

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2021-273

Andrew Mahoney (Referee)

Referee comment on "A probabilistic seabed-ice keel interaction model" by Frédéric Dupont et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-273-RC1>, 2021

A probabilistic seabed-ice keel interaction model

by Frédéric Dupont, Dany Dumont, Jean-François Lemieux, Elie Dumas-Lefebvre, and Alain Caya

Submitted to The Cryosphere

Review by Andy Mahoney

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Summary

This manuscript presents a probabilistic approach, dubbed ProbSI, for computing the basal shear stress arising due to interaction between sea ice pressure ridges and the seafloor for the purposes of improving the simulation of landfast sea ice in coupled ice-ocean models. The technique described is based upon that of Lemieux et al (2015), but with a more sophisticated approach that utilizes probability density functions of both the ice thickness and bathymetry in a grid cell rather than simply relying on the mean values. One result of this approach is that two grid cells with the same mean ice thickness might have different maximum keel depths, depending on the shape of the ice thickness distribution (ITD). Additionally, by relating the maximum keel depth to a fixed percentile of the ITD, the authors introduce a non-linear relationship between mean ice thickness and keel depth that better matches observations. Although the initial results from the ProbSI model do not appear to be a drastic improvement over the those derived using Lemieux et al's (2015) linear keel depth parameterization, the authors demonstrate the sensitivity of the results to the variability of bathymetry within a grid cell and, hence, the importance of retaining the sub-grid bathymetric distribution.

Overall, the paper is well written and does a good of presenting complex concepts associated with the interaction of different probability distributions in a largely clear and concise way. However, I have one concern regarding the apparent conflation between mean ice thickness and level ice thickness (see comment 1) and I have some recommendations for improving the manuscript by revising some key figures (see comment 2) and including more discussion about early landfast ice formation mechanisms in the Laptev Sea (see comment 3). I provide a longer list of minor comments as well, but I do not think the authors will find it difficult to address any of my concerns. However, because my first major comment below relates to a fundamental part of the methodology, I believe this may qualify as requiring major revisions.

Major Comments

1. Mismatch between mean ice thickness and level ice thickness

Near the beginning of Section 3.1.1, the authors note that both Melling and Riedel (1996) and Amundrud et al (2004) derived relationships between keel depths (h_{dk}) and the draft of surrounding level ice (h_{dl}). However, later in the same paragraph the text describes these relationships as being between keel depth and mean ice thickness (h_{mean}). I thought that this may have been a simple typo, but the x-axes in Figure 2 are labelled as h_{mean} and the text in the discussion on lines 347-348 again refers to mean ice thickness. I am therefore concerned that the authors may be incorrectly applying the findings of Melling & Riedel and Amundrud et al by applying their relationships to mean ice thickness instead of level ice thickness. Since mean thickness will almost always be greater than the level ice thickness in a grid cell, this will have the effect of moving the curves shown in Figure 2 downward, suggesting that x_{997} may not be the best fit as claimed

2. Lack of clarity in key figures

Figures 1 and 3 are important figures, but both could use work to improve their usefulness to the reader. I found it necessary to read both the captions and the main body of text multiple times before I understood what either figure was supposed to be showing. Although I appreciate the avoidance of what Tufte (2001) describes as "chart junk", I believe the information content of each figure as a whole would be greatly improved with better labelling. Specifically, I would recommend adding a legend to Figure 1 to explain the meaning of each curve and symbol without having to read a full-paragraph caption.

This would also allow the caption to be shortened significantly.

I also recommend using textual axis labels so that the reader doesn't have to refer back the main text to remember what each symbol or abbreviation means. For example, the y-axis in Figure 3c is labeled "Bathymetric PDF", rather than $b(y)$, and I recommend adopting this practice for all axes and legends. Also, for accuracy, the y-axes of Fig 1c-f should reference both ice thickness and bathymetry.

Lastly, I recommend using a different color to highlight the final ice thickness category in Figure 1a, since the choice of yellow suggests some relationship to the yellow curves in panels c-f.

3. Incomplete discussion of difference in landfast ice development in Laptev Sea

The authors draw attention to the earlier and more rapid development of simulated landfast ice in the Laptev Sea, as compared with observations of landfast ice in ice charts from the U.S. National Ice Center. They state that an "in-depth analysis is required to investigate what is behind this discrepancy" (lines 356-357), but suggest that it may be related to overestimated of keel depths resulting from deformation of thin ice. Although I do not want to suggest any new in-depth analyses, I would recommend additional discussion referring to the work Selyuzhenok et al (2015; 2017), which describes the formation of landfast ice in the Laptev Sea in some useful detail.

Specifically, Selyuzhenok et al (2015) identify a period of "initial formation" November and December, during which time landfast ice slowly approaches approximately 20% of its annual maximum extent. This is followed by brief period of rapid expansion when most of the remaining expansion takes place. These periods are robust features of the annual cycle of landfast ice in the Laptev Sea and are captured in the NIC-derived landfast ice extent shown in Figure 6. In their 2017 paper, Selyuzhenok et al go on to show that during the initial formation period, grounded features can form offshore while being entirely surrounded by ice that is still mobile. The drift speed of the mobile ice gradually decreases until the ice becomes stationary, at which point there is a rapid growth in landfast ice extent. Selyuzhenok et al (2015) attribute the onset of rapid growth to the achievement of a critical thickness or strength within the formerly mobile ice. Hence, the ProbSI model may not be overestimating keel depths, but instead overestimating the shear strength of the surrounding ice.

Minor comments

Line 27: Replace "Most" with "More"

Line 112: I assume that the symbol σ in equation 5 refers to the internal stress within, but since not all readers will be familiar with the sea ice momentum equation, it should be explicitly defined. Also, it appears that σ is used later in a different context (see comment for line 158), so further clarification maybe needed.

Lines 118-119: I believe the cross reference to section 3 should be a reference to 3-point-something

Line 158: σ is apparently being used here to a different property than in equation 5 above (see comment for line 112). A different symbol should therefore be used either here or above. Also, I recommend providing a physical explanation of both σ and μ as expressed here.

Line 176-177: How are σ_b and μ_b related to σ and μ as defined in equations 7 and 8? Here, σ_b and μ_b are referred to as "mean value" and "spread". Is this how σ and μ should be interpreted?

Line 205: I recommend replacing "This figure" with "Figure 2"

Line 221: Where the text reads "the depth", I assume the authors are referring to water depth. However, since the text regularly refers to both water depth and keel depth, I recommend taking care to specific each time the term depth is used

Lines 221-222: I'm confused here. Please explain why the probability of finding thicker ice is greater with the TU bathymetric distribution

Line 224: The use of "later" here is imprecise as it suggests a time-dependent process. I believe a phrase like "at a greater mean water depth" would be more appropriate.

Line 227: I find the phrase "visually very close" to be ambiguous. I recommend finding a more accurate and specific phrase.

Line 228: I believe "less impact that" should read "less impact than". Also, I assume the authors are referring the impact on basal shear stress, in which case I think it would help to add "on basal shear stress" after "impact".

Figure 3: See major comment 2 above regarding the replacement of abbreviations in the legend with full text. Also, I believe the word "truncated" is missing on the 4th line of the caption before the second usage of "Gaussian"

Line 335: I am not convinced that the number of months of landfast ice per year is a meaningful metric when the timing of formation and breakup is not well reproduced. This approach would suggest the model is somehow more accurate if it simulates earlier dates of both formation and break up. Those are two separate errors that this metric will mask.

Line 336: I do not know what "stronger" means in the context of landfast ice onset. I recommend using plainer, clearer language. In this case, I think "earlier", or "more rapid" (or perhaps both) would be more appropriate.

Line 338: Please clarify what "lower number of landfast ice cover means". I think there maybe a typo here, but I can't uniquely identify a solution

Line 338: I don't think "akin" is the right word here. Perhaps "prone" or "susceptible" would be more appropriate.

Line 339: While appreciate the graphically descriptive nature of the term "high frequency wiggles", I am sure the authors could find a phrase that more accurately describes the variability to which they are drawing attention.

References cited in this review that are not cited in the submitted manuscript

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Tufte, E. (2001), *The visual display of quantitative information*, edited, Cheshire: Graphic Press.-2001.-213 p.