e.g v_g and v_w are missing)

AUTHORS: We added v_g , v_w , and Davis and Annan 1989 as references for v_g . We chose not to add references where they appear in the main text (if this is what was

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suggested by the reviewer), as they are clear by themselves and ordered according to their appearance in the main text. Table A1 is already very long.

P 5822, Fig. 6a: Some marks for probing locations are difficult to identify. Suggestion: use red color instead of black.

AUTHORS: Done as suggested.

Interactive comment on The Cryosphere Discuss., 7, 5793, 2013.

TCD

7, C3368–C3375, 2014

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Comment

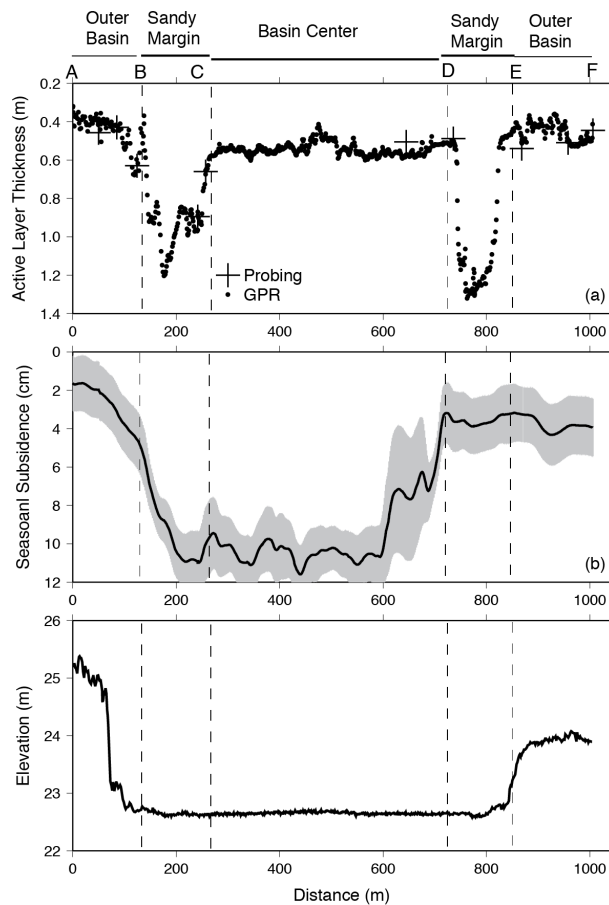
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**Fig. 1.**