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The Cryosphere
Discussions

Interactive comment on "Presentation and evaluation of the Arctic sea ice forecasting system neXtSIM-F" *by* Timothy Williams et al.

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Review of **Presentation and evaluation of the Arctic sea ice forecasting system neXtSIM-F** by Williams et al.

In this paper the authors present and evaluate a new sea ice forecasting system based on the neXtSIM sea ice model. They have evaluated drift, sea ice thickness and concentration against different datasets and show that the forecasts, in general, show good agreement with the observations. Being also in the business of ice-ocean predictions, I recognize the amount of work that was done by the authors to develop and evaluate this kind of forecasting system.

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I have, however, two major concerns about the paper.

The first one is easy to address. I think the authors need to do a better literature review. We have the impression there are currently only two sea ice forecasting systems (Topaz and GOFS) in the world and that neXtSIM-F is the third one to be proposed. For example, the Danish meteorological institute, the UK Met office, ECMWF, Environment and Climate Change Canada (ECCC) all have operational sea ice forecasting systems. Note also that we (ECCC) have, a few years ago, developed a similar system as neXtSIM-F: a stand alone sea ice model coupled to a slab ocean model (with MLD and SSS initialized from a coupled ice-ocean prediction system and SST from an analysis...). It is not used anymore as it has been replaced by a coupled ice-ocean forecasting system but I think, given the similarities between the two systems, that it should be mentioned. You could then describe what is different (e.g. the ocean heat flux correction you propose...which is interesting by the way.)

The second major concern I have is that neXtSIM-F was evaluated for only half a year. And especially, the evaluation was done during the winter months, that is at a time when there is almost no navigation in the Arctic. I would understand why you would choose this period if your system was designed specifically for the Baltic Sea or the Gulf of St-Lawrence (where there is a lot of navigation during winter). I have the impression that the authors have submitted their paper too early and that it would make more sense to show a complete seasonal cycle of the forecast scores. You mention, anyway, that neXtSIM-F will be operational in November of this year...this is coming soon. This means you will soon have the evaluation for the summer months? Then I really think you should include these.

This paper will be an important contribution to the field of sea ice forecasting but first the authors need to address these concerns and the minor comments given below.

1 Minor comments

1) p.5 line 19. I guess you use a turning angle for the ocean-ice stress? Please mention it.

2) p.5 line 22. this is already mentioned above.

3) sections 3.6-3.7. What is this the atmospheric forcing you will use once the system becomes operational? I hope it will be the same one used for the evaluation because then it does not make sense.

4) p.8 lines 7-9. You give information that is not needed. We don't need to know that it was first coded in Matlab. Just say that the version of neXtSIM used for neXtSIM-F is described in Rampal et al. 2016 and Samake et al 2017.

5) p.8 lines 13-14. Mention that the MLD varies spatially and that this spatial field is fixed (I guess) during the 7 days of the forecast.

6) section 4.2. Mention how you initialize the sea ice velocity.

7) section 4.2. What justifies the values of 0.2 and 0.8 for the initial thin and thick ice?

8) p.10 line 19. The first condition (i.e. $c_t > 0$) is not needed, right?

9) section 5.1. Clearly state how long (time period) is the free run.

10) p. 11, line 6. Typo "the no ice".

11) Fig. 3. Explain how you calculate the concentration and uncertainty. Is it the mean over the whole domain? Then it means you have many grid cells with a concentration of zero and many with a concentration close to 1.0. This means the signal you are interested in is kind of buried because most grid cells have a forecast concentration close to the observed one...which is not surprising. How do you deal with uncertainties when the concentration is 1 or 0...it cannot be gaussian, right?

12) p. 13, lines 6-7 and Fig. 6. Is it possible the MEB rheology leads to too much convergence? (e.g. North of the CAA).

13) p. 13, lines 18-24. Too many numbers given. I don't think you need to give all these values.

14) p. 13, line 27. What do you mean by "eroded"? Please clarify.

15) p. 13, line 33. I don't think the ice is landfast there but it is (very) slowly drifting.

16) p. 14, line 1. remove "very respectable"...just give the number and that's it.

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17) p. 14, line 10-14. Please clarify this paragraph. It is not clear here what are exactly the experiments (especially the sentence"...without assimilation, with assimilation of concentration and with assimilation of concentration and thickness.").

18) Fig. 9. I am not surprised persistence is doing so bad here because the beginning of November is a time when there is a lot of ice growth. No wonder the model performs so well. I think it would be good to show another case for example in March. Is it always true that the model beats persistence? Is is always true that assimilation improves the quality of the forecast?

19) p. 16, lines 13-14: The captions in Fig. 10 and 11 do not match the text here about the dates.

20) p. 18, line 1: East Siberian and Chukchi seas...not obvious to me when I look at Figs 10 and 11.

21) Fig. 13 and 14 are not discussed. Are they really needed?

22) p. 19, lines 11-13: Please rephrase...

23) p. 20, line 1. What drifters are we talking about here?

24) p. 25, lines 11-17. I think it could also be the thermodynamic model itself. There is clearly too much ice growth...the model might require some tuning. This should be mentioned. What about the way the thin and thick ice categories are initialized? How

does it affect the overestimation of the growth?

25) p. 25, line 19. What do you mean by "fragmentation"?

26) p. 25, line 23. I thought neXtSIM is using our grounding scheme for landfast ice? Would it be worth tuning the k1 parameter?

27) p. 26, lines 20-22. Is it a result you presented in this paper? Or I just don't understand the sentence. Please clarify this.

Congratulations for your work on developing this new sea ice forecasting system.

Jean-François Lemieux

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