

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

H. F. Goessling (Referee)

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.*

1) Concerning terminology, I think it would be worthwhile 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 worthwhile 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.

Agree, we specify in the introduction that we work in the context of a “hindcast-forecast” with analysed atmospheric forcing, which error uncertainties are emulated as representative of forecast errors. In the context of data assimilation, this procedure is akin to the propagation step of Ensemble Kalman Filter for generating “forecast” error covariance matrices (which should also be called “hindcast-forecast” to be precise).

2) 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.

Thank you for your comment. No indeed, we replaced the not suitable term: “in situ” by “virtual”.

3) 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?

Yes indeed. Actually, all mechanical terms are involved. We changed the text (p.3 l.32).

4) 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.

Yes, the h and h_s are indeed volumes per unit of area and A is bounded to 1, even when ridging occurs.

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

Yes indeed, we used the same α value, i.e. -20, as in Rampal et al. 2016b. To make sure this is clear to the reader, we decided to add a table listing all the parameters of the model and the values used for our experiments.

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

We removed that paragraph.

7) 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 μ_b contains basically the same information as " $b_{i,H}$ " 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).

Thank you for your comment. Yes indeed, the means of $b_{i,H}$ and $b_{i,L}$ are zero and highlight only directional information. However, as the directions are perpendicular, we use the sample standard deviation of these distances to define the anisotropy. Then, $b_{i,H}$ and $b_{i,L}$ are no longer used in the paper. We rephrased the paragraph to avoid confusion (end of p.8).

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

About events, the temporal sampling frequency used is one day. The ice velocities from these events are compared with the observed ice drift from OSISAF dataset. We updated the text accordingly.

9) P9L17: "the internal stresses in the ice, and the corresponding $\text{Grad}(\sigma_h)$ 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)?

Thanks for this suggestion. We rephrased the sentence to avoid confusion; the use of the word "dominant" was indeed not a good choice. This is now clarified in the revised manuscript (p11. l.11).

10) 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 individual 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.

Thanks to your comment: we completely missed to provide explanations on initial conditions. Indeed, all members are initialised with the same sea ice state coming from the reference simulation presented in Rampal et al. 2016b on the same period with same external forcings without wind perturbations. We have now added all the details regarding initialisation in the revised manuscript (end of p.11 and beginning of p.12).

11) 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.

You are right. It was confusing in the text, we updated the text stating the total number of trajectories (12 times greater).

12) Eq11: While you can certainly say that the omitted pressure term, as the stress term, belong to the rheology, the omitted τ_b could also be mentioned.

Yes, this is a good point. We updated the text accordingly (p.12 l.13).

13) 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)?

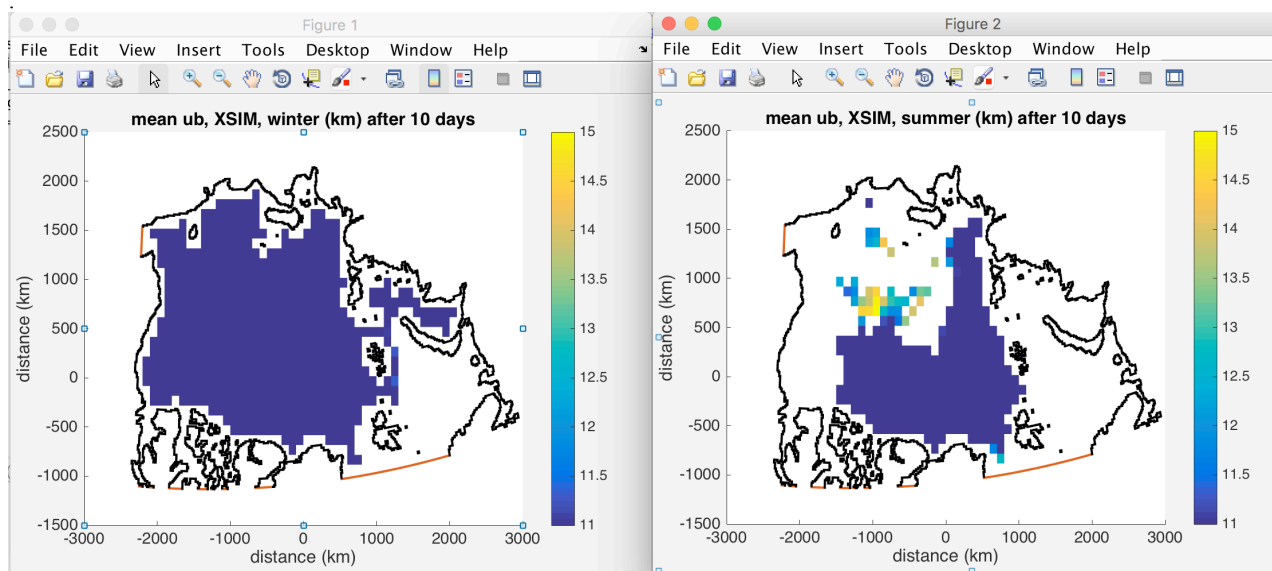
Correct, a buoy in the open ocean would experience a different ocean drag. We use the term « mimics » for the sake of the analogy, given that The free drift case is analogous to the pure Ekman drift case in ocean dynamics. We have rephrased the text to avoid the confusion (p.12 l.16).

14) Fig5+7: i) I do not understand why the sea-ice thickness pattern is so clearly visible in the dispersion strength (μ_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 μ_b becomes even more obvious

i) This is because the free drift model (eq. 11) takes into account the ice mass via the Coriolis and gravity force

terms. The thickness patterns, which are used as initial conditions and which are the same as in the corresponding neXtSIM simulations, are therefore reflected on both the advective and dispersive response of the FD model.

ii) We tried to use the same colorscale 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 μ_b pattern (see Fig. below).



15) 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 μ_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 stated in P14L5-6), so that doesn't fit. Could you please clarify? (Also at the beginning of Sect.5)

Thank you for your comment. Actually, this number was a global mean over both periods and both distances, but we removed it and changed the sentence.

Yes indeed, the distance μ_b provides quantitative information on the response to wind perturbations. In summer, the reduction shows the behaviour of neXtSIM is closer to FD. This may be explained by the fact that, in summer, the ice concentration is reduced leading to a significant decrease of the internal stresses within the ice (p.131.23-32).

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

It is an interesting assumption. Besides that, we studied this ratio on different ways: looking at the spatial pattern and the time evolution. However, we could not highlight any relevant correlation with wind perturbations and/or physical quantities. This ratio is roughly driven by μ_r and is not directly related to the wind perturbations.

17) 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?

You are right.

18) 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?

You are right, the initial damage is taken from the outputs of simulations used in Rampal et al. (2016b) for neXtSIM (not used in FD) and it is the same for all members.

19) 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.

We agree with you.

20) 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.

You are right. We changed the text to highlight this valid interpretation (p18).

21) P16L11-13: "We found that the ensemble spread follow two distinct diffusion regimes, one for small time $t \ll \Gamma$ and one for large time $t \gg \Gamma$ where Γ 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 worthwhile pointing out that this subtle difference exists between the present and the Rampal et al. 2009 study.

This is not true. The integral timescale can be influenced by the unperturbed winds and ocean forcing fields, the perturbations autocorrelation and (in the case of neXtSIM), the rheological model. Since the perturbations of the winds have an autocorrelation of 2 days, quite close to the limit of 1.5 days, it is unfortunately not easy to tell their effect apart from other effects, but we have not tried to add additional experiments for that matter.

22) 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.

We reminded the reader that this study is "in hindcast mode".

23) 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)?

Thank you for your comment; this is an interesting point. We agree with you and we updated the text accordingly (p18).

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

Unfortunately, with the same colorscale, either pattern of neXtSIM or FD will not be longer visible. 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 coverage close to the coast and in the center of the domain.

25) 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).

Yes indeed. We updated the figure as you suggested (Fig. 13).

26) 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?

Yes indeed. This is an unfortunate typo, we corrected. The vector e is directed from the observation to the barycentre. Thus, standing on the observation and looking in the drift direction, a negative e_L corresponds to seeing the barycentre to the right (Fig. 15).

27) 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 (μ_b) versus the error evolves. In weather forecasting, the "spread-error relationship" is a common way to measure whether probabilistic forecasts are underdispersive ("too optimistic") or overdispersive ("too pessimistic"). The latter terms could be introduced also in the context of this study.

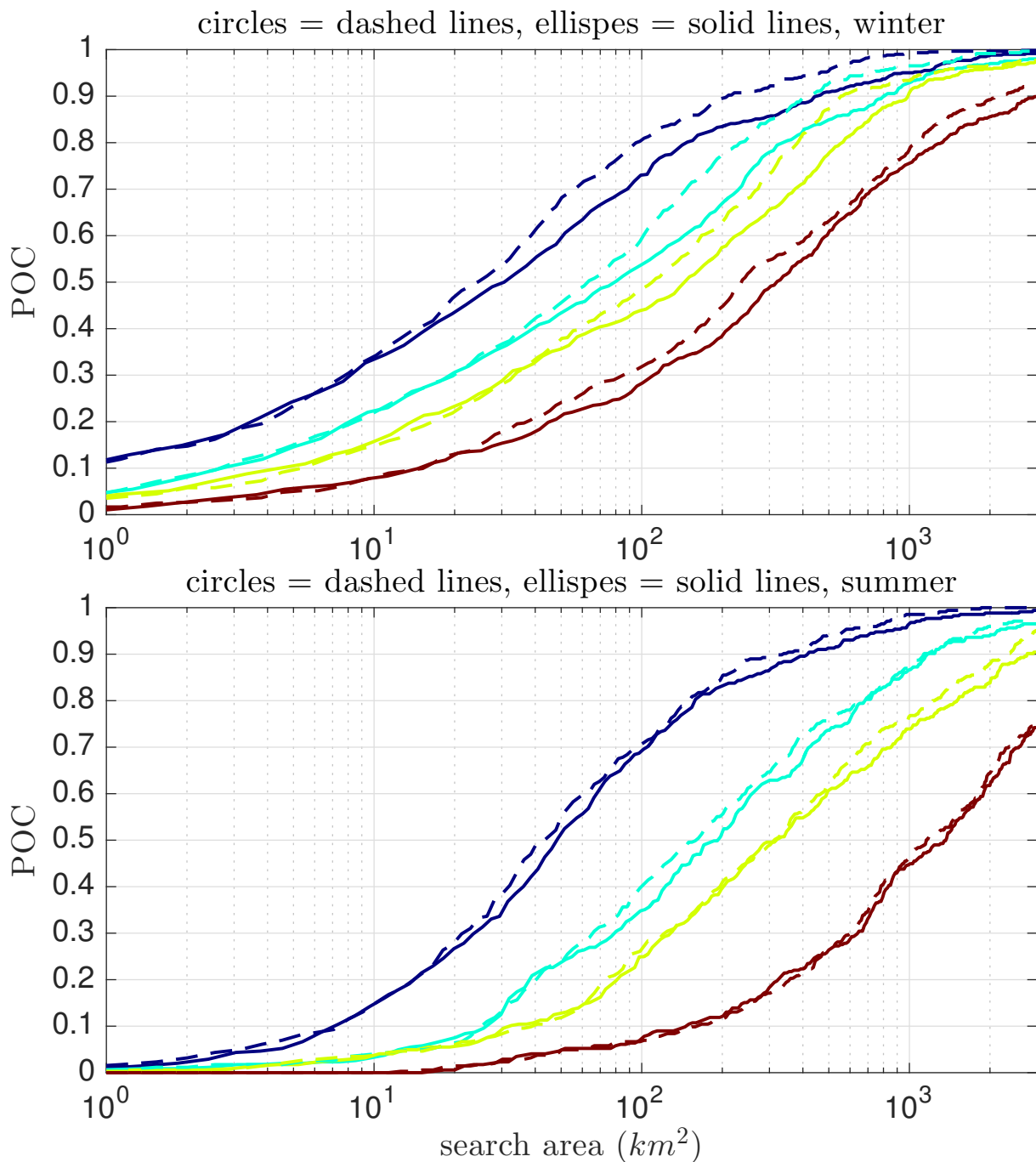
Thank you for your comment. We added a "spread-error relationship" graphic (Fig. 18 on the revised version) as you suggested. Actually, both models are underdispersive, however, where the spread is larger than 10 km, only neXtSIM seems become too pessimistic. We have added comments on this somewhat surprising behaviour (end of p.23).

28) 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.

Done. Hopefully improved.

29) 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 identical, see my previous remarks)?

Thank you for your comment. This is an interesting point. Indeed, when we define the search area as a circle (=without anisotropy), the POC from neXtSIM, for small time horizons and for large search areas, becomes greater than FD. For other time horizons, smaller areas and in summer, the difference is far less visible (see Fig. below). This means that the ensemble run is "too confident in its anisotropy", which could be improved with a better initialization of the ensemble. We added this note in the revised manuscript.



30) 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?

We have noted some anisotropy in the EVP model in a previous experiment with TOPAZ (Bertino et al., 2015), which could unfortunately not be compared to the present results since it was not carried out in similar conditions.

Bertino, L., Bergh, J., and Xie, J.: Evaluation of uncertainties by ensemble simulation, Tech. Rep. Tech. Rep. 355, NERSC, ART JIP Deliverable 3.3, Bergen, Norway, 2015.

31) 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"?

Yes.

32) 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 do you refer to those model variables as "inputs"?

We significantly rephrase these sentences in order to clarify our discussions (p.28 l.23-30).

Technical corrections / comments

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

Done

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

Done

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

Done

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

Done

P3L7: "measures" -> "measurements"

Done

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

Done

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

Done

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

Done

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

Done

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

Done

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

Done

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

Done

P6L25: "informations" -> "information"

Done

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

Done

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

Done

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

Done

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

Done

P17L11: "average module"; ?

Done

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

Done

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

Done

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

Done

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

Done

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

Done

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

Done

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

Done

P26L4: "founded" -> "funded"

Done