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Interactive comment on "Probabilistic forecast using a Lagrangian sea ice model: application for search and rescue operations" *by* Matthias Rabatel et al.

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Summary

Rabatel et al. investigate the drift behaviour of virtual buoys in a sea-ice model with elasto-brittle rheology and a Lagrangian unstructured mesh. They construct ensembles by driving several realisations with different spatio-temporally correlated wind perturbations added to reanalysis-based atmospheric forcing. They compare results to a simplified free-drift version of the model to assess the impact of the sea-ice rheology. The authors compare simulated ensembles of trajectories with observed drift from IABP data. They show that the model with rheology better captures anisotropic drift

dispersion and, despite being underdispersive, can help to define search and rescue areas with increased probabilities of containment compared to the free-drift model, in particular in winter.

The paper is well written and the quality of the research and presentation is high. I have a number of remarks that might help clarifying some aspects, including quite a number of technical corrections and comments. Overall, I clearly recommend the manuscript should be accepted, after minor revisions.

(Note that I have not read the other review prior to writing this review to ensure an independent assessment.)

Specific comments

Concerning terminology, I think it would be worthwile to clarify that this study is using the term "forecast" not in the sense of forecasting actual future trajectories, where the future evolution of the system is becoming more and more uncertain over the forecast lead time through chaotic error growth, but in a slightly different way where the future evolution of the most chaotic component - the atmospheric forcing - is approximately known. Of course uncertainties are introduced in another way, namely through perturbations of the atmospheric forcing, but still the underlying synoptic evolution is the same in all ensemble members. I'm not trying to say that this is not worthwile doing; in particular, one can imagine search & rescue applications where one aims to find the current position of a target that got lost 10 days ago, so one could run a "forecast" system like the one used here to "nowcast" the current position using near-real-time atmospheric (re-)analyses. And, obviously, one could also use actual atmospheric forecasts to drive the model, but that is not done in this study, so I recommend to just clarify this.

P2L33: "departing from independent in situ drifting buoys, and compare them with real observations"; Does "in situ drifting buoys" not refer to the "real observations"? Please clarify.

P3L28-29: "the impact of some mechanical parameters on the ice deformation can still be considered as valid"; the "some" sounds very vague, could you be more specific?

Eq1: If I am not mistaken, this holds only when h and h_s are the "effective" (grid-cell averaged) thicknesses, correct? And for A it stops holding for A close to 1, in particular if there is a lot of damage where there can still be considerable convergence despite A=1 (even with the "pressure term"), right? This would deserve some clarification.

Eq5: It may help to mention what value is used for alpha (probably -20 as in Rampal et al. 2016?) so the strongly non-lineas dependence on A becomes obvious.

P5L24-28: To me this paragraph sounds very vague; could the authors be more specific on what inputs and outputs are considered?

Eq8+9: It might be worth noting that the means of "b_i,II" and "b_i,L" are zero and thus omitted in Eq9. It might also be worth pointing out that mu_b contains basically the same information as "b_i,II" and "b_i,L" (except the directional information); they do not relate to each other like the first and the second momentum of a distribution (which is not stated, but at least I was confused at first).

Fig2: What data and analysis is this figure based on? And what temporal sampling frequency is used to detect "events", e.g., one day?

P9L17: "the internal stresses in the ice, and the corresponding Grad(sigma h) term in Eq. (2), becomes very large and dominant"; Would it be more precise to say that it almost completely balances the other forces (so that the acceleration (and speed) becomes very small)?

P10L6: "We ran an ensemble of 12 members, each of them forced by the perturbed wind dataset generated as explained above"; If I have not overseen some important detail, there is some information on the experimental setup missing. In particular, how are the sea ice and ocean in the different members initialised? Is there one single "reference run" from which the ensembles are brached off, with all members keeping the

same initial sea-ice/ocean state? If so, does the reference run also have perturbations to the winds (and accordingly uses the re-tuned parameters)? Or are there just 12 simulations overall, covering the whole time period, so that the "initial" sea-ice/ocean states are different between the ensemble members? The latter doesn't seem to be the case as you speak of inidividual simulations in P10L3. Also, P25L5-6 seems to hint that indeed the initial states are identical. In any case I have the impression that the question of whether or not the initial sea-ice states are identical is very important for the interpretation of some of the results (see below), so I think this should be described very clearly.

P10L9: "8000 virtual buoy trajectories over the winter season"; Is this the number of ENSEMBLES of buoy trajectories? For individual trajectories I would expect a larger number, given the approximate number of initial positions in Fig4 and the number of 10-days periods.

Eq11: While you can certainly say that the omitted presseure term, as the stress term, belong to the rheology, the omitted tau_b could also be mentioned.

P10L19: "The FD model therefore mimics the drift of a buoy at the surface of the ocean."; I would think that this is not really the case because the drag coefficients would be quite different (in particular on the water side due to turbulent momentum transfer between deeper layers and the surface water surrounding the buoy)?

Fig5+7: i) I do not understand why the sea-ice thickness pattern is so clearly visible in the dispersion strength (mu_b) for the free-drift model where the rheology shouldn't play any role; could the authors comment? ii) I suggest to use the same colour scales for the two bottom panels so that the difference in mu_b becomes even more obvious.

P14L1: "In both winter and summer, the response to wind perturbations is overall lower by 35% in neXtSIM than in FD"; Where does this number come from? I would have thought that the difference of mu_b in neXtSim versus FD would quantify "the response to wind perturbations", but those are reduced by 63% and 39% in winter and summer, respectively (as statet in P14L5-6), so that doesn't fit. Could you please clarify? (Also at the beginning of Sect.5)

P14L21-27: Is the assumption correct that the values found for the ratio mu_r/mu_b should scale with the strength of the wind perturbations? If so, this might be worth mentioning.

P15L8-9: "This reveals that the ice will first tend to move compactly along the wind direction away from the origin, but it then starts to break and depart from the barycentre"; First, the wind directions felt by the different ensemble members differ instantly after the initialisation, right? So, moving compactly along the wind direction would imply a slightly different direction for each member from the very beginning. Second, the ice is "broken" (i.e., has fractures) already at initial time, right? Third, and maybe more importantly, I think that the interpretation of the decreasing anisotropy might depend strongly on the initial sea-ice state: Assuming that the sea-ice initial states are identical for all ensemble members, even slightly different winds will initially tend to drive motion in the same direction because the motion is strongly constrained by the pattern of fractures. Only after some time will the pattern of fractures differ between the ensemble members, and then the sea-ice motion fields will also be more different between the members. Could this not explain why the anisotropy is even larger at the beginning in neXtSIM and then goes down to lower values? This argument of course requires that the initial sea-ice states are identical, so that should be clarified.

P16L11-13: "We found that the ensemble spread follow two distinct diffusion regimes, one for small time t«Gamma and one for large time t»Gamm where Gamma is the so-called integral time scale (Taylor, 1921), which is about 1.5 days for sea ice according to Rampal et al. (2009)"; Do I understand correctly that this integral timescale is quite directly determined by the autocorrelation timescale of wind anomalies or - in the present study - by the autocorrelation timescale of the wind perturbations? It might be worthwile pointung out that this subtle difference exists between the present and the Rampal et al. 2009 study.

P17L1-2: "Predictive skills" and "able to forecast real trajectories"; please see my general comment on the way the term "forecast" is used in this study.

P15L12-14: "We observe that highest degree of ensemble anisotropy (R > 1) is found north of Greenland and Canadian Archipelago, where the ice is the thickest and the ice drift and winds the lowest, in overall agreement with the interpretation of the temporal evolution of R for neXtSIM in the winter"; There are also high values of R along the Eurasian and Alaskan coasts; can't this be explained by the fact that the sea-ice motion (and the associated dispersion) occurs mainly in parallel to the coasts because motion towards the coast tends to be suppressed by counteracting ice pressure (even when the thickness is moderate)?

Fig11: For my taste it would again be better to use the same scale for all panels.

Fig12: If I understand correctly, the slopes at lower timescales are all approximately 2. I suggest to note that also in the plot (as is done for the longer timescales).

P19L10-11: "For FD, e_L still being positive for both periods, corresponds to a drift too far to the right in the observations"; What is meant by "to the right in the observations"? And is e_L for FD not NEGATIVE according to Fig14 right?

P21L10-13: "even if the forecast errors are smaller in neXtSIM than in FD, its shrunk search areas lead to a smaller POC for neXtSIM than for the FD model (not shown): in practice the probabilistic forecast from neXtSIM is too optimistic, underestimates the uncertainties in the forecast, while the FD forecast overestimates them"; First, I would in fact like to see a graph that shows how the spread (mu_b) versus the error evolves. In weather forecasting, the "spread-error relationship" is a common way to measure whether probabilistic forecasts are underdisperive ("too optimistic") or overdispersive ("too pessimistic"). The latter terms could be introduced also in the context of this study.

P22L31-33: "The fact that most of the superiority of neXtSIM over reveals during winter is, as stated in previous instances, in full agreement with the expectations, given that during the summer the ice mechanics in the two models is similar"; Please check the grammar of this sentence.

Fig17: Could the superiority of FD at very short lead times and for large search areas (for which the skill of the barycenter is not important) be explained by the possibly too strong anisotropy of neXtSIM close to the initial time, due to the shared fracture pattern in all ensemble members (if the sea-ice initial states are indentical, see my previous remarks)?

P24L7-8: "This mechanism is missing in the absence of rheology (like in the FD model) and represents a clear strength and advantage of the elasto-brittle rheology in neXtSIM"; Could the authors comment on what differences one might expect for other rheologies like the standard (E)VP?

P24L18: "The model sensitivity to winds has been evaluated"; Wouldn't it be more precise to say "The model sensitivity to wind perturbations has been evaluated"?

P24L20-P25L1: "we are confident that the spread simulated by the model is physically consistent. Alternative sources of biases must be called such as, for example, other model inputs (thickness, concentrations, damage, ocean currents)"; Deficiencies to simulate reliable spread are commonly not referred to as "biases". Also, what does "must be called" mean here? Maybe in the sense of "must be mentioned?" And why to you refer to those model variables as "inputs"?

Technical corrections / comments

P1L6: "10-days" -> "10 days"

P1L10: "in Arctic" -> "in the Arctic"

P1L12: "to of free-drift model" -> "to the free-drift model"

P2L33-34: "Without aiming to make it a key objective."; In terms of grammar, this

seems to be an incomplete sentence.

P3L7: "measures" -> "measurements"

P3L8-13: Please check these lines for grammar (including commas).

P3L19: "stands on the fact"; sounds strange.

P3L31: "as follow" -> "as follows"

P4L3: "Generalities"; I do not think that this term is commonly used this way.

P4L4: "description neXtSIM" -> "description of neXtSIM"

P6L10: "a initial position" -> "an initial position"

P6L22: "explicit mention on the dependence" - "explicit mention of the dependence"

P6L25: "informations" -> "information"

P6L28: "Let consider" -> "Let us consider"

P8L14: "the and the wind" -> "and the wind"

P8L20-21: "ASR reanalysis" -> "ASR" (two times)

P15L3: "On another hand" -> "On the other hand"

P17L11: "average module"; ?

P20L12: "all simulated ensemble of buoys" -> "all simulated ensemble members"

P21L19: "can posed" -> "can be posed"

P22L4: "models comparison" -> "model comparison"

P22L5: "allow as also" -> "allow us also"

P22L27: "larger of about" -> "larger by about"

P23L7: "hold for" -> "hold also for"

P25L18: "a elasto-brittle" -> "an elasto-brittle"

P26L4: "founded" -> "funded"

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