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Interactive comment on "Response of ice cover on shallow lakes of the North Slope of Alaska to contemporary climate conditions (1950–2011): radar remote sensing and numerical modeling data analysis" *by* C. M. Surdu et al.

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We thank Reviewer 1 for taking time to read and provide valuable comments on our paper. We have addressed each concern and have revised the manuscript to reflect the suggested changes. Detailed answers to Reviewer 1's comments have been copied below. Overall comments 1. Reviewer 1 comment: This paper is well written and the topic addressed will interest the readership of 'The Cryosphere'. As reflected in the list of references the use of radar remote sensing and numerical modelling for the characterization of lake ice cover and ice cover change is not new. However, the

paper is novel in the sense that it addresses the joined application of both the radar remote sensing and numerical modeling approach and considerably extends the period of observation relative to earlier studies. Rather than advancing the scientific aspects of the radar remote sensing or numerical modelling of lake ice this paper represents a demonstration of the approaches and their potential for climate change studies.

Response to Reviewer 1's comment: We are glad that Reviewer 1 found our paper well written and novel. Indeed, previous research has shown the advantages of spaceborne radar, C-band SAR in particular, for providing a clear discrimination between floating and grounded ice. Using SAR and numerical modeling jointly, we captured changes in floating/grounded ice and ice thickness of shallow lakes over a relatively long-term period in relation to recent changes in climate observed in the Arctic.

2. Reviewer 1 comment: My list of specific comments below identifies certain weaknesses of the study / paper. For the most part they are minor in nature. I have some concerns re the approach adopted for the validation of the SAR segmentation results (see comment re page 3792, line 7) but these, in my view, go beyond the scope of the paper, the interest of the readership and are not likely to seriously affect the overall findings. Response to Reviewer 1's comment: We concur with Reviewer 1's concerns regarding the validation approach and we also agree that a more detailed validation for this site would provide a more comprehensive assessment of the segmentation accuracy. IRGS segmentation of SAR images in the MAGIC software, the software used in the present study, has proven to be a robust method and has already been evaluated and fully validated for both sea ice (Clausi et al., 2010) and lake ice (Ochilov et al., 2010), and is being succesfully used by Environment Canada's Canadian Ice Service (CIS) for sea ice classification. Following Reviewer 1's suggestion, we therefore removed the description of validation and instead referenced previous work that has already fully validated this method in section 3.1.

3. Reviewer 1 comment: Among the more important flaws is the poor legibility of the figures 3, 4, and 6. The quality of these figures should be improved prior to publication.

Response to Reviewer 1's comment: We agree with Reviewer 1 but this is the editorial office's decision to reduce the figures in the version of the paper provided to the reviewers. However, we will advise them about this concern hoping that this problem can be fixed before final publication.

Specific comments 4. Reviewer 1 comment: Page 3789, line 13; "This discrepancy was possibly related to the . . ." The explanation offered for the observed difference in the fraction of grounded ice mapped using ascending (asc) and descending (desc) passes is rather weak. In fact, the authors should be in a position to either prove or disprove that the difference in coverage as provided by asc and desc images is responsible for the observed discrepancy in the fraction of grounded ice. To do so, they would need to evaluate the fractions of grounded ice in the areas of overlap between ascending and descending images as well as in the remaining, non-overlapping, image areas. The difference in the proportion of grounded ice in the non-overlapping areas should be of the order of 1.5% to 2%. On the other hand, there should not be a difference in the area of grounded ice observed in the area of overlap between asc and desc images that were acquired two-days apart. Any differences observed in this overlap area would indicate that one or more complicating factors other than 'coverage differences' are at play.

Response to Reviewer 1's comment: The differences in both overlapping and non-overlapping areas are in order of 1-2%. The differences in the grounded ice fraction observed in the overlapping ascending and descending images are attributed to the right-looking ERS geometry. The SAR looking geometry of ERS – from the east in ascending mode and from the west in descending mode – limits the identification of the exact same ground features in overlapping images due to the angle of illumination. Issues such as foreshortening and layover are known to result in possible deformations in areas where the topographic slope is greater than 10°. However, considering that the study area is a coastal plain, such deformations are unlikely and the difference in the grounded ice fraction is associated with the illumination differences. We felt that

it was important to report this difference (1-2%) between ascending and ascending as previous studies have generally been silent regarding the geometry of overpasses (i.e. dates are usually provided in papers but not overpass mode). The text was also updated to show this additional comment (Section 3.1).

5. Reviewer 1 comment: Page 3789, line 24; see comment re line 13 above. Response to Reviewer 1's comment: Please see reply to Reviewer 1's comment: Page 3789, line 13. Explained in previous paragraph just above.

6. Reviewer 1 comment: Page 3791, line 5; "bubbles mainly resulting in roughness scattering" This is not correct, the inclusion of (spherical) bubbles in ice cover, assuming they are of a size comparable to the incident wavelength, will result in volume scattering rather than surface scattering (the term roughness scattering is not commonly used). Response to Reviewer 1's comment: We agree with Reviewer 1 and have re-phrased the statement. The text now reads "The higher radar return could also be explained by the presence of smaller tubular bubbles rejecte during freeze-up or of larger ebulition spherical bubbles, resulting in a double-bounce effect (Mellor et al., 1982)."

7. Reviewer 1 comment: Page 3791, line 7; "ponding water that reflects the radar signal IN A DIRECTION AWAY FROM THE SENSOR" Please add the latter text shown here in a BOLD type face; it is better to be absolutely clear. Response to Reviewer 1's comment: The suggested text has been added.

8. Reviewer 1 comment: Page 3791, line 10; The segmentation results presented in Figure 3 are shown too small for interpretation/evaluation by readers: please enlarge. Response to Reviewer 1's comment: We agree with Reviewer 1 but this is the editorial office's decision to reduce the figures in the version of the paper provided to the reviewers. However, we will advise them about this concern in hoping that this problem can be fixed before final publication.

9. Reviewer 1 comment: Page 3791, line 18; In terms of your image segmentation

method, please comment on how you dealt with mixed image pixels, i.e. pixels that are located at the edges of the lakes and include both land and ice. Such pixels complicate your analysis because their backscatter levels will likely be lower than for floating ice and higher than for grounded ice. Response to Reviewer 1's comment: We understand Reviewer 1's concern. MAGIC, the method used for image segmentation, uses a region-based approach rather than a pixel-based one. Sites (group of pixels) are likely to be included in the same region if they are neighbours and the gradient between them is not significant. An edge penalty is also applied meaning that strong edges at the boundary between floating and grounded ice or between ice and land will result in two different regions. Conversely, if these boundaries are weak then the marginal pixels will be assigned to the region that is closest and most similar.

10. Reviewer 1 comment: Page 3792, line 5; It is not clear from text whether or not this -12.5 dB threshold is used in the applied MAGIC software. Please clarify. Response to Reviewer 1's comment: The -12.5 dB threshold was only used for evaluating the performance of MAGIC (removed in the updated version of the paper) and backscatter values were compared to segmentation results for individual pixels. This threshold was not used for segmentation.

11. Reviewer 1 comment: Page 3792, line 7; In my opinion, the adopted approach for evaluation/validation of the segmentation results would not suffice if this manuscript had been submitted for publication in a journal focusing on remote sensing techniques/applications for the following reasons: (1) I believe the approach adopted was rather subjective. For example, visual interpretation would have to be done by an independent interpreter (someone unaware of the automated segmentation results). Was that the case in this study? (2) how/why were the three images referred to in line 29 of page 3791 selected; does this statistically amount to a sufficient number of samples? (3) was there ground reference data available to validate both visual interpretation and segmentation? (I know of areas that look like grounded ice in C-band SAR images but upon validation in the field are not). (4) the statistics referred to in the text would

have to be explicitly shown in the paper; by the way, the reported error rate of 0% is not believable. Having said this, there is ample evidence in existing literature that SAR images offer considerable potential for the mapping of grounded freshwater ice. Also, the mapped decrease in the grounded ice (22% over 20 yrs) likely exceeds the SAR analysis error and 'The Cryosphere' does not qualify as a remote sensing journal. As such, I feel the authors can limit themselves to referencing papers that evaluate the potential of MAGIC for ice mapping and papers re the mapping of ground ice with C-band SAR. The description of the weak validation attempt could be omitted. Response to Reviewer 1's comment: Following Reviewer 1's suggestion, we removed the description of validation and instead referenced previous work that has already fully validated this method. Please also see comment #2 above. However, to explain a little more about validation: (1) visual interpretation of the images was performed by the authors of this study; (2) the three images used for validation were randomly selected. We agree with Reviewer 1 that the number of samples could be improved; (3) in situ data was not available for validation; and (4) References to previous work that has fully validated MAGIC has been included in the text.

12. Reviewer 1 comment: Page 3792, line 8; Figure 4 is rather small. I see no evidence of segmentation results being presented in Figure 4b. Hence, this figure does not demonstrate the effectiveness of your image analysis method. See also, comment above. Response to Reviewer 1's comment: Following Reviewer 1's suggestion, we removed the description of validation and Figure 4 as well and instead referenced previous work that has already fully validated this method. Please also see comment above (comment #2). To address Reviewer 1's concern, the segmentation was performed only on the selected 40 pixels and the rather small area covered by these is the reason why the segmentation results are not clearly visible in the presented figure.

13. Reviewer 1 comment: Page 3792, line 13; I am not sure why Fig. 5 is included in this paper because it is not a result of the presented work. Response to Reviewer 1's comment: As suggested, we have removed the figure as we consider that sufficient

references were provided to support the performance of CLIMo.

14. Reviewer 1 comment: Page 3794, line 8; “higher fraction of floating ice” I feel it makes more sense to refer to a “lower fraction of grounded ice” given that the percentages following in line 10 refer to grounded ice too. Response to Reviewer 1’s comment: We agree with the reviewer and we have updated the text to reflect the suggested changes.

15. Reviewer 1 comment: Page 3794, line 14; the graphs shown in Figure 6 are barely legible: please show larger. Response to Reviewer 1’s comment: We agree with Reviewer 1 but this is the editors’ decision. However, we will advise them about this concern.

16. Reviewer 1 comment: Page 3794, line 14; Re Figure 6e, from 2006 onwards we see little change in the simulated ice thickness but a considerable decrease in the percentage of grounded ice mapped (somewhat visible in Fig 6d too). Can the authors hypothesize/explain why this is the case? Response to Reviewer 1’s comment: One hypothesis to consider when explaining the considerable SAR-derived changes in grounded ice and the small changes in simulated ice thickness from 2006 onward is the yearly variation in ice thickness. Minimum changes in ice thickness should occur, once a threshold is reached, thinner ice covers are reflected by the higher fractions of floating ice. Increasing lake water levels at the time of freeze-up or greater lake depths are aspects to also consider in explaining the differences between the changes in grounded ice and those in ice thickness. Since calculated P – E values do not indicate higher water levels from 2006 to 2011, changes in lake depth may explain the minor ice thickness changes. However, considering the lack of data on lake bathymetry, this hypothesis needs to be further investigated. This comment was also added in text, Section 6.2.

17. Reviewer 1 comment: Page 3794, lines 20-24; the authors seem to suggest here that a 22% reduction in the surface area of grounded ice translates in a 22% in (max-

imum) ice thickness. What is the basis for this statement, I do not believe this can be true. Moreover this does not agree with the numbers provided in page 3795, line 28. Response to Reviewer 1’s comment: We agree with the reviewers comment. There is not a 1:1 relation in terms of percentage change between the reduction in grounded ice of 22% and ice thickness, although ice thickness has been decreasing as well. Despite the less steep reduction of simulated ice thickness, the grounded ice fraction decrease relates well to that of thinning ice and respective trends show this difference. The hypothesis for the less abrupt change in ice thickness was discussed in the above reply to Reviewer 1’s comment: Page 3794, line 14. In this respect, we have gone through the paper and corrected, whenever needed, to ensure that the reader would not be left with the impression of a 1:1 relation between grounded ice and ice thickness changes.

18. Reviewer 1 comment: Page 3796, line 13; Figures 6d and 6e seem to suggest that this may not be the case in more recent years (>2006) (see also comment re Page 3794, line 14 above). Please comment. Response to Reviewer 1’s comment: See our reply to comment #17 above.

19. Reviewer 1 comment: Page 3796, line 3; “earlier freeze-up dates” I believe this should be “LATER freeze-up dates”. “earlier” is also not in line with simulations results referred to in line 11. Response to Reviewer 1’s comment: Thanks for catching this. The text has been updated to “later freeze-up dates” to show this correction.

20. Reviewer 1 comment: Page 3797, line 28; “53% snow cover DEPTH” please add “depth” (also on page 3798, line 22 and possibly elsewhere in manuscript). Without ‘depth’ it will easily be misread as 53% of the surface area being covered by snow. Response to Reviewer 1’s comment: The text has been updated accordingly to “53% snow cover depth”.

Interactive comment on The Cryosphere Discuss., 7, 3783, 2013.