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Discussions

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

**Anonymous Referee #1**

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Review of the manuscript entitled " the influence of albedo parameterization for improved lake ice simulation " by Alexis L. Robinson, Sarah S. Ariano and Laura C. Brown.

Lake ice phenology is a key physical parameter both as a signature of climate changes and as a driver of lake ecosystem changes. Yet, lake ice remains complicated to model as it requires to couple ice/snow optical and mechanical properties to thermodynamic principles. The authors showed that the performance of a well-established thermodynamic model for lake ice simulations can be significantly improved by local information on the albedo. While the importance of the albedo has been long identified as key parameters regulating the heat flux (Leppäranta, 1993 for instance), deterministic models still struggle to correctly reproduce optical properties of the ice. In that sense, this



contribution is very relevant.

I have however several issues with the present manuscript. I found this manuscript very hard to follow and identify how the main goal is achieved.

1) Model description and discussion

If I understood correctly the main objective of this work is to show that the model performs better when the temporal evolution of the local albedo information is provided. While this is not a fundamental surprise that local forcing conditions are always better than global or random boundary parameters, I recognize that it is important to quantify the sensitivity of a model to such a parameter. Yet, the way the model is presented (Eqs. 1-5) is very confusing. For instance, the boundary conditions (especially the lower one at the ice-water interface) of the model are not presented. Off course, such information can be found in previous publications but the manuscript should be self-explicit. Furthermore, the authors presented 3 related equations to parameterize the albedo. They discuss optimization but it is very hard to understand what was really done. Specifically, there is no parameters in the three equations provided. It is thereby very hard to follow the changes described in the results. I think that the way the work is presented makes the study difficult for other scientists to follow and finally reproduce the findings. I thereby recommend to the authors to rewrite the results and discussion and reformulate the set of equations and finally discuss how the parameterizations have been modified to improve model's skill so that readers can understand what is modified.

2) Lower boundary condition

I have not understood how the flux at the lower boundary was calculated. This heat flux is proportional to the water temperature, which will evolve from let's say 0.1°C to 4°C, that is a 40 times increase in the heat flux. Said differently, the heat flux will range from < ∼1 W/m2 to > 10 W/m2 (without daily cycles). How is this increase over time of heat flux from the lower boundary taken into account? This boundary condition and its variability is not discussed. Without a proper quantification or at least discussion regarding the lower boundary, one may think that changes in albedo parameterization are actually also taking into account changes in heat flux at the lower boundary.

3)Ice-on prediction

I have also not understood how this model can be used to predict ice-on as discussed in the results and discussion section. How much heat must be extracted from the lake before reaching the condition T=0°C? What are the initial conditions being used? The modeling approach is adapted to ice thickness and ice-off but not to ice-on.

4)Albedo

I am curious why the authors did not assimilate the observed albedo using Ensemble Kalman Filters or any other approach.

I also would need some clarifications regarding how albedo is actually measured. Some publications show huge daily variability (due to solar angle, melting and consequently daily changes in ice properties). It seems that you focus mostly on the seasonal variability that is the change from snow covered to snow free and further increase of the scattering with ice warming. I am curious if the daily variability is relevant or not.

Finally, it is mentioned many times that albedo drives the melting. This is potentially misleading. Solar radiation (and air temperature) drive(s) the melting. The albedo modulates the intensity of the forcing (see for instance a very cloudy day with -20°C but with very small albedo vs sunny day with +10°C with larger albedo).

5)Model skill metric

I suggest to develop the model's skill metric as a function of the calibration parameters to show the effective improvement of the new model's version. The authors actually discuss this in section 3.6 Model Performance, but I don't see the indexes in a figure, or how the calibrated parameters achieved an optimal value. The metrics are just shown in Tables 6 and 10, so we need to assume that these are the maximum values



achieved, but what are the optimal calibrated parameters?

6) Non exhaustive specific comments

*Abstract: first sentence not really attractive

*L10 "northern and temperate ice": odd formulation

*L28: why "exponentially"? More lakes up north?

*L43 "ice melt initiation is controlled by albedo". Please clearly stress that the drivers are solar radiation and air temperature.

*L45: Please distinguish main drivers to the secondary drivers for clarity reason

*Eq 3-5: Please add parameters instead of numeric values as you will be changing them. What is "m"?

*L246: "mixing depth" What is this? How many parameters do you have in the model? A table with the list of parameters with chosen values is needed. Very important for reproducibility reasons.

*L264 the standard deviation seems very small. Other studies report more that ∼0.3 daily variability in albedo due to continuous changes of solar angle, cloudiness (scattering of downwelling radiation), and ice properties (melting, increase in the gas fractions) over the day.

*Figure 1. Latitude and longitude would help

*Figure 4. For model comparison purpose, the y axis is not ideal. I would split into 2 plots (and potentially remove ice on)

*Figure 6. Very nice figure. Maybe add the forcing (Tair, W) and the albedo time series used for "unadjusted" and "adjusted" cases. This is the central figure in my view. The performances of the "adjusted" model are impressive.

*Eq. (1). Is a reference density or is it considered a dynamical variable?

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