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The Cryosphere Discussions

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Interactive Comment

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

Anonymous Referee #2

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The authors present polynya simulations with a dynamic-thermodynamic sea ice model in an experimental set up that is similar to that of Bjornsson et al. (2001). They investigate the sensitivity of the modelled polynya to the following processes and parameters: (1) formulation of consolidated ice internal forces; (2) demarcation thickness of ice newly formed in open water; (3) formulation of lateral boundary conditions.

The results reported in the paper are of some interest and probably deserve to be published after quite considerable revision. In the following, I provide a list of comments, questions and recommendations organised by paper page number and line. Those that I consider fundamental are preceded by a double asterisk.

-1 Page 1026, line 15. "m is the total sea ice mass per cell". Replace by "m is the sea ice mass per unit area".

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-2 ** Page 1026, line 21. S_A is described as a “thermodynamic source/sink term”. There are in fact both thermodynamic and dynamic aspects to S_A. The only thermodynamics involved in S_A is the one that goes in calculating S_m and \Delta h_{ow} in (7) and (8). To obtain from these an expression for S_A, an ice pile up parameterisation is still required. This parameterisation must be based on dynamical assumptions about how the newly formed ice in leads accretes alongside the existing ice. In the parameterisation of S_A used in the paper, one just prescribes a pile up thickness h_0. It is subgrid dynamics, not thermodynamics, that determine this pile up thickness.

-3 Page 1027, line 20. Replace “gradient” by “divergence”.

-4 ** Section 2.2. The viscous-plastic and granular rheologies are well known and detailed descriptions of each of them can be found in the pertinent literature. This section could be greatly shortened with no detriment to the paper.

-5 Page 1031, line 12. Replace “h” by “A h” in (20).

-6 Page 1031, line 14. The choice C=20 is a common one, but P^\star has been ascribed many values in the literature. The authors should justify the one used by them in the paper. Also, I do not think the units of P^\star are correct, the should be N/m^2.

-7 Page 1032, line 10. No expression with \delta_{ij} has been previously introduced.

-8 Page 1032, line 17. Replace “h” by “A h” in (25). The mean ice thickness “h” in Tremblay and Mysak (1997) is what the authors call “A h” in their paper.

-9 Page 1034, line 2. This upper bound for the viscosity is too small. Why should a smaller upper bound be desirable? I would expect the opposite to be the case.

-10 ** Section 3. I do not see the point of spending so much time describing polynya flux models. These models are extreme simplifications of the actual physics of a polynya, and they are hardly superior or better equipped to describe polynya dynamics than dynamic-thermodynamic sea ice models. No comparison of polynya flux models and

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dynamic-thermodynamic sea ice models is done in this paper, and so I would remove most of the discussion in pages 1034 and 1035.

-11 Page 1037, line 17. “von Neumann boundary conditions”. Boundary conditions consisting in prescribing a flux or a derivative across a boundary are called “Neumann boundary conditions”, after Carl Neumann. John von Neumann is a different mathematician...

-12 ** Page 1037, line 20. I find phrases such as “reasonably good results”, and many other equally vague phrases in the paper, very irritating. “Good”, “acceptable”, etc. are not technical terms and, in general, have not place in a scientific manuscript.

-13 ** Page 1039, lines 16-17. “This should, in essence be equivalent to using an infinitely long homogeneous channel.” Not at all! If you make the effort of working out the solution of the problem at hand for the case of an infinitely long half channel you will find that sooner or latter the ice will become arrested and the polynya will close. This is because as the ice grows down stream, lateral friction between the ice cover and the channel walls increases and eventually internal shear stresses will balance the wind stress and prevent the ice from moving.

-14. Page 1040, last paragraph. More discussion on the minimum viscosity value of Hibler (1979). If the authors are solving the full ice momentum equation, including non-linear advective terms, they need indeed to add a small enough amount of viscosity in order to avoid grid-scale noise in the velocity field. Typically, $U \Delta x$ must be smaller than $2 \zeta \rho_i^{-1}$, where U is the ice velocity (e.g., Griffies and Hallberg, 2000). The solution will not necessarily become unstable and blow if the inequality is not satisfied, but it will be noisy. If, in spite of what is suggested by (3), the authors are not including the inertial terms in their momentum equation, then satisfying the grid Reynolds number condition might not be necessary. Hibler (1979) did indeed include advection of momentum.

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