

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 2 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 2's comments have been copied below.

1. Reviewer 2 comment: While the paper is reasonably well written in parts, my main concern are an overlong and rather confusing composition of sections “Discussions”

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and “Summary and Conclusions”. While these sections contain all the required material and findings, a certain lack of structure makes it very difficult to find their story lines. This is especially true for Section 5. I would like you to consider (re)structuring these last two chapters of the paper to improve their clarity.

Response to Reviewer 2's comment: We admit that our organization of the important findings in this paper may have created some confusion and in order to improve its clarity, we have restructured both Section 5 and 6 (now renumbered to sections 6 and 7 as a new background section 2 was added).

General comments 2. Reviewer 2 comment: As mentioned above, your two main Sections (Sections 5 and 6) are difficult to read and almost exhausting. First and foremost, I think this is due to the lack of any structuring element especially in section 5. The unstructured running text makes it very hard to recognize a consistent story line in these sections and it makes it difficult to stay focused. Finally, the language in these sections is, in parts, quite imprecise, adding to my confusion and frustration. I would suggest to re-organize these sections into bite-sized pieces that have individual clearly phrased sub-headings and can be digested independent from each other. Also, please try to highlight better what the real difference between Sections 5 and 6 is? To me, at some point, these sections seem to blur together. Response to Reviewer 2's comment: As suggested by Reviewer 2, we have restructured Sections 5 and 6 (now sections 6 and 7), which now also include individual sub-headings. We believe that the changes made to these sections also highlight better the differences between them. We thank the reviewer for this useful suggestion.

3. Reviewer 2 comment: One last comment about Sections 5 and 6: It seemed to me that some of the conclusions and statements in Sections 5 and 6 aren't based on the SAR or model data and seem additionally rather speculative. I would suggest reducing the amount of speculative statements. I think they distract from the focus of the paper and lower the relevance of the paper rather than increase it.

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Response to Reviewer 2's comment: Since Reviewer 2's comment was rather general than specific we were not sure which statements seemed speculative. However, we have removed some statements and the updated version of the text reflects these changes.

4. Reviewer 2 comment: Throughout the paper, you introduce measurements and derived averages without providing any information about their accuracy. For instance, on page 3788 you state "maximum ice thickness . . . is happening earlier by 6 days . . . and is delayed by 7 days . . .". I am guessing these are averages that were derived from field or remote sensing measurements, which, as such, need to be associated with standard deviations. Only with error information attached are these numbers meaningful. Without accuracy information, there is no way for me to judge the significance and relevance of your statements.

Response to Reviewer 2's comment: We understand Reviewer 2's concern regarding error information but since the changes in the timing of maximum ice thickness are model outputs and not field or remote sensing observations as the reviewer seem to suggest, no standard deviations are generated by the model. For example, in the case of ice thickness there are only two maximum ice thickness values simulated with CLIMo; one without snow on the ice and the other with 53% snow depth on the ice. Therefore, calculating a standard deviation is not relevant for either ice thickness or freeze-up and break-up dates simulated with CLIMo.

5. Reviewer 2 comment: While you don't really talk about it, I am convinced that the temporal sampling of the freeze-thaw process with remote sensing data is very sparse. Please make sure that you add more details on the temporal sampling throughout the observed seasons to the paper, in order to allow for an assessment of the potential and limitations of your study.

Response to Reviewer 2's comment: Indeed, the low temporal resolution (repeat cycle of 35 days) of ERS-1/2 would not be suitable for the determination of freeze-up and

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break-up dates. However, considering that our study focuses on the determination of grounded and floating ice during the ice growth season, this is not a significant issue since high temporal resolution is not necessary (i.e. ERS observations still allow for the monitoring monthly changes in the fraction of floating and grounded ice, and appropriate comparison between years). This is also one of the reasons why we used CLIMo as it simulates freeze-up and break-up dates with a daily temporal resolution. ERS-1/2 observations and CLIMo simulations complement each other. Additional details explaining the potential and limitations of ERS were also included in Section 2.

6. Reviewer 2 comment: In Sections 4.1 and 4.2, you mention results of many statistical tests but you never show any data or test variables that would verify your statements. Please provide more information on the test and don't require the reader to simply believe you.

Response to Reviewer 2's comment: There may have been a misunderstanding in the statistical results presented. All data that was used for statistical tests on variables (i.e. ice thickness and freeze-up/break-up dates from CLIMo as well as grounded ice fraction from ERS-1/2) are provided in the graphs of Figures 8 and 9 of the manuscript. We used the Mann-Kendall method, referred to on p. 3794 (lines 17-19), to test for significance of trends and magnitude of change using Sen's method. These approaches are widely used for detecting trends/changes in ice cover and hydrological time series. We did indicate the use of Mann-Kendall and Sen's methods in the manuscript submitted: "Trend detection was performed using the Mann-Kendall test, a method often used for detecting the presence of linear trends in long-term lake ice observations (Futter, 2003; Duguay et al., 2006). Trend magnitude (slope) was estimated with Sen's method (Sen, 1968; Duguay et al., 2006)." We added a new reference (Noguchi et al, 2011) in the revised manuscript.

Specific comments 7. Reviewer 2 comment: Page 3785, line3: please add "water composition" and "water dynamics" to the list of parameters that may cause heterogeneous scattering responses

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Response to Reviewer 2's comment: Reviewer 2's suggestions were added to the text.

8. Reviewer 2 comment: Page 3786, line 20: replace "excludes" with "precludes"

Response to Reviewer 2's comment: Text was updated to reflect the suggested change.

9. Reviewer 2 comment: Page 3787, line 6: add the word "an" before ". . . area that is dominated . . ."

Response to Reviewer 2's comment: Text was updated to reflect the suggested change.

10. Reviewer 2 comment: Page 3788, line 8: you write "+-1 degree Celsius change in air temperature resulting . . .". What air temperature are you talking about here? Monthly average temperatures? Daytime temperatures? Nighttime air temperatures? Average column temperatures or temperatures in the first 5 meters over ground? Be more specific.

Response to Reviewer 2's comment: Monthly means of 2-m air temperature values is what we refer to as 'air temperature' when talking about the ± 1 °C change. This detail was also added in text.

11. Reviewer 2 comment: I would add a trend line to Figure 2 to better support some of your trend statements that you are making in the text.

Response to Reviewer 2's comment: Trend lines have been added to Figure 2. We also added the statistical significance of the trends in the figure caption.

12. Reviewer 2 comment: Page 3789, line 19: You write "Optimum radar images are acquired . . ." – optimum in what sense? Be more specific.

Response to Reviewer 2's comment: By "optimum", we meant images that were not affected by possible melt at the end of ice season (late April-early May). Image selection also considered daily air temperatures as recorded at the Barrow met station.

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Therefore, prior to segmenting late-winter images, we ensured that air temperatures values prior to and at the time of SAR acquisitions were below 0° C. This additional clarification was also added in text.

13. Reviewer 2 comment: Page 3790, line 25: Consider changing ". . . off the ice surface . . ." to ". . . off the ice-water interface . . ."

Response to Reviewer 2's comment: Text was updated to reflect the suggested change.

14. Reviewer 2 comment: Section 3.1: How do you deal with occasional registration issues and geocoding errors of the ERS imagery? Will such issues lead to the mis-interpretation of the data? Have you analyzed how often geocoding errors occur and how they may affect your results?

Response to Reviewer 2's comment: We agree with Reviewer 2 that ERS data comes with occasional registration and geocoding errors. In order to assess the possibility of dealing with mis-geocoded images, we applied the terrain correction function available in the MapReady software and then compared the outputs of the geocoded images using a DEM with those that were geocoded without a DEM. The comparison showed that in both situations the images overlaid with a LANDSAT-derived land mask. However, this is not an issue for this study since we did not do pixel-to-pixel comparisons (i.e. precise geographical alignment of pixels between images was not necessary). Total grounded ice fractions were calculated for each image separately and then compared between years.

15. Reviewer 2 comment: Section 3.1, assessment of area percentages: Your percentage calculations and area percentage assessments are limited by the low resolution of your system. E.g., if a lake is only 1 pixel large, you can only say that this lake is either 100% frozen to the ground or 100% covered by floating ice. Hence, especially if your test site is dominated by very small lakes, your area percentage assessment might become very coarse.

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Response to Reviewer 2's comment: Considering the low spatial resolution of ERS, lake area was an aspect that we considered in our analysis. In order to avoid extremely small lakes, covered by potentially only one pixel being classified, we used a lake mask that only included lakes with an area larger than $10 \times 10^4 \text{ m}^2$.

16. Reviewer 2 comment: Page 3794 and Table 1: Table 1 only shows the April and May data that was used in your study even though from reading first sentence in Section 4.1 I expected the Table to show the full list of used data sets. Seeing the full list of data would be desirable as it would allow the reader to get a sense of the temporal sampling issues that might be inherent to your data. A complete Table 1 would make assessing the potential and limitations of your study easier and more transparent. Adding more measurement information (e.g. backscatter time-series plots) to Section 4.1 would enhance its clarity.

Response to Reviewer 2's comment: Table 1 was updated to reflect the acquisition dates of all images used in the current study. Backscatter time-series plots would indeed be interesting to see from a reader's point of view but we believe that in this case these are not as relevant considering that these plots would show backscatter values for individual ice pixels whereas the segmentation was performed on the entire lake or lake areas.

17. Reviewer 2 comment: Section 4.1: Measured mean fraction of grounded ice: Also here, measurements are presented without standard deviations. Please add some information about the variation of the data.

Response to Reviewer 2's comment: In order to reduce confusion, we removed 'mean' from the text and added a standard deviation, which is now reads: "The greatest change was observed to occur in April, with maximum deviation values ($\pm 15\text{--}18\%$) from the monthly mean of 43% calculated from all years (1992-2011) and a standard deviation of 9.83.". The deviations provided for the grounded ice fraction are deviations from the calculated monthly mean ice fraction from 20 years.

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18. Reviewer 2 comment: Figure 6: You mention that Figure 6 would show the correlation between the "transition toward lower fraction of grounded ice . . ." and "trend toward thinner ice . . .". I disagree with this statement in the sense that I don't think the bar plot-type Figure 6 is helpful with identifying such correlations. I would suggest to change this Figure into a scatter plot, as scatter plots make correlation behavior very easy to see. It would enhance the clarity of your discussions.

Response to Reviewer 2's comment: We agree with Reviewer 2 and therefore have changed the bar plot-type to a scatter plot, now shown in Figure 4.

19. Reviewer 2 comment: Section 4.1: Trend detection using the Mann-Kendall test: did you include the uncertainty of your observations in this trend analysis?

Response to Reviewer 2's comment: This study did not involve an uncertainty analysis as seems to be suggested. As a result, trend analysis using the Mann-Kendall test did not account for these.

20. Reviewer 2 comment: Figure 8: I don't see any indication of a "period of accelerated decline in maximum ice thickness" in the model data plotted here (the lines in Figure 8 don't show a visible acceleration).

Response to Reviewer 2's comment: Please see response to Reviewer 1, copied below. 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

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also added in text, Section 6.2.

21. Reviewer 2 comment: Page 3796, lines 4-8: Please add standard deviations to your estimates of changes in freeze-up dates and break-up dates.

Response to Reviewer 2's comment: We did not fully understand this comment (i.e. standard deviation to estimates of changes). We believe that a standard deviation is not needed since we are referring to trends (i.e. rate of change over time), not variability along the trends.

22. Reviewer 2 comment: Page 3796, first two sentences of Section 5: I would remove these sentences from the document. Your document is addressing longer term trends in lake ice behavior and the reader is going into this Section with this expectation. Starting off with a discussion of short term (seasonal) trends is therefore confusing and I would suggest removing these sentences from Section 5.

Response to Reviewer 2's comment: We agree with Reviewer 2 and have removed the first two sentences of Section 5 (now section 6) from the text.

23. Reviewer 2 comment: Page 3796, line 19: You write "A specific temporal pattern in the evolution of the grounded ice fraction . . . was not observed . . ." – what time frame are you talking about here? Seasonal? Long term? Please try to increase the clarity here.

Response to Reviewer 2's comment: We agree with Reviewer 2 and in order to improve the clarity, the text now reads "During the 20-year period of SAR analysis, a specific temporal pattern in the evolution of the grounded ice fraction for individual lakes was not observed on a yearly basis." The italicized words were added to the text.

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