Answers to tc-2022-266 RC2

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.

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 results section to better focus on the influence of the mushy thermodynamics on specific processes.

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 L77 and L447 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 sub-grid effects would be best parameterized as part of the 1D column model. As the reviewer pointed, this was mentioned in the submitted manuscript, but is further addressed and discussed at L74-80 and L448-466 in the revised manuscript:

> *"In our case, the deployed IMBs were located in a well sheltered landfast channel dozens of kilometers away from the landfast ice edge and the flooding is very unlikely to be related to adjacent dynamics. This is corroborated by the slow rate of snow-ice formation corresponding 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 L460-465 in the revised manuscript:

> *"One difficulty in the approaches discussed above is that they do not account for the fact that, 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, L115-130. We also included new figures to show the correspondence between the GDPS and IMBs surface air temperature and compared the GDPS precipitation 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 at L335-345.

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 added a L150-155:

"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., assuming that there are no ocean currents (the skin friction velocity is set to the minimum of 0.005 m s^{-1}) and using the default mixed layer depth (20m) and Sea Surface Salinity (34 PSU). The sea surface temperature is initialized at the freezing point calculated as defined by the liquidus relation."

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 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.
- Add time series of the congelation rates, bottom-ice temperature, salinity and enthalpy to better discuss the impact of the mushy layer physics on the ice congelation rates.
- Add a time series of the salinity and liquid fraction in specific layers in mushy layer simulations to better discuss the effect of snow flooding on the ice interior.

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 did not include a quantitative metric to rate the different simulations as the goal of the manuscript was not to assess whether the mushy layer thermodynamics corresponds to an improvement (this has already been assessed and confirmed in previous studies), but to investigate how changing from the Bl99 to the mushy layer physics alter the represented processes. We however agree that are results and discussions could be more quantitative, and computed the Mean Integrated Errors in ice thickness, congelation rates to provide quantitative references in the revised manuscript.

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.

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 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.
- L180: Which (community) needs? : This is mostly for the monitoring of ice conditions near the on-ice snow-mobile routes. This statement was clarified in 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? No, as the snow fall forcing correspond 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 later. Note that switching off the hydrostatic equilibrium amounts to switching off the snow-ice parameterization. This is clarified in the revised manuscript.

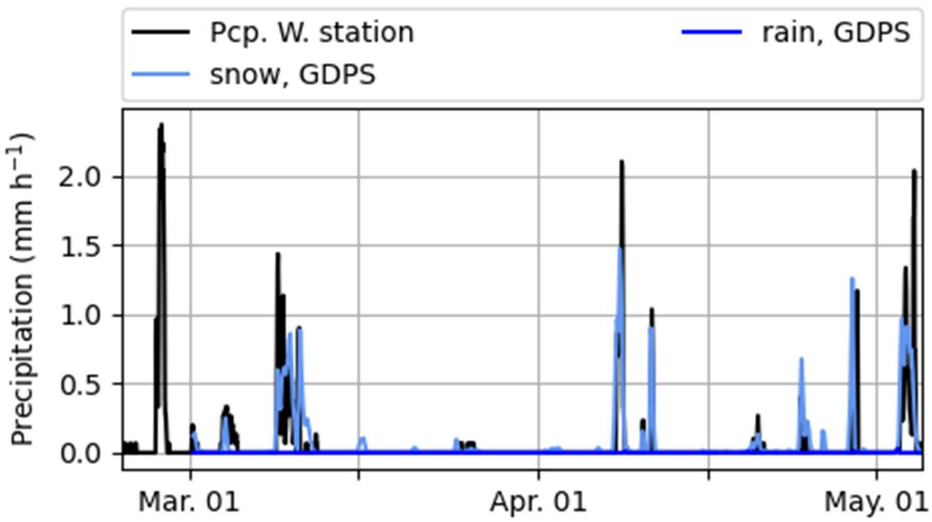

Figure 4: Time series of the precipitation rates from the Nain airport weather station (black) and from the GDPS data used to force the simulations (light blue). There is no rain (blue) reported in our observation period.

References

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