

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

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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p5797, line 23, any comment on how the motion compensation strategy of Zebker et al. (2010) differs from conventional slave to master registration and why you used it?

AUTHORS: We added the following description on the InSAR processing method: “Using accurate satellite orbit information, this new processing strategy simplifies image coregistration and improves accuracy of InSAR measurements from conventional methods.” (page 4, lines 96–98)

p5798, line 23–27, the authors are clear that the InSAR values are unlikely to be affected by double bounce effects and changing water levels, however, what about inaccuracies related to more subtle surface saturation? In small flooded patches phase values would be less reliable. Do you have any such surface conditions? In Fig 4b, there are some patches where the inaccuracies are 80–100%, could surface saturation be an explanation for these unreliable patches? A recent paper in RSE discussed this effect specifically for hi-res InSAR over permafrost terrain.

AUTHORS: We don't have surface moisture measurements other than GPR and field observations, based on which we assume that the soil surface is fully saturated. We therefore chose not to further analyze effects of flooding on measurement accuracy. The large RELATIVE uncertainties are mostly located at pixels where small settlement occurs. The ABSOLUTE uncertainties are similar at most of these pixels. We quantify such a small spatial variation in the manuscript (page 9, line 267), but only plot the relative uncertainties in Fig 4b.

p5807, lines 13–23. GPR profiles are often used to identify the presence of ground ice. Did your GPR data provide any information or confirmation of these general theories and observations about local ground ice?

AUTHORS: This is similar to a comment from reviewer 3. The use of GPR to study ground ice in permafrost settings is viable in two ways: mapping subsurface massive

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ground ice bodies in winter when active layer is frozen and identifying ice lenses/ice wedges beneath polygons in thaw seasons. Our GPR data were collected near the end of thaw season and the dominant signal is from the interface between the unfrozen and frozen layers and therefore providing no information beneath the permafrost table. We added a paragraph in the discussion section to explain this, together with a new figure (Fig. 7) showing a radargram. (page 11, lines 344–349)

p5795, line 19, 'ice forms in three major mechanisms', replace 'in' with 'by'?

AUTHORS: done as suggested (page 2, line 36)

p5806, line 19, 'inversed spatial pattern', replace 'inversed' with 'inverted' ?

AUTHORS: We changed the wording to 'inverse spatial pattern' (page 10, line 310)

p5806, line 23, should one explicitly conclude that the SAC basin is therefore not a closed system?

AUTHORS: We chose only to describe results in section 3 and to discuss them in section 4. The discussion of the observed thaw strains and their implications that the basin center is not a closed system can be found in the first paragraph of section 4 (see page 10, lines 315–320).

p5807, lines 8-9, 'that range of 20-35%', suggest change to 'that range from'

AUTHORS: We changed this sentence to '... thaw strains range from 20 to 35%' (page 10, line 324).

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