The Cryosphere Discuss., 3, C582–C589, 2010
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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

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Received and published: 23 February 2010

One of the reviewer's main complaints is that the study "appears very unfocused" and is lacking a "red thread". In order to improve the readability of the manuscript we have made the following changes: We've rewritten the second half of the introduction to make the motivation behind this study clearer. Section 2 was shortened considerably in accordance with the wishes of the other reviewer. It was then combined with a shorter and more concise version of section 3. Section 2 now comprises an overview over polynya formation focusing on dynamic-thermodynamic models (DTM), a subsection on our DTM and a subsection on the control run with which the other experiments are to be compared.

The reviewer also states that "[t]he underlying goal of the study is not clear to me". The underlying goal of the study is to better understand how a polynya forms in DTMs. In particular we have noted that different authors use different methods to identify polynyas (e.g. Smedsrud et al, 06 and Marsland et al, 04). This indicates that differences in parametrisations or model formulations may substantially affect the polynya simulation. We therefore test a few parametrisations (see below for which) in an idealised setup, which is easier to analyse than a realistic model.

In the review the referee mentions that the choice of parametrisations appears arbitrary. First of all the ice in and around a polynya can be split into three regimes; frazil ice in the polynya interior, consolidated ice at its edge and the thick initial ice beyond that. In an idealised setup the polynya interior and the consolidated ice can be considered the "active" areas, while the initial thick ice just drifts away at a constant speed. We must therefore deal with these two sides to the polynya formation; the consolidated ice, the behaviour of which is controlled by rheology and the frazil/pancake ice in the polynya interior, the behaviour of which is controlled by the new-ice thickness parametrisation.

With respect to rheology we chose to look at the response of the elliptic yield curve because it is the most commonly used rheology, but was designed for very coarse resolution - it is therefore interesting to see how it responds at such high resolution. The modified Coulombic yield curve was included because, contrary to the ellipse, it was designed for high resolution modelling. In terms of new-ice thickness it is interesting to see how choosing different values of h_0 affects the solution since the choice of h_0 is never a straight forward one. We choose to look at $h_0 = 10$ cm (in addition to $h_0 = 30$ cm) since that is closer to the value one expects for pancake ice. Having considered different values of h_0 also helps when considering the parametrisation of that variable. We also consider the new-ice thickness parametrisation of Mellor and Kantha ('98) since it is in fairly wide use and, in particular was used by Smedsrud et al ('06) in a recent high resolution polynya study. They defined a polynya as the area where $Ah < 30$ cm, which we found odd and wanted to better understand why this was done.

The reviewer also complains that the comparisons made in the paper are too qualitative and that a guideline for evaluating model results is missing. The main guideline is our understanding of how a polynya is formed (based largely on research surrounding polynya flux models and outlined in section 3) and the granular model which Bjornsson et al ('01) validated against a polynya flux model. In essence we assume that the DTM using the granular rheology has been validated in this setup and then proceed to do sensitivity experiments using different parametrisations. The results can, admittedly, be difficult to assess, but we've made an effort to remove all vague and unnecessarily qualitative statements from the text or replace them with quantitative ones.

The points made so far were, admittedly, not made clear enough or even missing in the original submission. We have made appropriate changes to the manuscript so that these points stand out more clearly.

With respect to section 2 the reviewer asks at what resolution the continuum approximation breaks down. This happens when the number of single floes in each grid cell becomes "small" in some sense. In a paper by Savage ('98) it is shown that a scale of approximately 10 grain widths can generally be modelled without resolving each individual element using a granular model. This means that for our setup individual floes should be no larger than 250 m in diameter. This is clearly true for pancake ice and the consolidated ice, but may not be true for the thick initial ice (depending on geographical location and the time of year). The initial ice does however only play a minor role in our study. The continuum approximation should therefore hold quite well here. This is discussed in section 2.1 of the revised manuscript.

The reviewer also says that "I have the impression that the authors stretch the subgrid scale interpretation of [the fractional sea ice cover] variable". This is true to some extent, in particular in the discussion on p. 1047, paragraph 3, to which the reviewer refers. That paragraph has been removed from the revised manuscript. On the other hand it is clear that in our setup the area of low ice concentration can be interpreted as a polynya and that the area of high concentration as consolidated ice. The transition

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region between the two should then be analogous to a polynya edge. In the control run this region is no more than 5 grid cells wide and, in a realistic simulation one would expect the actual polynya edge to be found in this region. In the revised manuscript the discussion in section 2 should make this clearer than the original submission.

The reviewer complains that the definition of a polynya in the context of DTM's is not clear. In section 2.2 in the revised manuscript we discuss how to define a polynya in the control run. This is then also discussed w.r.t. the new-ice thickness formulation by Mellor and Kantha ('89). In short, it is clear that in this idealised setup we only have one polynya. That is the area of low concentration which is surrounded by either land or ice at high concentration; i.e. the consolidated ice. Separating the polynya and consolidated ice is the polynya edge, a band of high concentration gradient. The $A = 0.8$ isoline is consistently within this band of high gradient, but it is also approximately the ice concentration at which the internal ice stress starts playing a role. Using $A < 0.8$ to define the polynya is therefore useful here, and should be useful also in a realistic model.

The sentence on p. 1036, l 14 starting with "Based on the results of Bjornsson et al. ..." and the reviewer refers to is clearly very misleading and has been removed as a part of the rewrite of sections 2 and 3 in the original submission. The sentence makes it sound as if we can make some assumptions about the behaviour of Hibler's elliptic yield curve model based on Bjornsson et al ('01). This is not the case. What we wanted to come across was that we're using the results of Bjornsson et al ('01) to validate the elliptic yield curve model. So for the ellipse to be valid it must behave similar to the granular model.

Section 4 has been removed as the reviewer suggests. We do, however, chose to use the Neumann condition since it eliminates the high velocities seen at the open boundary when setting $P = 0$ there.

We've included a reference to Hunke ('01) and removed references to the ζ_{min} discussion from the abstract and conclusions. See our response to the other reviewer for more details on the part about ζ_{min} and about including the material derivative.

We've added a figure showing the stress states in relation to ice concentration (included here).

The new-ice thickness parametrisation introduced at the end of the discussion-section cannot be tested further in this setup. This is because the ice is always thin enough so that this parametrisation becomes the parametrisation of Winsor and Bjork ('00) (equation 29), which has already been tested. The parametrisation in equation (31) should be tested in a realistic model. This point is now discussed at the end of the discussion-section.

We've given the manuscript to someone with a critical attitude towards language and made revisions based on her suggestions. We've also acted on the suggestions made by the reviewer w.r.t. language.

Minor comments/technical suggestions:

These were all acted upon as suggested by the reviewer or are not relevant because of the considerable rewrite we did of the first sections. We would like to respond to some of the suggestions here as well:

The abstract has been completely rewritten with the reviewer's comments in mind.

Introduction:

p1025, l10-12: The weight of polynya flux models in the manuscript has been greatly reduced, as suggested by the other reviewer.

Section 2:

p1027, l10: We discuss the advection of ice momentum in our response to the other reviewer.

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eq. (9): There is no difference to Hibler's formulation. What we meant was that Hibler uses fixed ice growth rates while we calculate them based on temperatures etc. The sentence before equation (9) has been rewritten to prevent this misunderstanding.

eq (11): The case $h = 0$ is mentioned at the end of the paragraph following the equation as: "In particular, this means that when ice forms in a grid cell that had no ice before this cell will become fully covered with thin ice". We've added an explicit reference to $h = 0$ to this sentence.

p1029, l21: For a completely plastic solution all stress states are indeed on the yield curve. However, if we have points where the calculated viscosity is outside the maximum (or minimum) bounds on ζ the viscosity becomes constant and the flow linear viscous. These stress states will be located inside (or outside) the yield curve. Since we shortened section 2 so much this discussion doesn't really fit anywhere in the paper and is omitted.

p1031, l15: See our response to the other reviewer.

p1031, l14: Bjornsson et al ('01) showed that the choice of P^* made little difference for the granular model and we find that this is also the case for the other rheologies. We therefore use the same P^* value as they did.

p1031, l18: We've shortened the description of all yield curves very much, in accordance with the comments from the other reviewer. With respect to the pressure term the revised manuscript now states: "The pressure term is found by perturbing the last known solution to the momentum equation (4) so that the resulting velocity field has a divergence determined by a given angle of dilation". For more details we refer the reader to the original publication.

p1032, l21: The granular model uses an additional iterative solver to calculate P (which is calculated algebraically for the other two yield curves). This should be clearer in the revised manuscript.

p1034, l3: Using a smaller ζ_{max} means that creep flow sets in at higher strain rates. This has no effects here.

Section 3:

p1035, l10: In Pease's model the frazil ice is transferred from the polynya interior to the edge immediately - the speed is therefore infinite. Finite speed is then anything slower than infinite speed, but that doesn't mean it's constant. Of course in Ou's model (and, I think, all flux polynya models) the frazil ice speed is both constant and finite.

p1038, l17-18: When using $P = 0$ at the open boundary the velocity increase there makes comparison between the free drift speed and consolidated ice speed a bit difficult. We now use the Neumann condition and using it the difference is about 20

References: We can't reproduce the problem with the page numbers ourselves, but we'll discuss it with TCD staff.

Interactive comment on The Cryosphere Discuss., 3, 1023, 2009.

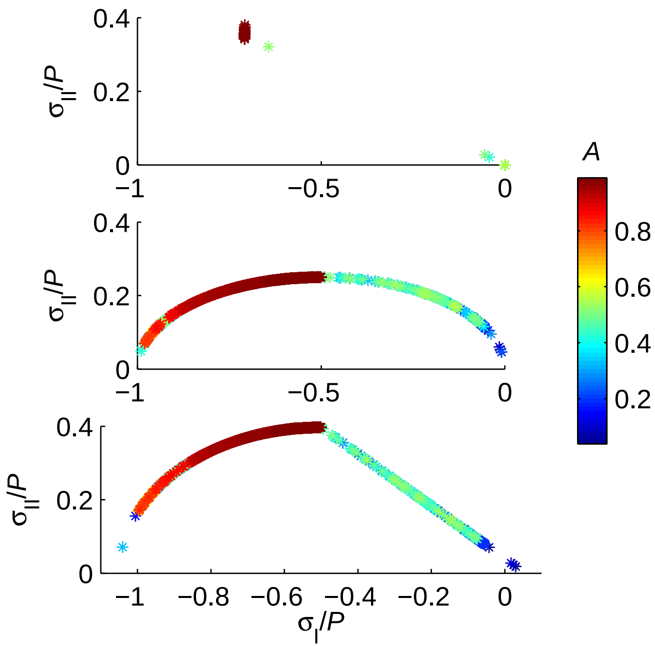

Fig. 1. Stress states using the granular model (top), the elliptic yield curve (centre) and modified Coulombic yield curve (bottom) after eight model days. The colour scale indicates the ice concentration.

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