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Hydrology and Earth System Sciences
Discussions

Interactive comment on "The influence of albedo parameterization for improved lake ice simulation" *by* Alexis L. Robinson et al.

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We thank the reviewer for their time and effort in providing a constructive evaluation of our study and particularly for their comments regarding a better organization and relevant model details. The manuscript will be strengthened and improved by rearranging the methods section in particular to clearly outline the improvements made. We will aim to modify the manuscript as suggested throughout. Responses are listed below each comment.

R2C1: The manuscript does not feedback their findings well to the original albedo parameterization, which appears to fail to reproduce ice thickness and ice off dates in the temperate lakes, but instead, it appears that the albedos were simply tuned to

match the observed ice off dates and ice thickness in the temperate lakes. In addition, further tuning is done just for a specific season (2015-2016). The needs for these tunings indicate the opportunity of improvements in the original model but this has not been achieved in the way the work is presented.

Reply: To clarify, the albedo was only tuned for 2015-2016 to better fit the ice on /off dates – the main value used for the other years was collected from field data and the average value was used. The standard deviation was used to further explore the effects of the unusual snow year in 2015-2016 (albedo of 0.88 vs. 0.85) to account for the early/frequent snowfalls in that unusual year. In 2015-16 the climate data shows that this winter was warmer and had earlier season snowfalls that exceed those in 2016-17 and 2017-18. However, no albedo was recorded in the 2015-16 season. This season had deeper earlier season snow (Table 2) and a lower snow density (Table 2) more indicative of fresh snow, which would have a higher albedo compared to the snow density values measured in 2016-17 and 2017-18 during the same January period. Therefore, our reasoning was to increase the snow albedo value to account for the fresher snow that was measured in 2015-16.

This was not explained well on our part. We will revise to clarify, in particular by moving the supplemental figure to the main manuscript to show clearly the minor difference between using the same albedo for all years, rather than fine tuning the 1 year with no albedo data collected. As suggested by Reviewer #1 we will also add the model metrics to the interim simulations leading up to the final adjustments to clearly show the effect of each individual albedo adjustment.

Reviewer: Surprisingly, the fixed albedo values 0.75 (pre-melt) is equivalent for the albedo with 4.5m thick ice in eq. (3), and 0.56 (melting) is about the upper bound in eq. (4). The authors attribute the failure of the original parameterization to white ice in temperate lakes not taken into account by the model. This might be the case, but if so, this is a shortfall of the parameterization, which should actually be latitude-dependent, or include white ice. The model by Duguay (2003) appears to include

snow ice (white ice) so including white ice in the albedo parameterization appears to be straight forward. The authors increased snow albedo just for the 2015-2016 season because of the massive snowfall earlier in the season, but doesn't this mean αs in eq. (5) should be snow-depth dependent rather than altering αs from year to year?

Reply: Potentially, yes, but there is not enough field data to conclusively quantify the snow albedo – snow depth relationship for the study area. There are no albedo data for the 2015-16 season, and the snow melts frequently in this region so continuous on-ice data would be needed. Snow depth on lake ice are very rare measurements to have in a dataset, so including this as a required value to run the model would limit the applications – the overall goal is an acceptable modification to be widely applicable to temperate regions. The 0.88 value was more meant to represent the large fresh snow than the depth per se. We will reword the relevant text to clarify that 0.88 was an exploration to see if the different snow conditions could be represented.

Reviewer: First, I suggest the authors provide a figure illustrating the relation among ice thickness, snow depth, surface temperature and bulk albedo for the original parameterization (yes it's multi-dimension but there can be a few ways for this, such as Figure 4 in Icepack Documentation, https://media.readthedocs.org/pdf/cice-consortiumicepack/icepack1.2.2/cice-consortium-icepack.pdf). Next, I suggest that the authors add data points of their observed albedos, as they have synchronized observations for snow depth, ice thickness, and albedos.

Reply: This is a great idea, thank you, we will explore this and work in a new figure to better explain the current parameterisation in conjunction with our field data. A new project is starting in our group this fall that focusses more heavily on the snow depth/ice thickness/albedo relationship. We have only limited data at the moment, but will ideally be able to present a more conclusive story of the relationships in the temperature regions after a few more years of data are added (currently 3 years, aiming for 2 more to capture more climate variability).

Reviewer: Finally, I suggest that the authors propose a new set of equations which includes white ice (ideally), or is latitude dependent (this could simply be another if branch). The improved parameterization would be a valuable contribution to lake ice modeling.

Reply: With respect to the snow ice currently parametrized in the model – this is based on the typical mass related slushing that would occur on northern lakes and does not capture melt/refreeze that occurs in the temperature regions (Ariano and Brown, 2019), hence we are not focussing on quantifying/validating the current snow ice parameterization. This is beyond the scope of this paper; however, it is the focus of an upcoming research project, aiming to quantify the white ice formation from the multiple mechanisms possible in the temperate regions. We can quantify how much white ice is present, but we cannot currently separate the formation mechanism – typical slushing or melt water refreeze and hence cannot parameterize it correctly yet. The end goal is to account for geographic location (temperate vs. northern) in the selection of which albedo values to use in the simulations. The current paper is the first exploration of adjusting the model for temperature regions to represent the overall ice thickness and timing. Future work will delve into the composition complexities.

R2C2: More details for the model description are needed. 'Mixing depth' appeared in section 3.5 but there is no description for what this does with the model. If the model includes 'snow-ice' (or white ice) parameterization, this should be stated in section 3.4. How many layers are defined? Clarify that 'the vertical coordinate 0' means the interface between the air and the snow or ice? Is there any heat flux from water to the bottom of ice? I understand that many of them are described in Duguay et al (2003) but this paper should provide at least minimum of the key information.

Reply: Thank you for highlighting the missing information. A fixed mixing depth is used to in CLIMo to represent the mixed layer depth. In CLIMO, when ice is present, the mixing depth layer is fixed at the freezing point, otherwise when ice is absent, the mixing layer temperature is computed from the surface energy budget (Duguay et al.,

2003). We will add a brief description of the mixing depth effects, layers and the heat flux. (Snow ice comment addressed above).

R2C3: The organization should be carefully reviewed. Multiple descriptions are misplaced. For example, section 2 should be about geography and relevant background knowledge for the study areas but it extends to descriptions on CID and Snow CD (data used in this study), which should belong to section 3 “Data and Methodology”. Adjustments to albedo in page 9 (section 3.5, simulation) should belong to section 3.4.1 (albedo parameterization).

Reply: Thank you for your suggestions, we will revise as suggested while factoring in some reorganization suggested by Reviewer #1 as well. We will be dividing the methods section describing the albedo in CLIMo into ‘unadjusted’ and ‘adjusted’ to clearly outline the changes and field data collection.

Other points: Reviewer: Page 1, L15: “the High Arctic ice cover” should be “ice cover in the High Arctic lakes”

Reply: This will be revised to “Simulations of High Arctic lake ice cover. . .”

Reviewer: Page 1, L17: The meaning of “underestimation” of “ice-off timing” is not clear.

Reply: Underestimation of ice-off timing refers to simulated complete ice-off (break-up timing) occurring earlier than actual (from camera imagery and the SWIP) complete ice-off (water-clear of ice). This section will be reworded to match Section 3.1 terminology where break-up and ice-off are defined as complete ice-off.

Reviewer: Page 7, eq. (3): Please define ‘h’.

Reply: This will be revised to “h is the total thickness of the snow and ice layers”

Reviewer: Page 8, L211: Does ‘the vertical coordinate 0’ mean the air-ice/snow interface? How many layers were defined?

Reply: the standard 5 layers used in CLIMo for previous research were held for these simulations as well. Where one layer represents the snow and four represent the ice. We will clearly outline this in the text where indicated.

Reviewer: Page 9, L265: “However, for the study year 2015-2016 the snow albedo was increased to 0.88 to better predict ice-off dates as this season had more early-season snow on the ice”. I don’t understand this reasoning. Doesn’t this simply mean the parameterization should include the increase of snow albedo with snow depth?

Reply: (See above)

Reviewer: Page 11, L312: Please define ‘Ia’.

Reply: This will be revised and added to Section 3.6 Model Performance Line 276: “. . .model performance was measured using the Index of Agreement (Ia) in the R package ‘HydroGOF’ (Ia; standardized measure of the degree of model prediction error which varies between 0 and 1, where 1 indicates perfect agreement; Willmott, 1981; Zambrano-Bigiarini, 2017).”

Reviewer: Figure 1: It’d be helpful if bathymetry information for the lakes are added. No mean depth info for the High Arctic lakes? I see that they are provided for the temperate lakes in section 2.2.

Reply: Since the model is representing the lake as a whole the bathymetry does not contribute greatly and adding the bathymetry would be a substantial undertaking as the maps are not available digitally. We will endeavour to include a reasonable estimate for the mean depth of the Arctic lakes based on the existing bathymetry maps from research work in the area, so that all four research lakes have mean and max depth provided.

Reviewer: Figure 6: I’d like to see thickness timeseries for the simulations in the High Arctic lakes as well. Do they capture the feature in the historical observations described in section 2.1?

Reply: Maximum ice thickness is provided for Resolute Lake between 1960 to 1984 in the Canadian Ice Database (CID; Lenormand et al., 2002), however no date is recorded for when the measurement was taken. For this reason, no ice thickness was included for Resolute Lake since we could not determine the accuracy of the daily thickness measurements for the model. With regards to Small Lake, no thickness measurements were recorded in the CID, however, we currently have a shallow water ice profiler (SWIP) deployed which is recording the full evolution of ice cover in this lake and plan to use this data for future comparison with the model to determine the accuracy of the ice thickness output. We will experiment with presentation ideas to include a dateless maximum thickness value to our thickness output, however, as mentioned there is no way to validate these simulations at this time.

Reviewer: More information on forcing (air temperature, wind, snowfall) would be appreciated. Maybe timeseries graphic or providing mean values for each season.

Reply: We will add the air temperature and snow data to the figure (similar to how we have done in Ariano and Brown, 2019) and explore the viability of adding albedo data as well. This should further highlight the benefits of the adjusted model as the climate link will be visually evident.

References:

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