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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

E. Ö. Ólason and I. Harms

einar.olason@zmaw.de

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We respond to each comment by number where a response is needed. If we do not respond to a comment here it is because we have simply changed the text as the reviewer suggests and don't feel there is any need discuss the change further.

2: That is absolutely correct. We now simply refer to S_A as a source/sink term, not a thermodynamic one. The discussion about S_A is also no longer refereed to as "Thermodynamics".

4: We've replaced the rheology discussion with three paragraphs, each discussing one rheology and a reference to the relevant literature.

6: 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 (this is now mentioned in the text). We've also fixed the units.

9: This is the upper bound suggested by Hibler and Schulson ('00), but it does seem too low. Using a smaller ζ_{min} means that creep flow sets in at higher strain rates, but it also means the model converges faster. Using such a small upper bound has no effect in our setup.

10: We've greatly reduced the time spent on polynya flux models since, as the reviewer points out, there is no comparison of polynya flux models with the dynamic-thermodynamic model (DTM). We do, however, find that it helps to keep in mind some of the assumptions of the flux polynya models. They are indeed a simplification of the polynya formation process, but simplified physical models are meant to underline the important factors while ignoring the less important ones. It seems that flux polynya models do this well; they have been used quite successfully to simulate a variety of situations. DTMs are on the other hand designed to model pack ice. We can't assume that although they do that quite well that they can be used to model polynyas where there's frazil ice and very thin consolidated ice. In fact, one of the most important points of our paper is how we can use the work already done on flux polynya models to better simulate new-ice formation in the DTM.

12: We've made an effort to remove all vague and unnecessarily qualitative statements from the text or replace them with quantitative ones.

13: Section 4, discussing the open boundary and initial conditions has been removed as suggested by the other reviewer. But it is right that the statement about the infinite channel does not take ice growth down stream into account and is patently false. Using the Neumann condition is, however, in practice, equivalent to using a much longer channel. If we hadn't removed section 4 then the statement about the infinite channel would probably have been replaced with a comparison between using the Neumann condition and a channel twice as long as the one we use. Such a comparison shows only a shift in the polynya edge by a few grid cells.

14: We were indeed solving the full ice momentum equation, but in this idealised setup no grid-scale noise was observed in the velocity field. We have, however, been working on a realistic simulation and do observe noise in it. We haven't confirmed that this is due to the non-linear terms, but it is likely. In response to this comment we've added a comparison between a run with and without the non-linear terms and a discussion of the results. We find that although there is some difference between the two runs it is very small, even at such high resolution. Any benefits we may gain from solving the full equation of motion are also lost because the required minimum on the bulk viscosity is too high.

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