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

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First of all, we would like to thank the referee for his in-depth review of the manuscript and his numerous and relevant comments and suggestions. Please find below the answers in blue text to each of the points raised.

NOTE: *In the revised manuscript, we added few words about how we proceeded to optimise the air drag coefficient for the free-drift model, and indicated which value we found. We also updated all the figures showing the results of the new FD simulation and changed the text when describing the results accordingly. Note that it does not change the conclusions of the paper, but modify quantitatively the results we obtain, especially making FD and neXtSIM more similar in the summer.*

title suggestion: Probabilistic forecast using a Lagrangian sea ice model: impact of rheology
We changed the title following your suggestion to:
“Impact of rheology on probabilistic forecast of sea ice trajectories: application for search and rescue operations in the Arctic”

1. Major comments

1) You need to give more details on how you initialize the forecasts. Do you use fields (h, hs, 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.

Thanks to your comment. Yes indeed, we completely missed to provide explanations on the initial conditions. We use fields from previous neXtSIM simulations on the same period with the same external forcings but without wind perturbations. The text has been updated accordingly (p.11 l.20 and p.12 l.1-2).

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

what is the justification for neglecting the inertial term?

When the rheology is not taken into account, the time scale of the ice dynamics is short (few hours) and the steady state solution (acceleration set to 0) is rapidly reached (see McPhee1980 and Lepparanta2005). Note also that for this study we wanted to use the simplest model as possible, for which an analytical solution can be easily calculated. This is also consistent with Grumbine 1998.

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

No concentration threshold here. A FD event is defined as the one when the simulated ice velocity is within a range of 10% around the value of the free-drift solution. By doing so, we avoid using any concentration threshold that can be somehow misleading or at least a poor constraint for defining region of free-drift. The text has been updated accordingly (p.10 l.12-15).

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.

Thanks to your comment, we realised that the way we optimised the drag coefficient for the free-drift model was not optimal because it used the same geographical restriction as for the neXtSIM model. We therefore re-optimised that parameter for FD, without applying any geographical restriction. Then we re-ran the free-drift simulations for both winter and summer with the newly optimised drag coefficient.

In the revised manuscript, we added a short explanation on how we optimised the drag for the free-drift model, and reported on the optimal value we found. We also updated all the figures relative the FD simulation and changed the text describing the results accordingly. Note that, the use of the new (optimised) drag coefficient does not lead to changes in the conclusions of the paper, yet it modifies quantitatively the results we obtain, especially for the summer.

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.

We choose to show the map of ice thickness because, after the concentration, this is the quantity that correlates the most with the drift response to external forcing, and has the advantage of being a meaningful physical variable to everyone. In winter for instance, the concentration is close to 1 everywhere and it is therefore unable to display any spatial correlation (if any). We agree with you that the elastic stiffness is more representative of the "strength" of the ice cover, in a mechanical sense. Nevertheless, except locally (along the LKFs) where the ice is highly damaged, the geographical pattern of elastic stiffness is at first order correlated to the thickness pattern, and have thus opted to show the latter in view of its clearer physical meaning.

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.

The pressure term is large only when the local deformation is convergent and if the concentration is close to 100% (See equations 17 and 18 in Rampal et al. 2016b). In summer, it is correct to say that there is no damage healing, and therefore damaged sea ice can only loose mechanical strength over time.

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...

Indeed, this is a good suggestion. The comparison with the deterministic forecast does provide important information. We have re-run our model to get the deterministic "forecasts", using the ASR reanalysis as forcing and the air drag coefficient equal to 0.0065 (after optimisation with OSI-SAF *dataset). We find the forecast error from a single deterministic forecast is close to the probabilistic barycenter, especially in winter, but is larger by 15% in the summer, so there is a small benefit of using the ensemble mean of a probabilistic forecast. Looking at the barycentric coordinates (e_para and e_perp), we note larger differences in the parallel components, whereas the perpendicular components are similar (see Fig. 15 in the revised manuscript).*

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.

While we agree with the Reviewer that the comparison with a persistent forecast could be an interesting experiment to perform, we consider it slightly out of the scope of this work that is centred on the impact of rheology. We hope to be able to investigate this further in the future.

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 line14, p.4 line4, p.11 line29, p.20 line5, p.22 line29, p.25 line28.

Done

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

Done

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.

Thank you for your note. We updated the name of the prediction system to RIOPS, and we added Dupont et al 2015 as a reference.

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

Done

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

Done

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

Done

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

Done

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

Done

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

Done

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

Done

11) p.4 line 23: What do you mean by ’effective’? Grid cell mean values?.

Yes, you are correct. The unit of the effective thickness is a volume per unit of area.

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’?

Yes indeed, we changed the notation.

13) p.6 line 8: Add ’virtual’ before ’buoy’.

Done

14) p.6 line 20: I think you need to divide by N in the equation for B(t).

You are correct. Thank you for having spotted this typo which is now corrected.

15) The second figure you refer to is Fig.4 (p. 8 line 4). Please change the order.

Done

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).

We use turning angles of 0 and 25 degrees for the air and water drags, respectively. These values are now listed in a table that we added in the revised version of the manuscript and in which all the parameters are reported with their respective values. Note also that these values are the same as those used in Rampal et al. 2016b.

17) p. 9 line 3: Remove ’state-of-art’...Just say what it is.

Done

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.

Yes, you are right. The word "dominant" is not the right one. We now have rephrased the sentence (p.11 l.11).

19) p. 10 line 3-5: Why are thermodynamics an issue? You have a thermo model, right?

There must be a misunderstanding here. The thermodynamics is indeed not an issue. We indeed use a zero-layer thermodynamics model (Semtner, 1976) to melt or form new ice, as well as for the damage healing.

What we intended to explain therein was that, in order to ensure a fairer comparison between the free drift model and a model with sea ice rheology, one need to make sure that the sea ice thickness/concentration fields during the simulation is as realistic as possible. This is not the case with the free drift model that, by definition, does not limit the amount of ridging for instance, leading to unrealistic thickening and consequent degrade of the forecast performance in terms of drift. We modified the text accordingly (p11. l.16-20).

20) p. 10 last line: Add 'steady state' before 'drift'.

Done

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)

We choose this colorscale in order to highlight the quantitative differences between mu_b in neXtSIM and FD. Indeed, in this case, the neXtSIM panels become very blue, especially in winter. We changed the colorscale as suggested in order to highlight the pattern.

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?

This is only a qualitative visual evaluation based on looking at the figures and checking for common patterns. In our case, we considered the correlation looked obvious.

23) p. 14 line 1: Clarify what you mean by 'the response'...ice velocity? 4

The response in terms of period averages of mu_r and mu_b. We updated the text as suggested.

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

Thanks, We did change the text as suggested.

25) Fig. 13: How do you define the mean sea ice coverage (A=15% contour)?

Actually, the grey area shows the presence of the sea ice at least during 10 consecutive days (the length of simulations) during the winter or summer period. We added a sentence in the caption figure to clarify this point (Fig.14).

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.

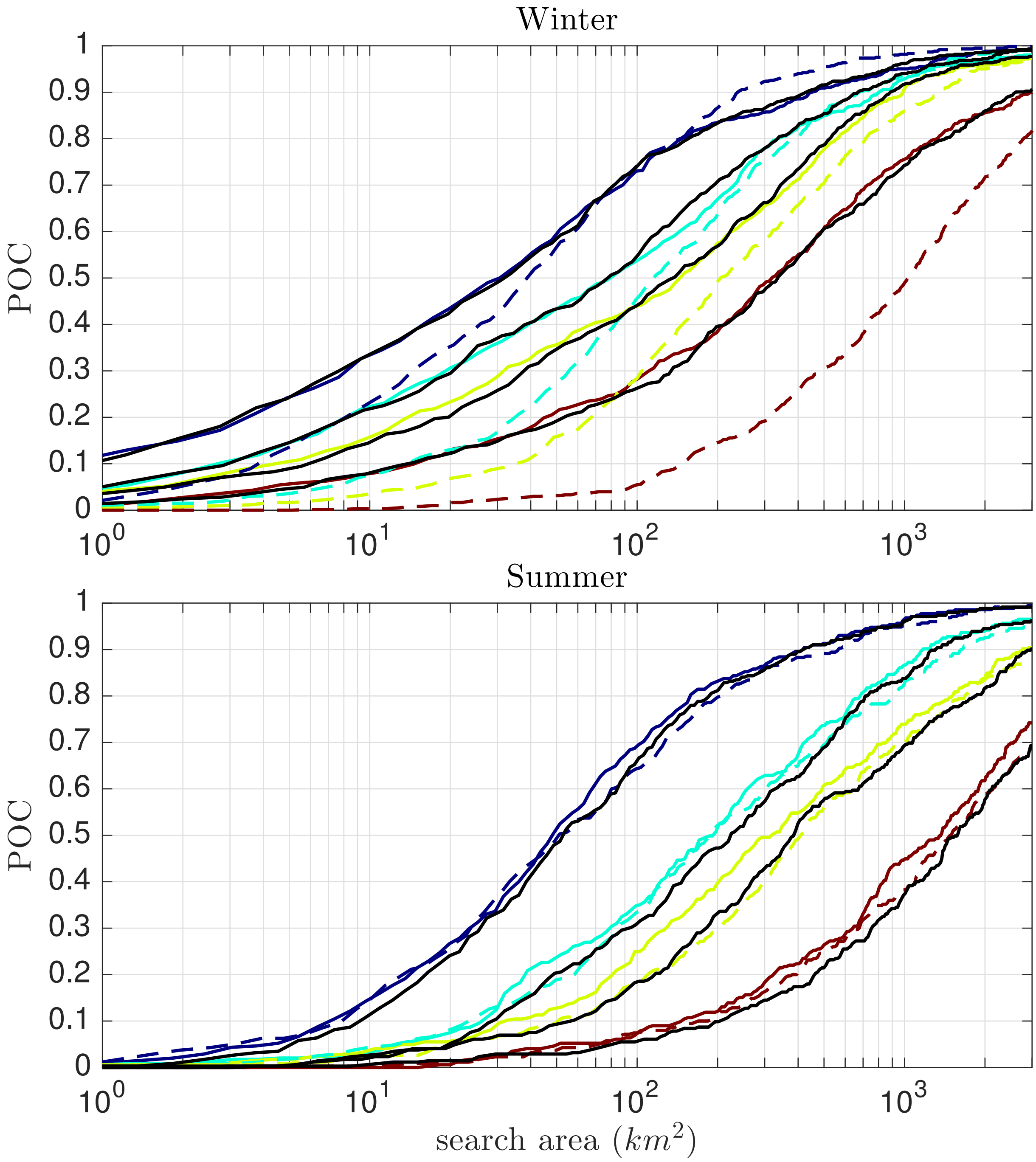
This is correct. We added a sentence as suggested (p.20 l.9-13).

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...

This is a good suggestion. We added a sentence with the exact definition of the POC (p.22 l.13-14).

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?

As explained before: we re-ran deterministic forecasts, using the ASR reanalysis as forcing and the air drag coefficient equal to 0.0065 after optimisation. We obtain similar POCs from ellipses centred on the deterministic forecast (black lines) in winter, whereas they are smaller in summer (see Fig. below).



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

Done

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

Done

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.

We agree that the sentence about the wind was confusing and in contradiction with the point we wanted to make. We therefore decided to remove that sentence.