Answers to tc-2022-266 RC1

May 18th, 2023

Note:

- The referee comments are shown in black,
- The authors answers are shown in blue,
- *Quoted texts from the revised manuscript are shown in italic and in dark blue.*

- Note that the exact pages and line numbers in our responses are subjected to change as the revised manuscript is being prepared.

Review on "Using Icepack to reproduce Ice Mass Balance buoy observations in land-fast ice: improvements from the mushy layer thermodynamics" by Plante et al.,

This manuscript needs a major revision or possible resubmission to the TC.

Undoubtedly, this research subject is important and is of potential interest to TC readers. This manuscript contains the following key elements: a) Icepack (v1.1.0); b) ice mass balance buoy (SAMS IMB), c) land-fast sea ice in Canadian Arctic Archipelago (CAA) and d) mushy layer~ slush layer (mixture of snow and ice).

By the way, please use "SIMBA (snow and ice mass balance apparatus)" in the revised manuscript to present SAMS IMB since this acronym has been used in many papers to name SAMS IMB.

We change any reference to the SAMS IMB for "SIMBA" in the revised manuscript. Note that we nonetheless keep the term IMB to refer more generally to ice mass balance buoys (for instance describing our algorithm, which we also use for SIMB3 buoy data).

The authors presented the Icepack model; processed the SIMBA data (observations) using a newly developed automatic SIMBA algorithm based on existing methods; simulated ice thickness (calculations) using the Icepack model; Summarized results (observations and calculations); Concluded that the modelled ice thickness is better when applying a mushy layer parameterization; pointed out the simulation errors and give suggestions on further actions. The

storyline of this manuscript seems ok, but the presentation suffers various ambiguities and makes it difficult to follow and understand.

We thank the reviewer for their useful review, and address their comments below:

Several major comments:

1 What is the relationship between Icepack1.1.0 and Bitz and Lipscomb's (1999) thermodynamics model? To my understanding, CICE is a 2D dynamic-thermodynamic sea ice model developed by the Los Alamos National Laboratory. Icepack 1.1.0 is the one-dimensional module of the CICE model. Bitz and Lipscomb (1999) is an independent one-dimensional thermodynamic sea ice model. Please clarify those models and present clearly how they support each other.

Icepack is the thermodynamics component of the CICE6 model. It was coded as a package separated from the model dynamics, so that it can be used separately as a column thermodynamic model. It is managed as part of the CICE consortium, and includes a wide variety of optional parameterizations. Thus, Icepack does not refer to a specific set of equation (as opposed to the BL99 or mushy layer parameterizations), and it can be used with different choices of thermodynamics, such as the 0-layer (Semtner, 1976) thermodynamics, the Bitz and Lipscomb (1999) thermodynamics, or the mushy layer thermodynamics (Feltham et al., 2006, Turner et al., 2013). In this analysis, we use the BL99 parameterization as the standard choice in previous CICE versions (and most particularly in the ECCC forecast systems), and test the improvement brought by the use of the mushy layer thermodynamics. This is clarified at L131-136 in the revised manuscript.

2) Are you trying to develop Icepack or simply to validate Icepack using SIMBA observations? Why is Bitz and Lipscomb's (1999) scheme mentioned separately?

The goal here is to assess the impact of upgrading the thermodynamics in all ECCC systems from the Bitz and Lipscomb (1999) to the mushy layer physics, especially in the landfast ice areas. This is clarified at L131-136 in the revised manuscript.

3a) The paper structure is not clear. The current chapters 2 and 3 mixture of many things and need to be reconstructed. One possibility could be

2 Data
Describe the data used in this study
2.1 Weather data
Describe weather conditions
2.2 SIMBA data
Describe SIMBA deployment and data
3 Method
Describe the model/algorithm used in the study
3.1 Icepack model
Surface energy budget

Heat conduction in snow and ice
Bottom heat and mass balance
Snow-ice interaction
3.2 SIMBA algorithm

I would like to see a sub-section dealing with the weather data.

We agree that the manuscript would benefit from a reorganization, especially with the added information such as the weather data. In the revised manuscript, we took the reviewer's suggestion of adding a data section, in which we present the SIMBA buoys and the atmospheric data used to force the Icepack simulations. We also reorganized the method section and added a new subsection in the results section about the in-situ meteorological conditions. We however kept the model description separated from the method section, so that the latter focuses on methods specific to our experiments and analysis.

3b) The result chapter needs significant updates too.

I would like to see a sub-section presenting analyses of weather data. This is very important for readers to understand your model performance and the snow-ice interactions. The weather part is missing entirely both in the data and result sections.

The observed weather conditions are now included in the revised manuscript in a new subsection (5.1), as suggested by the reviewer. We also included two new figures to show the correspondence between the GDPS data (used to force the 1D simulations), the surface air temperature recorded in-situ by the IMB, and precipitation recorded at a nearby weather station.

Do you have ice core samples to show how the snow ice was distributed vertically? It would be interesting to add some on-site photos.

Sadly, we have neither ice cores from this location nor pictures from the deployed IMBs. This project has a primary purpose to provide in-situ information about the landfast ice conditions along the snowmobile routes used by the local community. It is thus not supported by a wider scientific observation campaign. This is now specified at L94-98 in the revised manuscript. We would of course like to eventually sample ice cores at the site (for instance to better quantify the contribution of snow-ice and meteoric ice in the mass balance) and other types of in-situ observations (e.g., ice stress measurements) in future deployments, but this remains uncertain due to logistic challenges.

4) Several figures can be improved.

1. a) Figure 1 is not very representative. Please show a much larger domain so readers can better understand the region's geography. What is the distance between those two SIMBAs? What are the air temperatures and precipitation patterns of those two sites?

Figure 1 (included below) showed both a large (1000s km) and a local (~50km) domain, and we added a medium-size domain that better shows the region orography and the location with respect to the landfast ice edge and added some geographical references. The distance between the buoys is now included in the caption. The air temperature and precipitations during the observation period are presented in the new section 5.1 in the revised manuscript.

2. b) Figure 7-12 need revisions. Can authors make those figures to be consistent with the SIMBA figures? The figure captions need improvement for better clarity. Some of the results lines need to be smoothed, e.g., 5-day running mean.

We added references to the ice thicknesses, snow depth and snow-ice in the SIMBA schematics, so that the reader can better relate the time series in Figs. 7-12 to the observations. We also decided to add new Figures that show the simulated temperature profiles, as in Fig. 1 and 3b-c.

Concerning the smoothing or running mean, we believe that the reviewer refers to the fact that some of the observations had a step-like look, which partly came the way we assigned the daily retrievals to the 6 hourly data, without interpolation. This was not ideal, and we improved our data processing in the revised manuscript: we now use the surface retrieval algorithm at a 6hourly interval and apply a 24h running on the data. This effectively gets rid of the step-like behavior in all figures (see for instance the updated Fig. 7, included here below). Note however, that some of the spike-like behavior remains, especially when looking at the ice congelation due to the 2 cm uncertainty (spacing between sensors). We prefer not to use a 5-day running mean, since the intervals between data gaps are already small.

5) Surface retrieval algorithm validation: Could authors perform some statistical analyses to give a concrete assessment of your algorithm performance?

This is difficult as there is no “ground truth” data to validate against at these locations. This is also why we use a conservative measure of uncertainties (plus or minus 2 cm) for the retrieved interfaces and discuss the algorithm validation by visual interpretation. This limitation is now specified at L349-350 in the revised manuscript. We also note that vertical-gradient based algorithms similar to ours were recently thoroughly validated against other methods (visual inspection or based on the temporal evolution of each sensor) and shown to be the most robust for the ice thickness retrieval (Richter et al. 2022). This comment is added at L.227-232 in the revised manuscript.

6) section 4.2 (In situ ice mass balance conditions) should be moved to the data section.

We prefer to keep our in-situ observations in the result section, as this is an intrinsic part of our analysis. We however took your suggestion to move the description of the buoy and its deployment in a new data section.

7) Icepack simulations section looks weak. I see a description of the results, but please carry out some in-depth analyses.

We keep the more in-depth discussions about our results for the discussion section, where we present in depth analysis of the simulated processes and changes when the mushy layer physics is used. This includes results on the snow ice formation process, sea ice congelation and snow-variations.

In the revised manuscript, we strengthen the result section by adding a new section on the observed weather conditions and re-organizing it into subsection to discuss separately results from the BL99 and mushy layer thermodynamics. We added results on the simulated ice congelation, internal temperatures and the new simulations with variations on the snow-flooding rates. We also computed the Mean Integrated Errors in ice thickness and ice congelation to discuss the performance of each simulation.

8) The discussion section looks weak too. I would like to see some tables and comparisons with other studies. I am sure there are a lot of land-fast sea ice modelling papers and snow-ice simulations. Please make some concrete discussions.

We argue that the discussion is addressing many relevant points for future model development and for the landfast ice thermodynamics community. We discuss the observation of negative freeboards in light of a number of similar recent reports, the importance of snow ice formation on the simulated thermodynamics despite the fact that is remains often described as mostly an Antarctic process and offer alternatives to better represent the observed slow flooding by a porosity criteria or new parameterizations to relate the flooding to the model dynamics. In the revised manuscript, we further add a discussion on limitations when representing snow-ice in the km-scale of dynamical models, and new references to former studies (for instance incorporating results from Duarte et al., 2020, DuVivier et al., 2020).

We also note that there is very few data and analysis about the landfast ice along the Labrador coast. While we discuss our results in light of recent snow-ice formation observations from the NICE15 campaign, North of Svalbard, we do not believe that comparing our observed thicknesses to other IMB data from different locations would be meaningful, given the widely different regions, years and conditions. We prefer to keep the focus on the processes that are better or still miss-represented by the mushy layer thermodynamics.

9) “Code and data availability. All codes (model and analysis) are available on github upon request. The buoy data are available upon request.” I think this statement is not acceptable to the TC. Please make your code and data available with doi link or weblink.

Of course. Our Icepack model and diagnostic python codes are available on github and the SIMBA data are on Zenodo, with the links included the revised manuscript.

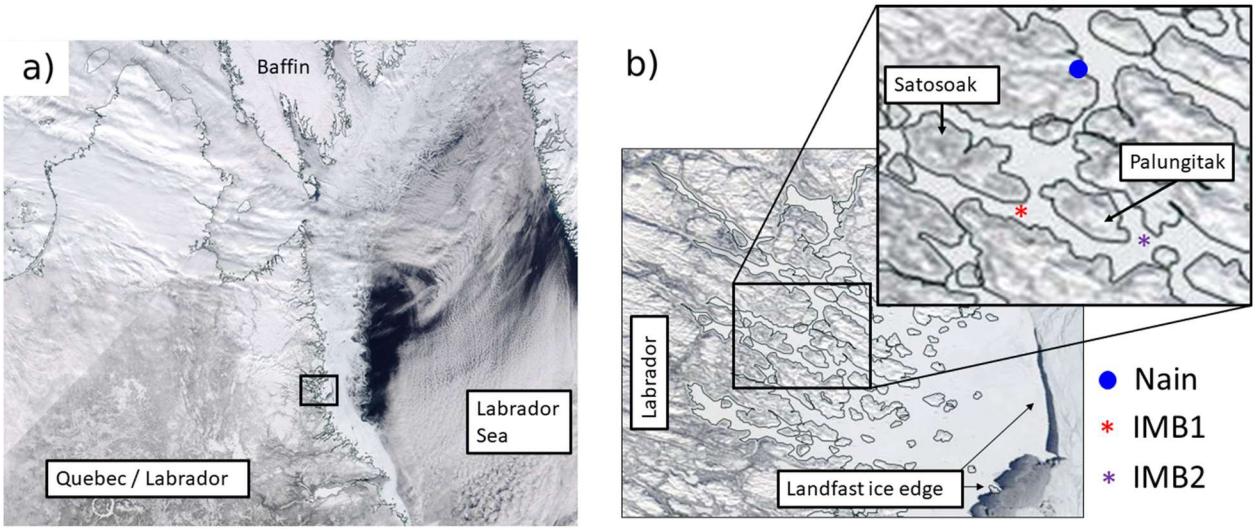

Figure 1. Location of the two IMB buoys on the Labrador coast (a), in a landfast ice channel close to the Nain community (b). The buoys are located at ~56.42˚ N, 61.7˚ W (IMB1) and ~56.43◦ N, 61.50◦W,~ (IMB2), 12 km from each other and ~50 km from the nearest landfast ice edgeImages are corrected reflectance imagery taken from MODIS worldview (https://earthdata.nasa.gov/labs/worldview/).

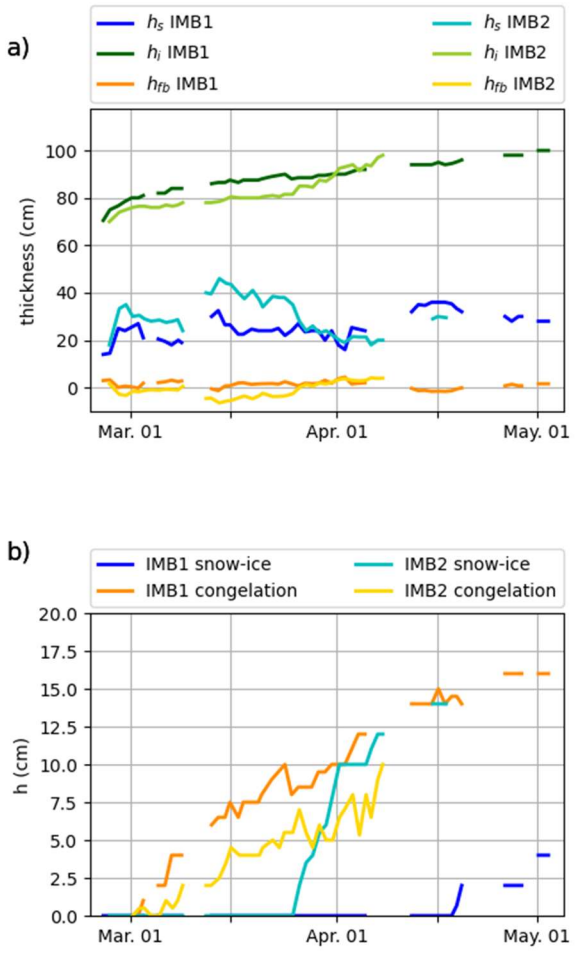

Figure 7.a) Snow (blue lines), ice (green lines) and freeboard (orange lines) thicknesses from the IMB observations. b) Contribution of snow-ice (blue lines) and congelation (orange lines) to the ice mass balance inferred from the IMB observations.

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