The Cryosphere Discuss., 9, C2787–C2791, 2016
www.the-cryosphere-discuss.net/9/C2787/2016/

The Cryosphere Discussions

Open Access

TCD

9, C2787–C2791, 2016

Interactive Comment

Interactive comment on "neXtSIM: a new Lagrangian sea ice model" *by* P. Rampal et al.

Anonymous Referee #1

Received and published: 15 January 2016

It is an excellent paper, with stunning results on a new generation sea ice model.

I have three main comments

1) there are lots of parasite words in the text. Either because of Frenglish use. Or because the authors abuse of "commercial" pieces of text such as "for the first time", etc. Remind that this is not a proposal and that the paper is meant to be read for long long times. I would argue that the paper would gain by being more sober in general.

2) The intro could be better balanced as well (see detailed comments)

3) I also have one very stupid question. Healing seems ok if the time scale is long enough, beyond which the model results do not seem to be affected anymore by the healing time scale. "Is healing useful in the end", is an obvious question that comes at the end of the reading. I know it is useful, but a good improvement of the paper would

be to show or explain what happens if healing is too slow, or if there is no healing at all.

Apart from these few remarks, the scientific changes that I propose are cosmetic.

1 Specific comments

1.1 Intro

- Arctic's. Check if ok: the sea ice cover does not belong to the Arctic.
- treacherous: first time I read this word.
- page 5887, lines 5-9. Your wording is loose here. I would simplify the sentence. Because 1) Speaking of "the role of Arctic and Antarctic climate in the climate system" sounds bizzare, I would rather speak of the role of sea ice in the climate system; 2) Polar amplification of climate change is not only due to sea ice, and the primary drivers are atmospheric. Check e.g. Pithan and Mauritsen, NG2014. Maybe speak of the ice-albedo effect, or of the impact of sea ice growth and melt on ocean circulation ?
- page 5887. L 9-25. "Earth" may need to be capitalized. Two times "in particular". I did not particularly fancy this paragraph. I think the justification that VP-like models are not in agreement with RGPS observations is sufficient and better than what is written now. Using climate models to justify the EB approach is not the best manner to motivate its development, in my view, as we rather suspect the sea ice mean state, the atmospheric and oceanic forcings in climate simulations to be responsible for sea ice model misbehaviour.
- The review on sea ice models is too long and the choice of references is debatable. I would shorten in one paragraph. I would cite Coon, first. Then explain

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

that existing large-scale sea ice models (CICE, LIM, MITgcm, MPI) virtually all base on Coon's approach. Describe VP, EVP in Eulerian framework. Then cite the papers that show that these models were found inadequate wrt RGPS observations. It is enough to justify your study. You don't need to write the full history of sea ice models.

- page 5887-5888. last paragraph of 5887 continuing on 5888. I would remove or rewrite this paragraph. Your choice of references seems either old-fashioned or bizzare. I like a lot Ukita and Martison and Huwald's papers, but these models are not used in coupled modelling studies. Recent research focused on adding ice salinity to thermodynamics (Vancoppenolle et al, Turner et al, Griewank and Notz, Rees Jones and Worster, . . .).
- following paragraphs. You speak a lot of large-scale models, but you don't cite any reference where these can be found.
- page 5889. Your statement that ice thickness distribution is needed for low resolution only is not backed by any reference. I think your statement is neither proven nor a priori true, since such formulations are meant to capture 1m-scale variations in ice thickness. ITD schemes enhance ice growth (growth-thickness feedback) and melt (ice-albedo feedback). How much they bring is detailed in e.g. Holland et al (JClim 2006).
- page 5889 Lines 12 and following. Ice dynamics is plural. Is formation of new ice really slow (count the time is required to form 10cm of new ice at -10°C)
- page 5890. Remove all "for the first time", "crucial", . . . Not that I contest, but your paper is convincing enough without them. They could have irritated me in a bad day.

1.2 Model description

- As I understand, damage is an extensive variable. What do you do when new ice is added at the base and in open water ?.
- page 5892. first line, is "P" or "grad P" the pressure term ?
- equation 5. I think the reader would benefit if you told that this is simply the representation of an elastic deformation.
- equation 11. Could you plot that relation schematically, if not done anywhere else ?
- page 5894. line 17. "led" -> "lead"
- page 5894. I was curious to know why you still need a pressure term to prevent unrealstic local thickness accumulation, whereas your model is elastic. Hence, the sea ice, if thick enough, should in principle be able to oppose to compressive deformation. Could you remove the pressure term if the elasticity depended in h^2 ? Did you make tests on this ?
- page 5896, first line. are you sure "neg. feedback" is a proper wording to describe the role of healing on deformation?
- page 5897. Do you change damage if concentration decreases ?
- Equation 24, 25, and 26 are not consistent in terms of units. Sh in is m/s, SA is in s-1, Shs is in m/s. . .

1.3 Other sections

- page 5902. Why do you use different bathymetries for TOPAZ4 and the basal stress term ?

TCD

9, C2787–C2791, 2016

- page 5910. What is D ? Why capitalizing Summer and Winter ? "one 3 days period to the other" is bizzare wording.
- Section 3.4 The section on ice albedo is without interest, I would remove. The albedo param you use is super simple. And the conclusions are obvious and known for a long while.
- Page 5912. If you cite the study, there is no scientific reason for the pers. comm.
- page 5913, line 9. dependant should be spelt dependent
- "nextSIM performs very well wrt the most important metrics we can impose on sea ice model performance". I agree, as long as you restrict this to scales smaller than a year. For longer time scales, you may find out that you need better thermodynamics or thickness distribution. Read Semtner 1984 for example to find out that the 0-layer model induces a shift of 1 month of the seasonal cycle.
- page 5915, line 22. proof -> prove

Interactive comment on The Cryosphere Discuss., 9, 5885, 2015.