Review: Response of ice cover on shallow lakes of the North Slope of Alaska to contemporary climate conditions (1950-2011): radar remote sensing and numerical modelling data analysis
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Overall

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 characterisation 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 modelling 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. 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. 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. In addition, there is a need to modify / clarify certain parts of the text (see comments below).

Specific comments

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.

Page 3789, line 24; see comment re line 13 above.

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).

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.

Page 3791, line 10; The segmentation results presented in Figure 3 are shown too small for interpretation/evaluation by readers: please enlarge.

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.

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.

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 focussing 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.

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.

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.

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.

Page 3794, line 14; the graphs shown in Figure 6 are barely legible: please show larger.

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?

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 (maximum) 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.

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.

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.

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.