Review of “Probabilistic forecast using a Lagrangian sea ice model: application for search and rescue operations” by Matthias Rabatel, Pierre Rampal, Alberto Carrassi, Laurent Bertino, and Christopher K. R. T. Jones

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.*

General comments

The manuscript “Probabilistic forecast using a Lagrangian sea ice model: application for search and rescue operations” by M. Rabatel, P. Rampal, A. Carrassi, L. Bertino, and C.K.R.T. Jones provides a comprehensive evaluation of sea ice drift response to uncertainties in wind forcing using the sea ice model NeXtSIM with elasto-brittle rheology. The authors demonstrate through comparison with what is referred to as a free-drift model anisotropic behavior associated with sea ice mechanical properties in winter, with implications for predictive skill. This paper presents novel concepts and tools to highlight the importance of characterizing sea ice mechanics and rheology for such applications as search and rescue operations in winter. It is recommended that this manuscript be accepted for publication, following consideration of aspects including systematic error in NeXtSIM as documented in earlier studies of this Lagrangian sea ice model, spatial variability in the air drag coefficient, boundary condition sensitivity studies, and further investigation of reasons for discrepancies in dynamics for modeled and observed trajectories. Please find below more specific comments for consideration.

This is also to express agreement with the comments of both reviewers on the quality of manuscript, in addition to statements in regards to justification for term selection in the free drift model, and the need for further description as to how the forecasts are initialized.

Specific comments

Introduction

1) p. 2, line 28. In Rampal et al. (2016b), the authors show systematic errors based on comparison of simulated ice drift with the GlobICE dataset (Figure 7). Perhaps note in the Introduction, and provide a figure depicting, the spatial distribution of systematic errors for given timeframes in winter and summer, to distinguish from differences due to compactness and rheology based on comparisons between NeXStSIM and the free drift model. Highlight systematic errors based on comparison with OSISAF.

Thank you for your comment. We have added, in the introduction, a figure depicting the spatial distribution of systematic errors in winter. We do not provide the figure in summer since we do not consider the OSISAF data to be sufficiently reliable in this period (Fig. 1, p.4 l.1-5).

2) p. 3, lines 22 – 29. What parameter values are used in the present study, and in particular for compactness (i.e. as in Table 2 in Rampal et al., 2016b)? In the sensitivity analyses for the compactness parameter in Bouillon and Rampal (2015a) it is noted that the opening and closing rates are influenced by the compactness parameter. How are the current wind sensitivity results influenced by the choice of the compactness parameter?

You are right, we used the same compactness as in Rampal et al 2016b. A list of the values of the parameters was missing in our submitted manuscript. We have now added a table listing those in the revised manuscript.

In this study, we decided to restrict the sensitivity analysis to external parameters only, here the wind, and not to extent it to the internal mechanical parameters of sea ice like compactness, cohesion, etc. This choice is further justified by our mid-term goal of using neXtSIM in conjunction with ensemble-based data assimilation in which context the ensemble would preferably reflect the uncertainty on the external forcing under the assumption that internal parameters have all been already optimised.

We agree however with Reviewer on the relevance of such an analysis but we believe that it is beyond the scope of this paper, and it will be addressed in a different study.

Sensitivity analysis

3) Air drag coefficient and other parameters: Will there be regional variations in the drag coefficients?

There are no regional variations in this present study, both for the sake of simplicity and because constant drag coefficients are still customary in the community.

4) How is spatial variability in the drag coefficients addressed?

This is not addressed in the present study. We assume this coefficient to be constant over time and over the Arctic

5) Is the calibration method used the same as that in Rampal et al. (2016b)?

Yes, indeed. This is the same method used here. We explain it better in the revised manuscript, and also specify the values we obtain for neXtSIM and FD.

6) As previously noted, what value is used for the compactness parameter in this study?

We use the same one as in Rampal et al 2016b, i.e. -20.

7) Specifically: p. 9, line 2 and reference to the OSISAF dataset. Are similar results and values obtained for the air drag coefficient using the globeICE drift product for comparison, as in Rampal et al., 2016b?

We have not done any comparison to GlobICE in this paper as it is important for this specific study (and its goals) to perform the optimisation of the drag over the whole arctic, which would not have been the case if using GlobICE that has significantly less spatial coverage.

8) p. 9, line 6 and p. 8, Figure 2. Is concentration considered to account for spatial variability in the air drag coefficient, as described in Steiner (2001)?

As said before, we do not consider any spatial variability in the drag coefficient in this study. The concentration is actually not directly used to account for any spatial variability of the drag coefficient. But indirectly it is so, since we perform the optimisation only where the simulated drift is close to the free-drift solution, which *happens to be at locations where the concentration is significantly lower than 100%.*

9) In addition, what impact does the drag coefficient have on results?

Although this is an interesting question, this paper is not intended to address it. Still, between the submission and the present review, the drag coefficient of the FD model has been reduced from 5.1 e-03 down to 3.2 e-03, which has changed quantitatively the results in the summer but did not invalidate the conclusions.

10) p. 8, line 15. Perhaps provide justification for this wind speed variance selection (i.e. a value that is 6 times smaller than that used in Sakov et al. (2012).

Note that the value of 6 may sound dramatic while it only makes a factor of 2.3 in standard deviation. If taking the variance used in Sakov et al 2012, the impact on the neXtSIM behaviour is too large, i.e. the ice is breaking up too much leading to excesses of ice drift and very small anisotropy of the ensemble. We therefore decided to reduce that variance to a reasonable level so that the physics of neXtSIM can be expressed. In the future we will compare the relative sensitivities of the model used in Sakov et al. (2012) and neXtSIM.

11) p. 9, Figure 3. Is it possible to also identify and show systematic errors spatially in another panel in this or a separate figure? Please see previous comments for the Introduction.

See comment 1

12) p. 10, line 5. 100 km initial spacing. Are results and differences between the NeXtSIM and FD models influenced by different initial spacings?

We have not tested this.

However, if using e.g. 50km, we may not be able to consider anymore that a given drift trajectory of 10days is independent of each other as a given virtual buoy would sample more than one "box" over that period (average sea ice speed is about 6km/day in winter). So, taking 100km almost ensure that the trajectory members started from the centre of one box are independent of the trajectory members of the neighbouring box.

Results

13) p. 12, Figure 5. Should the contours for the lower panels be the same (i.e. <= 3 for both)? If not, perhaps emphasize the difference in diffusive spread spatial scales for the FD and NeXtSIM models since this, in addition to similarity in spatial patterns between minimum and maximum diffusive spread for both models is of interest and relevant to the present study.

We adjusted the colorscale as you suggested (Fig. 6).

14) p. 13, Figure 7. Similarly, the contour range should be the same. Sea ice dynamics are different for neXtSIM and FD even in summer. Perhaps include in the text a possible explanation for these differences (i.e. systematic error, parameter selection, FD characterization).

We tried to use the same color scale for FD and neXtSIM but in this case, on one Figure of them (either FD or neXtSIM), one cannot see any pattern anymore. Finally, we choose to leave different colorscales in order to discuss on mu_b pattern.

15) p. 14, line 15. '...effective elastic stiffness E depends non linearly on the ice concentration...'

Should this nonlinearity (and spatial variability) also be considered when optimising for the air drag coefficient? Should this too be considered with optimising for the air drag coefficient? Please see previous comments.

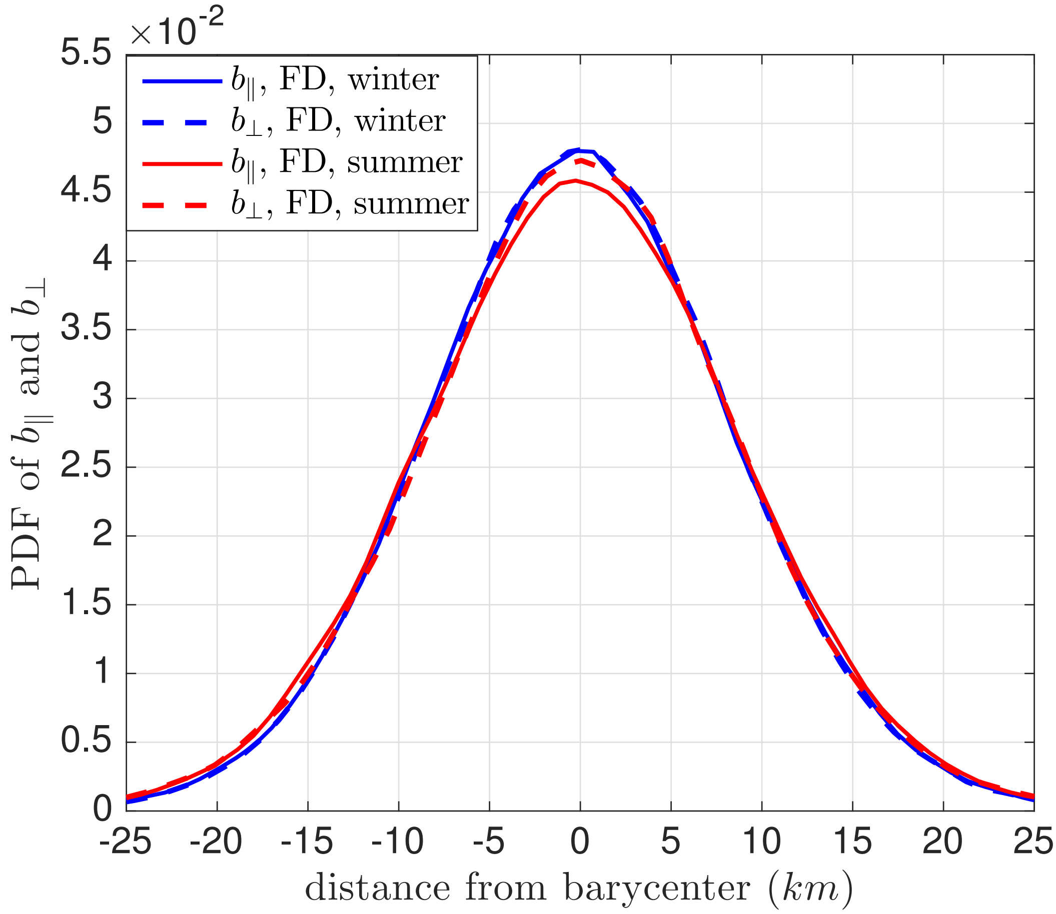
Such an optimisation of the drag where the rheology is active represents a complex non-linear inverse problem, highly sensitive to poorly known initial values (the ice damage and ice thickness among others). Our optimisation using free drift "events" is precisely intended to solve a simpler linear problem still using a sufficiently large number of observations.

16) p. 14, line 24. 'Where both (winds and ice thickness) are large, \gamma is large'. However, \gamma is also large in the southern Beaufort Sea for large winds and lower ice thickness in winter. Figures depicting maps of \gamma for the NeXtSIM and FD models in winter and summer would also highlight the impacts of ice rheology.

Thank you for your comment. Yes, indeed, both winds and ice thickness are not the only explanation. Perhaps, we may add: the sea-ice motion occurs mainly in parallel to the coasts because motion towards them tends to be suppressed by counteracting ice pressure. In summer, these coasts do no longer play the role of closed boundaries and the increase of R is almost no visible. This is corroborated by observing the pattern from FD where the pressure term does not interfere. We updated the text accordingly (end of p.18).

17) p. 15, Figure 9 caption. 'The PDFs for FD are similar for summer and winter...' Perhaps still show both PDFs in a separate panel with a different y-axis scale.

Thank you for your comment. We present this figure below. We believe it is not helpful to add it to the revised manuscript.



18) p. 15, lines 4 – 6. How are lateral boundary conditions (i.e. landfast ice and its extent)

addressed in the model? Would sensitivity analyses associated with boundary conditions highlight regional differences in anisotropy and preferential orientation?

We are not sure to well understand the question of the referee. However, here is our answer:

The lateral boundary conditions are either "free" (if at the ice edge) of "fixed" (if at the coast). If the ice cover does not extend anymore to the coast, the boundary conditions are therefore very different, and this likely has an impact at least over the peripheral band of ice near the ice edge. Further into the ice pack, the impact of the boundary conditions on the sea ice drift and anisotropy of the dispersion is less important. Local sea ice conditions (compactness/concentration and damage) are in this case more likely to be responsible for the anisotropy of the dispersion, which is what we discuss in our study.

As a conclusion, we do not think that performing sensitivity analyses associated with boundary conditions would reveal key information to understand the source of anisotropy of the ensemble spread.

19) p .16 and Figure 12. What are the possible reasons for discrepancies between the observed and modeled ice drift dispersion characteristics and temporal scaling exponents, namely the superdiffusive regime, in summer? Could superdiffusive behavior be attributed to other sources of uncertainty responsible for systematic error in the model?

We suggest that the super-diffusive behaviour we obtain for summer 2008 with neXtSIM, and which is in apparent contradictions with the results of Rampal et al. (2009) could rather be the fingerprint of a change in sea ice dynamical regime that occured over the most recent years, as a consequence of the thinner and more mobile sea ice cover . In this case, it would mean that the effect of the rheology became weaker (if not absent) in summer and that the sea ice response is now more directly related to ocean currents and winds, and therefore can exhibit super-diffusive behaviour as also reported in Lukovich et al. (2015). We modified the text accordingly (end of p.19 and beginning of p.20).

20) p. 17, Figure 11. Contour range should be comparable for the FD and NeXtSIM models. Is it possible to use the anisotropy ratio featured in Figure 11 to improve predictive skill for NeXtSIM?

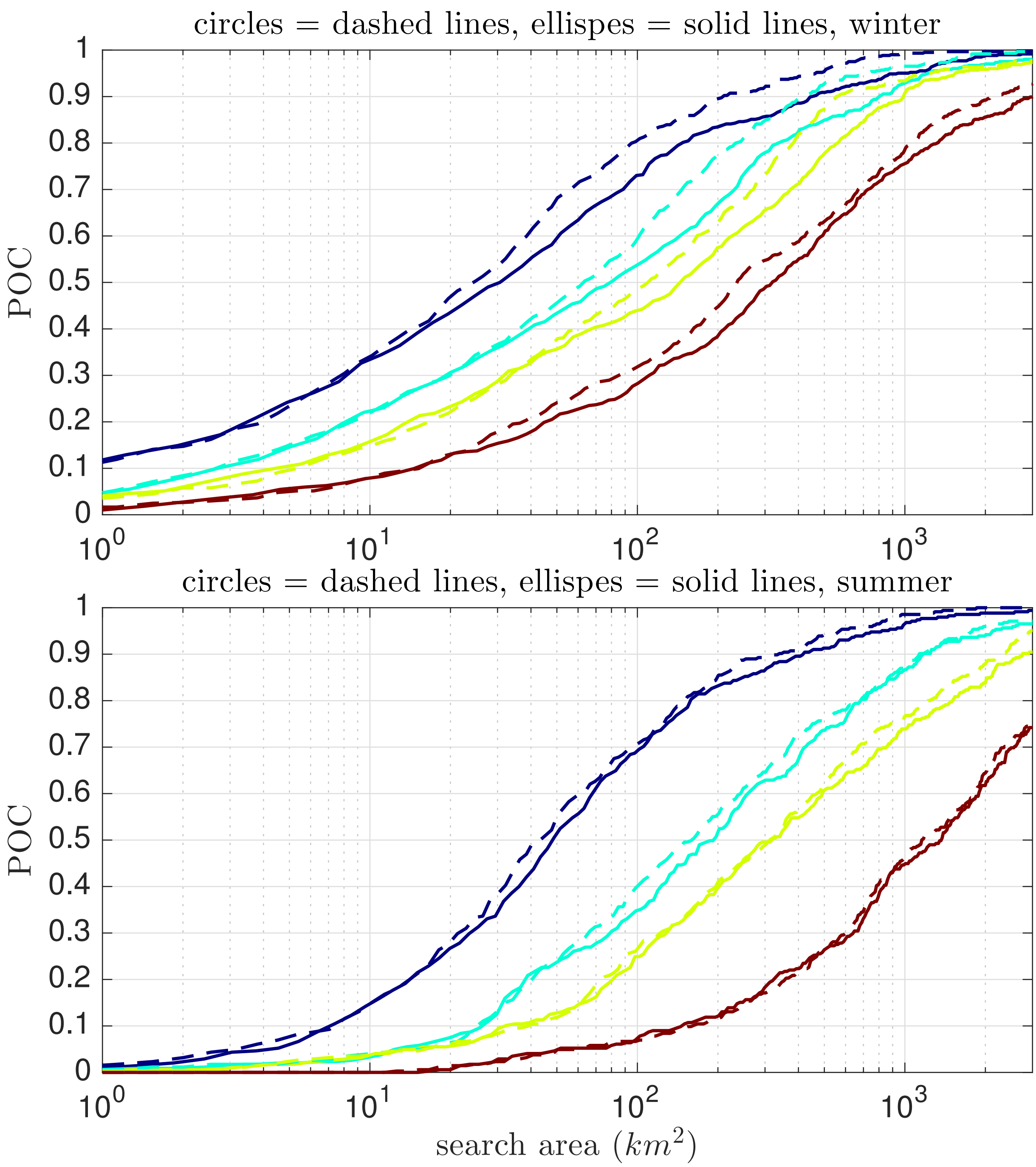
Unfortunately, if we use the same colorscale, either one of the patterns of neXtSIM or FD will disappear. We choose to keep different colorscale in order to exhibit the absence of spatial coherence for FD on the one hand, and on the other, the difference between ice close to the coast and ice in the center.

21) p. 17, line 10. The forecast error vector components should be depicted accurately in Figure 15.

We updated the Fig. 15 (16 in the revised manuscript) as you suggested.

22) p. 19, Figure 14. How are **e**, **b**, and **a** related when considering the anisotropy ratio and is relation to forecast error? Variance in parallel and perpendicular components of b could also be compared with those for the forecast error in this figure or in figure 12 to demonstrate the anisotropic effects associated with elasto-brittle rheology.

The comparison of ensemble spread and errors on the same graph would not be helpful because the spread is underestimated by both models (see the new Figure 18). Also, note the answer to a related question (29)) from Reviewer #2, and the complementary graph below. The strong anisotropy may remain more of a hindrance than an advantage to search forecasting as long as the deformations are not assimilated in the model.



23) p. 21, line 19. 'for an equal area that can be searched' Does this imply for a fixed area?

Not fixed in the sense that the geometry is different (circle versus ellipse) the area included in the ellipse/circle is the same. Thus the sentence should be understood as: taking an ellipse (or circle) from either models with the same encompassed area, which of the two is more likely to contain the object? We have replaced equal by "a given area", hoping this will be clearer (p.25 l.5).

24) p. 25, lines 5 – 7. Would it be possible to quantify these contributions in additional sensitivity analyses?

The contribution of ice drift to the TOPAZ system with respect to other assimilated observations is quantified in Sakov et al. (2012) using the Degrees of Freedom for Signal. neXtSIM does not assimilate the same observations but the maps of μ_b in Figure 6 represent a sensitivity analysis of ice drift to spatially and temporally stationary perturbations of the winds.

Technical corrections

p. 1, line 12. Replace ‘of free-drift’ with ‘the free-drift’.

Done.

p. 2, lines 33 – 34. Combine the sentence ‘Without...’ with the next sentence.

Done

p. 4, line 10. Change ‘spatial’ to ‘spatially’.

Done

p. 5, line 24. Change ‘analysis’ to ‘analyses’.

Done

p. 6, line 25. Change ‘informations’ to ‘information’

Done

p. 7, Figure 1 figure caption. Perhaps replace ‘bouquet’ with ‘-member ensemble’.

Done

p. 11, line 25. Please change to ‘Chukchi’

Done

p. 11, line 30. Please replace ‘inn’ with ‘in’

Done

p. 14, line 14 ‘influences’

Done

p. 19, line 4 Replace ‘get very’ with ‘are’

Done

p. 21, line 19. Insert ‘be’ prior to ‘posed’

Done

p. 22, line 5. Perhaps replace ‘allow as also’ with ‘also allows’

Done

p. 22, line 27. Replace ‘of’ with ‘by’

Done

p. 22, line 30, Perhaps remove ‘up’

Done

p. 22, line 31, Perhaps replace ‘reveals’ with ‘FD is observed’

Done

p. 23, line 3, Replace ‘sensitivity’ with ‘sensitive’

We are used to seeing the wording “sensitivity experiment” rather than “sensitive experiment” which we interpret as an experiment dealing with a sensitive topic. So we would prefer keeping “sensitivity” (p.27, l.6).

p. 23, line 6, Replace ‘contrarily’ with ‘in contrast’

Done

p. 24, line 21, Replace ‘called’ with ‘considered’

Done

p. 25, line 6, Remove ‘yet’

Done

Reference

Steiner, N., 2001: Introduction of variable drag coefficients into sea ice models, Annals of Glaciology, 33, 181 – 186.