

## ***Interactive comment on “The fate of lake ice in the North American Arctic” by L. C. Brown and C. R. Duguay***

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General comments:

In this paper the authors provide scenarios of changing lake ice phenology and thickness over the North American Arctic from a lake ice model driven with output from a regional climate model. The lake ice model was able to provide realistic simulations of the observed lake ice climate over a range of sites. The results show about a 3 week reduction in ice cover duration and a 25 cm reduction in mean ice thickness between 1970 and 2050 which corresponds to an ice cover loss rate of about 2.6 days/decade which is comparable to observed trends and previously published lake ice scenarios. The new aspects presented in this paper are the sensitivity of simulations to snow cover and to internal climate variability, and more detailed spatial information on pro-

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jected changes. However, the authors could make more of these results by focusing on the new findings and including them in the abstract. The paper could be significantly strengthened with some minor additions (see suggestions below). Overall the paper is reasonably well-written but would benefit from a better organization of material and more succinct language. Some figures could also be dropped where the results show little change (e.g. Fig. 4).

Detailed comments:

1. Abstract: The first half of the abstract is spent presenting background information. I suggest you trim this back and focus more on presenting the significant results of the study e.g. Fig. 12.
2. p. 1777 line 25 to p. 1778 line 5: The references you cite all relate to changes in the extent or duration of snow cover and not to changes in the depth or mass of snow on the ground. This is inconsistent with the arguments presented in the beginning of the paragraph. Future projections for changes in snow water equivalent (Raisanen, 2007; Brown and Mote, 2009) suggest a gradient over the Arctic with increases at higher latitudes and decreases along the southern boundary (can also see this in your Fig. 9b). It would be interesting to explore the implications of this gradient in more detail given the sensitivity of the lake ice climate to snow cover.
3. p. 1779 para starting line 9: The terminology here is a bit confusing. The act of gridding climate data does not solve data gaps. I think you need to replace “gridded climate data” with more specific terms like “reanalyses” and “climate model output”. I suggest you cut most of the material in this para and just cite examples of other studies that have applied reanalyses and climate models to simulate lake ice.
4. Section 2.2: There is no mention in the model description how snow is treated. I assume it is a single layer and eqn (1) is applied for the snow layer and for various depth increments (not stated) into the ice. Given the sensitivity of the lake ice processes to the snow layer, there should be some discussion of how well CLIMo represents on-ice

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snowpack properties such as albedo, density and thermal conductivity. Is melt ponding parameterized in some way? There are some recent papers looking at single layer versus multiple layer treatments of snow cover over ice that would be relevant to this discussion (e.g. Chung et al., 2010). It appears you are using the snow density from CRCM which comes from CLASS 2.7 where snow density rapidly reaches a maximum value of 300 kg/m<sup>3</sup> for cold snow (Brown et al., 2006). This is probably ok for Arctic environments where snow is subject to wind packing. You should check the thermal conductivity expression used in CLIMo against recent field values obtained at SHEBA to see how they compare; the lake ice simulations will be quite sensitive to how this is parameterized.

5. Section 2.2: CLIMo includes a mixed layer so water temperatures should be different between the current and future climate simulations. The water column would rapidly stratify in the fall and cut-off this heat source but warmer water temperatures may play a role in delaying the onset of ice cover (e.g. Arp et al., 2010; Kvambekk and Melvold, 2010). There was no mention of this aspect of the model in the paper.

6. Section 2.3: the material on the bias correction needs condensing.

7. Section 2.4: will snow density values from terrestrial snow courses be applicable to snow on lake ice? I think Chris Derksen has data traversing land and lakes in the Daring Lake area that could answer this question.

8. Section 3.1, first sentence: How about "Realistic representation of snow cover on a lake ice surface is important for accurate simulation of ice evolution (Brown and Duguay, 2011) because of snow's important insulating role and contribution to snow ice growth." You could eliminate about half the text in the paper with some judicious editing.

9. Section 3.1 line 26: "Comparison of the full snow scenarios is dependent on the equivalency of the snow conditions between the input data sets". What does this mean?

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10. Section 3.2.1: Since break-up is largely insensitive to snow depth is there a need to show Fig. 4?

11. Ice cover change figures: the differences due to lake depth seem subtle for the most part (unless these are being masked by the legend used). Do you really need to show all three depth results for all figures? Figure 12 is an exception as there are clear differences between the depths.

12. It was not clear what fraction of the CRCM snow depths you applied to the lakes. The observations in Table 3 show a wide range of values for the ratio SnoLake/SnoLand including zero values. I assume that you used some typical or average value of this ratio to apply to the CRCM output but I could not find this mentioned in the text. In general I found the documentation of the methodology used for handling snow to be rather confusing. I recommend you have a separate section in the methods section summarizing how snow cover is treated in the simulations.

13: Section 3.2.2, 3rd line: The spatial pattern does not change; all that changes are the mean values!

14: p. 1799 lines 5-7: Gridded data from lower latitudes have biases toward northern regions??? Not sure what you mean. You could cut out a lot of the material in Section 4.

15. In the Summary and Abstract the projected rates of ice cover change for 2050 seem rather modest in comparison to some of the recent observed trends. Is spatially averaging the results over the entire Arctic domain partially responsible for this? I would present the range in projected change as well as the average as there are significant differences in the ice duration changes projected for coastal regions versus the continental interior. This point should definitely be included in the abstract! You don't spend a lot of time on Fig. 12 but this is one of the few plots where there is a noticeable difference between the different mixing depths. There are also features like larger changes over the QEI (and Ungava Peninsula) than mainland Canada which is

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likely a response to decreased sea ice in CGCM3.

16. It would be useful to mention in the paper how CGCM3 projected changes in Arctic air temperature and precipitation compare to other CMIP3 GCMs. I think there is material on the IPCC website to address this.

References cited:

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Interactive comment on The Cryosphere Discuss., 5, 1775, 2011.

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