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The Cryosphere
Discussions

Interactive comment on "Polynyas in a dynamic-thermodynamic sea-ice model" *by* E. Ö. Ólason and I. Harms

Anonymous Referee #1

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Review of "Polynyas in a dynamic-thermodynamic sea-ice model" by Olason and Harms

The study compares a shopping list of parameterizations in a highly idealized configuration: granular vs viscous-plastic, elliptic vs. modified Coulomb yield curve, effects of initial and open boundaries, and thickness parameterizations of newly formed ice. The study appears very unfocussed. The choice of parameterizations to be compared appears arbitrary. While the latter parameterization is part of the ice thermodynamics parameterizations, the former ones concern ice dynamics. The underlying goal of the study is not clear to me. Rather, the way the paper is presented, it appears as if the authors tried a few things with their ice model and now the describe what happened along the way (that sounds hard than it is meant, it's refers to the presentation, I think that

this can be fixed without dramatically changing the intended content). The absence of a "red thread" makes the paper difficult to read. The comparisons are evaluated in terms of "fairly realistic", "quite well", etc. that give no clue about the true quality of the individual solutions. It is clear that in a highly idealized configuration, it's difficult to compare with observations, by how should the reader make the decision which parameterization to use. Some guideline should be provided. I recommend a major revision in which the authors try to focus their efforts and make clear why their work is relevant to the general sea-ice modeling community.

Here are the details of my critique:

In the introduction, the authors explain, why polynyas are important in the climate context, but it is not explained that (apparently, or to the authors' opinion, not clear from the text) they are poorly represented in state-of-the-art dynamic-thermodynamic sea-ice models (or components in climate models). It should be outlined in the introduction, what the advantages of "polynya flux models" over sea-ice models are, thereby motivating this particular work. Instead, previous, apparently very similar studies are used as a motivation.

Section 2 appears a little wordy, given, that most of the material is indeed "fairly standard" (although this terminology should be avoided, see below). On the other hand, many details are not yet clear to me, e.g. from eqs. 11 and 12 I do not see how ice can form starting from no ice (division by h=0); the granular model is not completely clear (both in physical and mathematical terms). At what resolution does the continuum approximation common to the Hibler-type models break down? A resolution of 2.5km (used here) is already very (too?) high. At the same time, even at 2.5km, the Hibler-type model averages over large areas (6.25km^2) and it appears impossible to define a polynya edge (which the authors claim to be definable within hundreds of meters, p1047, l19) on these horizontal scales. There is probably a good reason why the sea-ice models use fractional ice cover as a variable, and I have the impression that the authors stretch the sub-grid scale interpretation of this model variable too far. Just as an analogy: The analysis of ocean models cannot be based on the model values of individual grid cells. A wave phenomenon cannot be represented by 2 grid cells, etc.

The description of polynya flux models in Section 3 comes too late for my taste. Further, there are many details that appear not relevant to this paper. Instead, it would be useful to know, what the advantages of polynya flux models are, why they are used, and why they might be superior to the sea-ice model used in this works (which would motivate their use as "truth"). Again, it is not clear, why one would want to compare these PF models to sea-ice models. Both model types were designed for completely different purposes. The definition of polynya in the context of sea-ice models is not clear. Open water in a 80% ice covered grid cell can already be interpreted as a polynya (p1038, l10): even 10% of open water is 0.625km^2 and could be called a polynya. We learn here that "Based on the results of Bjornsson et al. (2001) we can also expect the different rheological formulations of the dynamic-thermodynamic model to give polynyas of similar size and shape as the granular model", then why do the comparison?

The discussion about the open boundary conditions in Section 4 appears a little out of place, as it does not have anything to do with polynyas. I suggest cutting it altogether.

Section 5 starts with saying that an important motivation for the study is comparing rheologies. In section 3 we learned that you do not expect large effects from this (see above). Please find a better motivation. The discussion about the minimum viscosity is not new. A zeta_min>0 should not be necessary for stability reasons. This is briefly discussed in Hunke (2001, JCP). It is annoying that these things are often not well documented, but the weight given to this topic in the manuscript (abstract, text, discussion and conclusions) appears too large.

Discussion: the discussion about the stress states starting at p1045, l17 should be supported by a figures showing the stress states. It is disappointing to be left with a combination of the new-ice thickness parameterization without seeing it in action. Why

not show by experiments, what you claim at the end of Section 7?

Maybe I am too picky, but this is my impression: The language of the manuscript is sometimes overly complicated and could be simplified. Here's a list of a just few examples (I suggest giving the manuscript to someone who know little or nothing about sea ice modeling and has a critical attitude towards language):

p1027, ll20: What you really mean by "The representation of ..." is this: "The last term in eq.(3) is the force due to the divergence of the internal stress tensor. Ice stress, strain rate and rheology are discussed in Sec 2.2."

p1030, l15: is this sentence necessary?

p1034, ll5: Instead of "The model used in this study is a dynamic-thermodynamic sea-ice model in a set-up similar to what Bjornsson et al. (2001) used." you could write: "For our experiments we set up a dynamic-thermodynamic sea-ice model in a similar configuration as Bjornsson et al. (2001)". The next sentence does not connect to this statement at all ("In this section, ...").

Please avoid phrases such as "fairly standard", "relatively", "somewhat different"

Minor comments/technical suggestions

Abstract:

I think it is common practice to avoid references in the abstract.

p1024, l9+l10: "new ice thickness formulation of ..." -> "formulation of new ice thickness (formation?) by/following ..." (otherwise you can read as a "new formulation of ice thickness")

l11: give good results. What is "good"?

Introduction

l19: forcing -> in this context "momentum forcing"?

p1025, l10-12: might be good to characterize polynya flux models in comparison (or contrast) to "full scale dynamic-thermodynamic sea-ice models"

The model

p1026, l3: cut "fairly standard"

p1027, first paragraph: this is actually Hibler's ridging scheme (see Schulkes 1995)

l10: how important is advection of ice momentum (often this terms is omitted)

ll17: if there are no turning angles, why mention them?

l20: it is not the "gradient" but the divergence of the stress tensor

p1028, l5: "relatively" to what? I would avoid all relative statements, if there is no reference

eq.(9) I don't see the difference to Hibler's formulation

p1029, eq(11), ll14: I don't understand how this works for h=0 (division by zero)

l21: I always thought that states outside the yield curve are neither viscous nor plastic, but simply not allowed (numerical discretization, incomplete convergence, etc.).

p1031, ll5: minimum values are not required to make the model stable

l14: P*=30e3 is at the high end of the spectrum.

eq(20): Isn't is P=Pstar (h*A) exp(-C[1-A])?

ll18: the description of the granular material rheology is not clear to me, neither the physical nor the mathematical description. From ll9 I gather \sigma_{I} = - P and \sigma_{II} = \eta \epsilon_{II} and with eq(23) \sigma_{II} = P \sin\phi, from eq(24) I can compute \delta, but I don't see how that helps me to compute P.

p1032: l13: fulfilling -> satisfying

l15: cut "therefore"?

l21: why is the convergence worse than for Hibler's model (which also employs an iterative solver)?

p1033, l13: "fairly realistic" compared to what?

p1034, l3, the upper bound is not 4e8 (that's the lower bound) for the ellipse. What is the consequence of the much smaller maximum \zeta?

Section 3

p1034, l10: what is "relatively simple"?

l18: which (bad reference)

p1035, l10: what is the principal difference between constant speed and finite speed?

l22 "with some parameterization": be more specific. Next sentence: What do we learn from that for this paper?

p1037, l19: the horizontal resolution of 2.5km should be listed somewhere here.

p1038, l17-18: the difference in drift speed is very small (only 3-4%), is that significant?

p1039,l1 fulfills -> satisfies, What's the stability criterion of Dukowicz(1997)?

p1042, l3: inside -> interior

p1047, l21: "feel quite comfortable" replace this statement by something more qualifying.

References:

There is a small problem in the page numbers (probably related to a bibtex style file?), many references (almost all of them, e.g. Bjornsson et al. (2001), Hibler (1979)) have all page numbers listed instead of a range.

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