

Review of ‘Probabilistic forecast using a Lagrangian sea ice model: application for search and rescue operations’ by Rabatel et al.

In this paper, the authors implement a probabilistic forecast capability for the neXtSIM sea ice model and compare these forecasts to the ones of a free-drift (FD) model. The ensemble of sea ice forecasts is produced by forcing the sea ice model with perturbed wind fields. They also investigate the use of these probabilistic forecasts for search and rescue applications.

It was a pleasure to read this paper. I found many ideas inspiring. I am not very familiar with search and rescue applications and found this section very interesting.

I am not really surprised to see that neXtSIM does better than the FD model (especially in winter). In fact, I would like to suggest a different way to present these results. Instead of saying you compare two models, you could say you study the impact of rheology on probabilistic forecasts. The title of your paper could be: Probabilistic forecast using a Lagrangian sea ice model: impact of rheology. This is just a suggestion.

The paper is in general well written. There are a lot of interesting results. There are, however, a few things that need to be clarified. I also suggest a few additional things that could be worth investigating. Please see my comments below:

1. Major comments

1) You need to give more details on how you initialize the forecasts. Do you use fields (h , h_s , A , d , u) from the previous forecast? And how do you deal with the FD model? I guess you use the thickness field from neXtSIM as the thickness field from a model without rheology would be completely unrealistic. Please relate that to the caption in Fig. 6.

2) I understand why you neglect the rheology term for your FD model. However,

what is the justification for neglecting the inertial term?

3) Fig 2. and p. 9 line 3: How do you define FD 'events'? Concentration threshold?

You have optimized C_a for neXtSIM. Your conclusion (p. 23) says you have done the same thing for the FD model. This should be mentioned and clarified earlier. I also suggest you give the C_a value you obtained for the FD model.

4) You often discuss spatial correlations between certain fields (e.g. Fig 7 and 8). You relate these high correlations to the rheology and the thickness field. I think it would be a good idea to show maps (winter and summer) of the effective elastic stiffness as it is more representative of the 'strength' of the ice cover than just the thickness field. I am also wondering what is the effect of the pressure term? My impression is that the effective elastic stiffness gets very small in summer because the ice is so damaged and cannot heal so that the pressure term plays an important role.

5) In the comparison of the predictive skill of the model with and without rheology, you look at the error of the barycenter. I think you could also discuss whether the error $e(t)$ of the barycenter is smaller than the one of a single deterministic forecast (no perturbation to the wind). Even if it is not the case, the probabilistic forecasts with its spread would still give important information...

I would also be curious about the following experiment...what happens if you move your virtual buoys with the persisted initial velocity of the observed buoys (see Hebert et al. 2015). At what lead time is the ensemble of neXtSIM better than the persisted observed initial velocity? I guess this could give you some indications about the quality of your forcing field.

2. Minor comments

1) Overall the english and the text is very good. There are a few typos. Here is a list of some of them: p.1 line 14, p.4 line 4, p. 11 line 29, p. 20 line 5, p. 22 line 29, p. 25 line 28.

2) p.2 line 5: Add 'sea ice' before 'forecasting systems'.

3) p.2 line 5: Note that RIPS is no longer in operations and has been replaced by the coupled Regional Ice Ocean Prediction System (RIOPS). It would be better to rephrase. The references for this new system are Lemieux et al., 2016 (the paper you already cite) and Dupont et al. 2015:

A high-resolution ocean and sea-ice modelling system for the Arctic and North Atlantic oceans.

4) p.3 line 3: remove 'advanced'...Just say what it is.

5) p.3 line 9: Coon et al., 1974 modeled sea ice as an elasto-plastic material...please rephrase.

6) p.3 line 18: 'sea ice responds in a linear way' is vague. Please clarify what you mean by that.

7) p.3 line 20: You could add '(due to the limited number of observations)' at the end of this sentence.

8) p.3 line 27: Change 'full complexity of the present version' by 'the latest model developments' .

9) p.4 line 9: Change 'spatial' by 'spatially'.

10) p.4 line 20: Change 'refreezing' by 'freezing'.

- 11) p.4 line 23: What do you mean by 'effective'? Grid cell mean values?.
- 12) p.4 eq. 2: I am not familiar with this formulation of the vector product for the Coriolis term...Don't you want to use the common formulation with the 'x'?
- 13) p.6 line 8: Add 'virtual' before 'buoy'.
- 14) p.6 line 20: I think you need to divide by N in the equation for B(t).
- 15) The second figure you refer to is Fig.4 (p. 8 line 4). Please change the order.
- 16) p. 8 line 5: Are these 10 m winds? Please specify this and mention the turning angle you use (maybe also for the ocean currents).
- 17) p. 9 line 3: Remove 'state-of-art'...Just say what it is.
- 18) p. 9 line 17: 'Dominant' is a bit confusing here because it sounds like it is the largest term in the momentum equation (the wind stress is usually the largest one). Please rephrase.
- 19) p. 10 line 3-5: Why are thermodynamics an issue? You have a thermo model, right?
- 20) p. 10 last line: Add 'steady state' before 'drift'.
- 21) Fig.5: It is difficult to see the coherency between the neXtSIM panels because the lower panel is almost only blue. Can you improve the colorscale so that we can see better the difference? (same idea for Fig. 7)
- 22) p. 11 line 25-30: You mention correlations between spatial fields. Is it just by looking at the figures or you actually calculated spatial correlations?
- 23) p. 14 line 1: Clarify what you mean by 'the response'...ice velocity?

24) p. 15 line 3 and elsewhere: Is 'on another hand' a correct expression? Is it better to use 'on the other hand'?

25) Fig. 13: How do you define the mean sea ice coverage (A=15% contour)? Looking at these two panels, as all the buoys are in regions of thick compact ice, it is kind of obvious that neXtSIM will do better than FD in this experiment. In other words, the FD model would do better if the buoys were uniformly distributed. I would add a sentence to mention that.

26) As you calculated the POC for Fig. 17, I suggest you give the exact definition of the POC in eq. 13 instead of saying that it is proportional to...

27) Fig. 17: same idea as before, what happens if you use the deterministic forecast instead of the barycenter? Do you get a real benefit from the ensemble forecast for the time evolution of the POC?

28) Please rephrase the last sentence of p. 22.

29) p. 23 line 3: replace 'sensitivity' by 'sensitive'.

30) p. 25 lines 7-9: I understand what you mean but I find that the two sentences ('Still it is the wind...' and 'we suggest instead...') kind of contradict each other. If the wind is the key player, efforts should be made to improve the forcing winds (by improving the assimilation and forecasts of the atmospheric model). Just rephrase a bit. By the way I like the discussion about assimilating sea ice fractures...Interesting.

Congratulations for your paper.

Jean-François Lemieux