Authors answers to tc-2022-266 comments

September 7th, 2023

Dear Editor,

We are pleased to submit our revised manuscript "Using Icepack to reproduce Ice Mass Balance buoy observations in land-fast ice: improvements from the mushy layer thermodynamics" to The Cryosphere.

We would like to thank the reviewers for their useful comments and suggestions. We made extensive modifications to the manuscript according to most of these suggestions. This helped improve the content, originality, and clarity of the article substantially. In particular, the scope of the analysis has been extended to include a comprehensive assessment of the mushy layer congelation and snow-ice parameterizations.

Based on the reviewer's comments, we investigated thoroughly the mushy layer thermodynamics at the ice-ocean interface and found that significant amounts of frazil are formed during congelation in our simulations. This was missing in our analysis and our results have been updated accordingly. This finding also led to significant work as part of the revisions to identify and comprehend the source of this frazil formation. After discussions with members of the CICE consortium (of which Adrian K. Turner is added here as co-author), we now propose a modification to the mushy layer congelation parameterization that improves its performance. We believe that this addition to the manuscript is in line with some of the reviewers' comments and represents a major contribution to the ice modeling community.

Thank you for your consideration for publication.

Sincerely,

On behalf of all the authors,
Mathieu Plante

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.*
- Amendments made to the responses in the open discussion are shown in green

Answers to tc-2022-266 RC1

September 7th, 2023

Note:

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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 somtimes 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 and 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 equations (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 L144-145 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 L82-83 and L144-145 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 and the new congelation parameterization. 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 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. These figures are added as pannels (a) and (b) in Figure 3 of the revised manuscript.

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 L108-110 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 showed both a large (1000s km) and a local (~50km) domain, and we added a medium-size domain (revised figure is included below) that better shows the region orography and the location with respect to the landfast ice edge. We also 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 thickness, 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. E.g., Fig. 5 in the revised manuscript shows the vertical temperature profiles from the 4 control simulations and Fig. 9 shows the profiles in mushy layer simulations with different snow flooding onset criteria.

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 each 6 hours, then apply a 24h running mean on the data. This effectively gets rid of the steps-like behavior in all figures (see for instance Fig. 5 in the revised manuscript, included here below). Note however, that some spike-like behavior remains, especially when looking at the ice congelation, due to the 2 cm uncertainty associated with the spacing between sensors. We prefer not to use a 5-day running mean, since intervals with valid data between 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 L425 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.307-309 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.

In the revised manuscript, we significantly widen the scope of the paper by adding simulations with modified mushy layer parameterizations (snow-ice and congelation). The result section is edited with a new section on the observed weather conditions and re-organized it into subsections to discuss separately results from the BL99 and mushy layer thermodynamics. We added the results on the simulated ice congelation, internal temperatures and snow-flooding. We also computed the Mean Integrated Errors in ice thickness, snow depth, ice congelation and snow-ice formation to quantify 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 it remains often described as mostly an Antarctic process and offer alternatives to better represent the observed slow flooding by a porosity criteria and 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 N-ICE15 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.

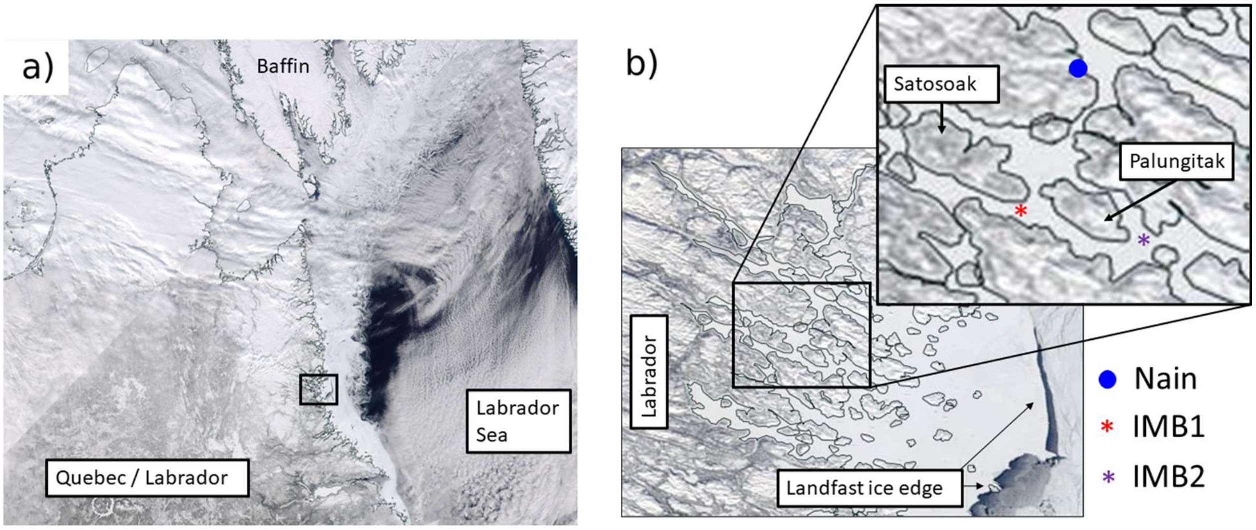

Figure 1. Location of the two IMB buoys on the Labrador coast (a), in a landfast ice channel close to the Nain community (b). The buoys are located at ~56.42˚ N, 61.7˚ W (IMB1) and ~56.43◦ N, 61.50◦W,~ (IMB2), 12 km from each other and ~50 km from the nearest landfast ice edgeImages are corrected reflectance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

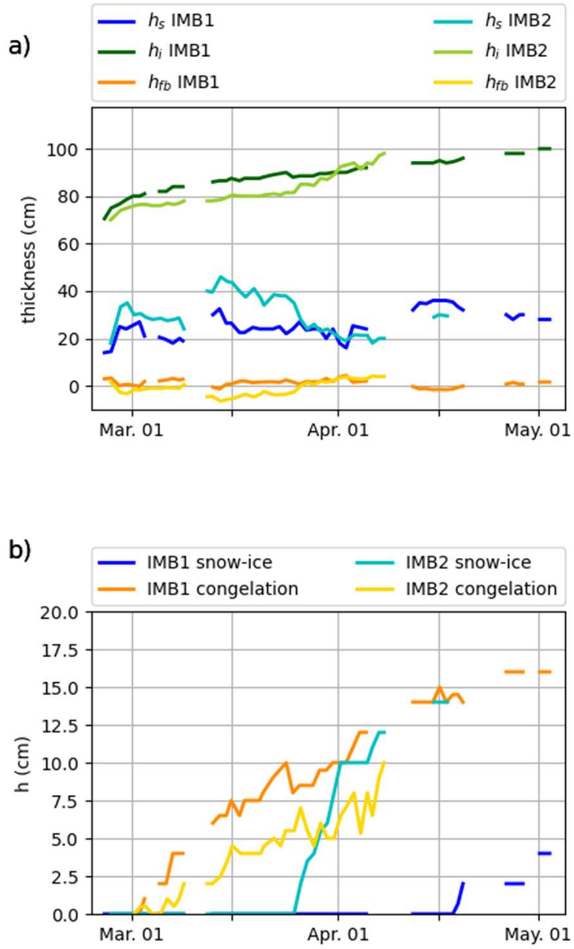

Figure 7.a) Snow (blue lines), ice (green lines) and freeboard (orange lines) thicknesses from the IMB observations. b) Contribution of snow-ice (blue lines) and congelation (orange lines) to the ice mass balance inferred from the IMB observations.

References:

Bitz, C. M. and Lipscomb, W. H.: An energy-conserving thermodynamic model of sea ice, Journal of Geophysical Research: Oceans, 104,15 669–15 677, https://doi.org/10.1029/1999JC900100, 1999.

Duarte, P., Sundfjord, A., Meyer, A., Hudson, S. R., Spreen, G., & Smedsrud, L. H. (2020). Warm Atlantic water explains observed sea ice melt rates north of Svalbard. Journal of Geophysical Research: Oceans, 125, e2019JC015662. https://doi.org/10.1029/2019JC015662

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Richter, M., Leonard, G., Smith, I., Langhorne, P., Mahoney, A., & Parry, M. (2022). Accuracy and precision when deriving sea-ice thickness from thermistor strings: A comparison of methods. *Journal of Glaciology,* 1-20. doi:10.1017/jog.2022.108

Semtner, A. J.: A Model for the Thermodynamic Growth of Sea Ice in Numerical Investigations of Climate, Journal of Physical Oceanography, 6, 379 – 389, https://doi.org/10.1175/1520-0485(1976)006<0379:AMFTTG>2.0.CO;2, 1976

Turner, A. K. and Hunke, E. C.: Impacts of a mushy-layer thermodynamic approach in global sea-ice simulations using the CICE sea-ice model, Journal of Geophysical Research: Oceans, 120, 1253–1275, https://doi.org/https://doi.org/10.1002/2014JC010358, 2015

Answers to tc-2022-266 RC2

September 7th, 2023

Note:

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

In this paper model simulations using Icepack are compared with Ice Mass Balance (IMB) buoy data from land-fast ice close to Nain (Nunatsiavut), on the Labrador coast. A new algorithm is presented to determine ice thickness and snow depths from the measured vertical temperature profiles in IMB buoys. Model simulations were run with two different thermodynamic formulations: the Bitz & Lipscomb (1999) and the mushy layer. One of the purposes of this study was to evaluate the performance of the former which is used in Environment and Climate Change Canada (ECCC) ice-ocean systems, and the improvements that may be expected from the latter.

Whereas the mushy thermodynamics generally outperformed the Bits and Lipscomb approach, both were unable to reproduce delayed snow-ice formation as a result of relying on hydrostatic balance and not allowing for negative freeboards.

In the following paragraphs I present my general comments. Minor comments are incorporated directly on the manuscript.

We thank the reviewer for their careful review of the manuscript and address all major and minor comments below.

General comments

The paper is very well written, and its contents are extremely clear. It is also well presented. Whilst the subject is not original, since comparisons between these two thermodynamic approaches were already carried out (e.g. Turner & Hunke, 2015; Bailey et al., 2020; DuVivier et al., 2021 – by the way, I suggest incorporating also the main achievements of DuVivier et al in a revised version of the paper), such comparisons were made using regional or global simulations, some with coupled models, introducing a number of factors that make it more difficult to disentangle the "pure" thermodynamic effects resulting from these two schemes. Here, 1D vertically resolved simulations are used, focused on thermodynamic processes alone. Moreover, results are compared with those of IBM buoys that provide a lot of spatial and temporal detail, regarding temperature and thickness of the snow, the snow-ice and the congelation ice.

Thank you for these comments. Indeed, the originality of our manuscript lies in our smaller-scale and higher time-resolution take on thermodynamic processes that were previously discussed in climate or pan-Arctic simulations, such as in DuVivier et al., (2021). This was insufficiently highlighted in the manuscript, and we modified the introduction to better convey this point in the revised manuscript. We also re-organized the model and results section to better focus on the influence of the mushy thermodynamics on specific processes, by first describing the BL99 model and simulations, then the differences when using the mushy layer physics. Note that in the revised manuscript, we include an in depth analysis of the formation of frazil associated with congelation when using the mushy layer simulations, which is not observed nor expected in the landfast ice cover far from the flaw polynya. We believe that this is a new major contribution that positively improves the originality of the manuscript.

The authors focus on possible thermodynamic reasons to explain the problems in reproducing delayed snow-ice formation. Whilst I am not criticizing this focus, I wonder if the problem here is mainly thermodynamic or mechanic, related for example with ice floe deformation. In fact, this possibility is mentioned in lines L374-375.

Our focus on thermodynamics stems from the fact that we are studying the landfast ice in a narrow channel, dozens of kilometers away from the landfast ice edge and well sheltered from offshore dynamics. It is thus safe to assume that the pack ice and marginal ice zone processes included in current dynamical models are not involved. This is clarified at L82-83 and L579-582 in the revised manuscript. Naturally, other dynamical processes (such as tidal or thermal cracking) that are usually not included in dynamical models cannot be ruled out entirely. We believe that these effects could be included as part of the 1D column model in future work by developing statistical sub-grid parameterizations. As the reviewer pointed, this was mentioned in the submitted manuscript, but is further addressed in the revised manuscript:

> *"This could indicate the influence of nearby sea ice dynamics although in our case, the deployed IMBs were located in a well sheltered landfast channel dozens of kilometers away from the landfast ice edge. Moreover, the slow rate of snow-ice formation corresponds well with percolation through the porous sea ice medium (i.e., as opposed to the sudden flooding expected when flood water is advected laterally from neighboring deformation sites, Provost et al., 2017)."*

On the other hand, it occurs to me (perhaps wrongly…) that when snow-ice is formed from the edges of an ice flow, this will change the porosity, making it more difficult for the water to penetrate further into the ice flow and continue snow-ice production. Such processes cannot be captured in 1D vertical simulations but may possibly be parameterized in 3D experiments. I guess some discussion about these aspects should be included.

In the submitted manuscript, we discuss the porosity of the landfast ice only in terms of the small-scale porosity associated with the brine channels, allowing for the vertical percolation of ocean water through the mushy medium. However, it is true that at the km-scale, the sea ice porosity can also be associated with the presence of cracks in the sub-grid scale, also allowing for the vertical percolation of ocean water. This larger-scale porosity is not taken into account when imposing a minimum mushy porosity criterion for snow flooding to occur. This is now discussed in section 6 (Discussions) in the revised manuscript.

As the reviewer points out, the lateral advection of flood water cannot be fully resolved in a 1D model. However, given the heterogeneity of sea ice at the km-scale of most dynamical sea ice models, it is likely possible to represent the likeliness of such water penetration by a sub-grid parameterization, similar to those representing melt ponds. The volume of ice formed in this km-scale will likely not be uniform over the grid-cell area and depend on the ability of the flood water to penetrate the snow layer, which will ultimately depend on the ice topography (ice thickness distribution), the snow conditions and the ice heterogeneity (i.e. the average distance between cracks). We also note that this heterogeneity is made evident in our results by the different in-situ conditions recorded by our two IMBs. These points are now discussed at L583-589 in the revised manuscript:

> *"One difficulty in reproducing the snow flooding onset with porosity criteria is that they do not account for a percolation associated with the larger-scale porosity (e.g. from thermal cracking) unrelated to the smaller scale mushy layer characteristics. At the km-scale of most dynamical sea ice models, the volume of snow-ice will likely not be uniform over a grid-cell area. This is made evident in our results by the different in-situ conditions recorded by our two neighboring IMBs. Most likely, the snow-ice volume will be spatially distributed according to the ability of the flood water to penetrate the snow layer, and ultimately depending on the ice topography (ice thickness distribution), local snow conditions and the ice heterogeneity (i.e. the presence and average distance between cracks). The snow-ice volume at this scale would thus likely be better represented by a subgrid parameterization relating the snow conversion to a spatial probability for water penetration."*

These problems of negative freeboard, flooding and snow-ice formation combining IBM and simulations with the CICE model were "touched" before by Duarte et al. (2020).

Yes, we thank you for bringing this study to our attention. Indeed, while they did not investigate the flooding process itself, Duarte et al., (2020) discussed the inadequacy of the hydrostatic balance criteria for snow flooding and resorted to "manually" switching the snow-ice parameterization on and off, depending on the observed conditions, to reproduce their IMB observations. This is now incorporated throughout the revised manuscript.

As far as I understood, the model was forced with re-analyses atmospheric data. Whilst I don't think this is the ideal forcing for such an experiment, since it may introduce bias in the results that may confound a bit the effects, I understand that *in situ* measurements would be hardly available. In any case, the uncertainties in the forcing should be addressed in the paper, without the need to get into major details.

The data used to compute the atmospheric fluxes are not from reanalysis, but from daily forecasts from the Global Deterministic Prediction system. More information about the atmospheric data is included in a new data section in the revised manuscript, L106-127. We also included new figures to show the correspondence between the GDPS and IMBs surface air temperature and compared the GDPS precipitations to those observed at the Nain airport ECCC weather station. These observations correspond well and would only cause minor effects on the simulated thermodynamics processes. This is discussed in section 5.1.1.

Moreover, it is unclear to me how did the authors managed the ocean forcing. I guess that water temperatures were taken from the measurement arrays of the IBM buoys. However, Icepack expects data on current velocities and heat fluxes in/to the ocean layer in direct contact with the sea ice (by the way, what was the thickness assumed for the ocean slab layer?). I did not find information about these details in the paper, and I think they should be included in a revised version. In fact, it would help to have graphs showing the time series for all forcing functions, even if only in Supplementary info.

Indeed, our use of the mixed layer parameterization was only briefly mentioned in the submitted manuscript and more information is now added at L161-167:

"Due to the absence of ocean salinity and currents observations at the buoy locations, no forcing data is used in our simulations to represent the oceanographic conditions underneath the ice. The ice-ocean fluxes at the ice bottom interface are defined from the mixed layer parameterization included in Icepack v.1.1.0., which determines the Sea Surface Temperature (SST) and heat exchanges between the sea ice and the ocean based on a fixed mixed layer depth, Sea Surface Salinity (SSS) and skin friction velocity. Here, we set the SSS to 33 PSU (a value coherent with our measured ocean surface temperature of ~-1.85 ℃), the mixed layer depth to 20m (default value) and the skin friction velocity to 0.005 m s^{-1} (the set minimum in Icepack). The SST is prognostic but initialized at the freezing point (as calculated from the liquidus)."

Note that in light of this comment from the reviewer, we decided to investigate thoroughly the ice-ocean interaction and congelation in our simulations. Doing so, we found that a significant portion of the ice mass balance was missing in the previous analysis, with some bottom melt and significant frazil formation that were not considered. These are only present in mushy layer simulations. We added significant work to show the source of these differences. We identified that the frazil formation results from the treatment of the freezing front in the mushy layer congelation scheme and is likely behind the large frazil (new ice) volume documented in previous studies using the mushy layer physics in the CICE model. We now propose a modification to the congelation scheme that better represents the observations. This work is included in the revised manuscript. The modifications to the manuscript associated with this change include a new subsection (3.3.3) to describe the modified congelation parameterization, new analysis and figures to describe the model sensitivity of the congelation parameters (section 5.4) and an Appendix where the treatment of the freezing front in the standard and modified parameterization is detailed.

Comparisons between model results and observations are presented only for “thickness-related” variables. I think these should include the modeled and observed temperature profiles as well. Once again, such comparisons may be added to Supplementary info.

In the submitted manuscript, we did not show the simulated internal temperatures (except at the lowest layer) as most of the differences can be attributed to differences in snow depth, ice thickness, and flooding. This was a point of criticism from all reviewers, which we address in the revised manuscript by adding new figures, analysis and discussions about the temperature profiles and salinity in both the results and discussion sections.

In particular, we:

- Add time-series of the simulated temperature profiles that can be compared to Fig. 3b-c from the observations (Fig. 6 in the revised manuscript).
- Add a time series of the temperature profiles in mushy layer simulations with different snow-flooding criteria. (Fig. 9 in revised manuscript) and of the top ice layer temperature, bulk salinity and brine salinity profiles (Fig. 10 in revised manuscript) to better discuss the effect of snow flooding.
- Add some time series of the bottom-ice temperature, bulk salinity, brine salinity and brine drainage strength to better discuss the impact of the mushy layer physics on the ice congelation rates (Fig. 11 in revised manuscript).

Moreover, comparisons between model results and observations were addressed only visually, and I suggest using some metrics for an objective comparative evaluation of both thermodynamic approaches.

We agree, and computed the Mean Integrated Errors in ice thickness, snow depth, congelation ice and snow-ice to provide quantitative references in the revised manuscript. These are included in new Tables 1 and 2.

The model was run with a 3-h time step. I wonder if authors checked the results sensitivity to the time step. Duarte et al. (2020) found out that very small time steps may be necessary to avoid bias in the sea ice energy budget fluxes. Interestingly, some of these biases may cancel each other, not affecting model performance when it comes to the prediction of sea ice thickness. However, they may become relevant in coupled models by biasing the feedbacks between the sea ice and the atmosphere, for example. I understand that forcing frequency may limit such verification in this case, but this is something to keep in mind in a revised version.

Our understanding is that the time-step dependency in Duarte et al. 2020 is related to their use of daily reanalysis for atmospheric forcing, and their seeing significant improvements when instead using the forcing data at 1min resolution. In our case, we are already using our forcing at its highest temporal resolution (3h, interpolated to the 1h timestep of our Icepack simulations). A better-resolved atmosphere would likely have a small impact on the simulations but would also likely affect all simulations similarly, such that we do not expect this to affect the main results and discussions provided in our manuscript.

As a final remark, I suggest transferring section 2 to Supplementary info, since most of its contents reproduce already published science (e.g., Hunke & Lipscomb, 2015).

We agree that this would be a good way to shorten the manuscript, but we prefer to keep it in the manuscript for reference when discussing the simulated processes, especially given the added contributions in the revised manuscript (modified congelation parameterization, discussions on the brine drainage).

Minor comments from PDF:

All comments were addressed in the revised manuscript. In particular:

- L175: Why 3 hours? : It is actually 1h, with outputs every 3h. This is the default timestep in Icepack. We kept with this value as it is already smaller than our forcing resolution, small enough to represent the diurnal cycle and computationally inexpensive. We corrected the statement and added this precision in the revised manuscript at L-396.
- L180: Which (community) needs? : This is mostly for the monitoring of ice conditions, near the on-ice snow-mobile routes. This statement was clarified at L-110 the revised manuscript.
- L233: It would help match Ts and Zs in Figure 2: Indeed, Zs and Ts are not used, and these references are removed from Fig. 2 in the revised manuscript.
- L325: What about the possible lack of variability in snow fall forcing, given its possible inaccuracies? The snow fall forcing corresponds well with the local observations. This is shown in a new figure in the revised manuscript (here below), as discussed in the new subsection on the weather conditions.
- L330: This could be better checked by switching off hydrostatic equilibrium. : This is what we actually do later in the analysis, in our simulations without the snow-ice parameterization. Switching off the hydrostatic equilibrium amounts to switching off the current snow-ice parameterization. Note that we added more in-depth analysis of the snow-ice onset and its impact on the simulated thermodynamics in the revised manuscript.

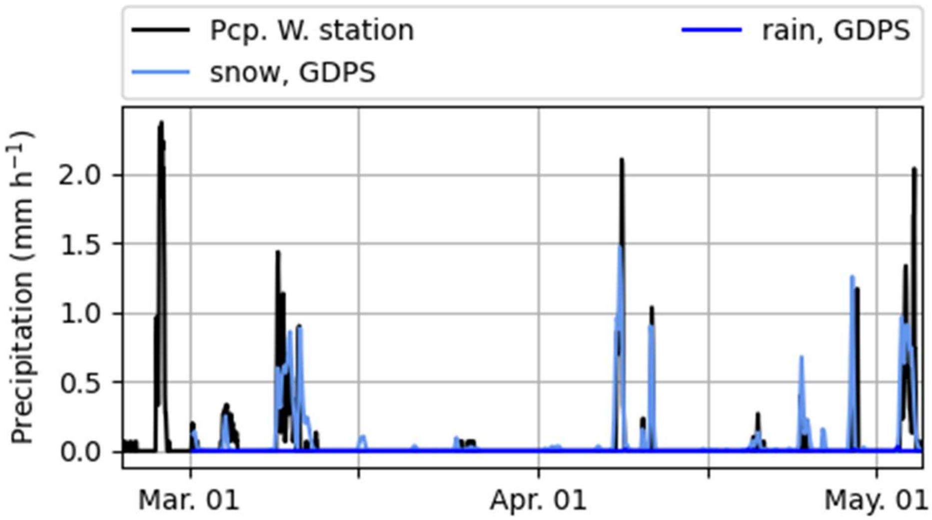

Figure 3 b) Time series of the precipitation rates from the Nain airport weather station (black) and from the GDPS data used to force the simulations (light blue). There is no rain (blue) reported in our observation period.

References

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