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Title: Towards improving short-term sea ice predictability using deformation observations

This is a second review of the paper entitled “Towards improving short-term sea ice predictability using deformation observations”. Results from the paper show that data assimilation of (reconstructed) concentration and damage derived from observed total deformation improved LKFs forecast up to two days and that the model is able to fill the gaps in the Sentinel coverage (used to calculate the total deformation) with LKFs that are qualitatively reasonable. The authors implicitely assume that the ability of the model to fill the Sentinel gaps stem from the BBM rheology by suggesting that an mEVP rheology would not be able to achieve this.

The paper is much improved, and the authors have addressed satisfactorily most of the comments from the reviewers. There is still, however, some unsubstantiated statements, misleading statements and a circular argument in the approach used in the assimilation procedure in the revised manuscript. The paper attempts two things a proof of concept that (A) assimilation can improve LKFs forecast and (B) that the BBM rheology is responsible for this improved forecast including being able to fill in the gap in Sentinel coverage, but neither is done convincingly. Both goals would be welcome contributions to the community, but in its present state the manuscript is not satisfactory. The authors should choose one option and present it in a convincing manner (see points below for details). I recommend that the paper be accepted for publications after the comments below are fully addressed (i.e., not rebutted).

Major comment:

General: Irrespective of the goal of the paper (Option A or B), the following points must be addressed prior to publications.

1- The study is simple. The ice strength is set to nearly zero along observed lines of high deformations (Linear Kinematic Features, LKFs), and the model in turn produces large deformation along the observed LKFs where strength is set to ~zero. Not surprisingly, the model is also able to link observed LKFs when there are observational gaps along a given LKFs (this was shown in idealized experiment by Ringeisen et al 2019). The ice strength in the BBM model has an exponential dependence on ice concentration (or open water fraction) and a linear dependence on damage. Given that total deformation cannot be assimilated in a simple manner – because it is not a prognostic variable – transfer functions (operator) are developed between simulated total deformation and simulated concentration and damage. These transfer functions are then used with observed total deformation in order to derive reconstructed damage and concentration that are used in the assimilation. This approach implicitly assumes that the relationship between deformation, sea-ice concentration and damage in the model is correct. This assumed equivalence between simulated and observed total deformation is problematic. I suggest instead:
    a. Damage cannot be observed and therefore, a transfer function must be developed from model output, as done by the authors.

    b. Concentration however can be calculated directly from Sentinel-derived divergence (eps_I) contrary to what is done in the paper – see for instance work by Kwok who has produced a dataset of thin ice based on RGPS divergence as an example of how this is done. The reconstructed concentrations from observed divergence should be assimilated in the model, instead of the reconstructed concentrations obtained with the model-trained operator.

2- Another choice made by the authors is to associate total deformation with a reduction in sea ice concentration irrespective of the sign of the divergence – which is clearly not realistic. Radarsat-derived divergence PDFs are nearly symmetrical around zero with an equal number of scenes with positive and negative divergence. Presumably, the authors make this assumption because the assimilation of damage (present in both divergence and convergence) does not improve the predictability of LKFs. Instead, a lengthy and unclear discussion related to timescale is included to explain why damage does not improve predictability. This may suggest instead that the functional dependence of ice strength on damage is not appropriate. See item #3 below for more on this issue.

3- The two stated reasons for why damage does not improve LKFs forecast (different timescale; linear vs exponential dependence) are not convincing. I would argue instead that this may be an indication that damage is not correctly parameterized in the EB/MEB/BBM model.

    a. **Timescale**: The important timescale for damage to consider is not the timescale for damage growth but rather the timescale for damage healing. If damage grows and propagate in the correct direction – along the line of maximum stress (Line XX) – the benefit of that should be seen one day later.

        i. The authors states that hourly Sentinel observations would allow to test this hypothesis, but they do not exist. A feasible way to test the hypothesis would be to slow down the propagation of damage in the model and see if assimilation improves LKFs forecasts.

    b. **Exponential vs linear dependence**: The fact that assimilation of sea ice concentration has a positive impact on LKFs forecast and damage has not, suggest that perhaps ice strength should also have an exponential dependence on damage.

4- Another result that suggests that Pmax may not have the proper functional dependence on damage is the fact that the authors must resort to decreasing sea ice concentration (and therefore ice strength Pmax) whether divergence or convergence is present in the observations. The reason for implementing damage in the first place in sea ice models (Girard et al., Rampal et al., Dansereau et al., etc) is to weaken the ice early when ice deforms without the need to rely on a decrease in sea ice concentration that operate on longer a timescale, i.e. for divergent or convergent flow. The fact that sea ice concentration must be reduced even when convergence is present suggest that damage does not do its job (see Line 368). The authors appear to be missing a good opportunity to suggest a different functional dependence of Pmax on damage based on results from this assimilation procedure.

5- Figure 11: The colorbar must be defined. I suspect it shows the density of points for a given damage and eps_tot. More importantly, there is a dark yellow line (highest density of points) on the top right of the figure that shows a large number of undamaged ice ($\log(1\text{-d}) = 0$) for very large total deformation ($-1 < \log(\text{eps\_tot}) < 0$). These points are contrary to the argument presented in the paper (see Equ 5). In panel (b), these points are omitted (the near zero log(1-d) and log(eps_tot) ~= 0 are removed). This feature must be explained, not deleted from the panel.

6- Line 563-564: “Note, that unlike Eq. 19, the optimisation is performed here in the space of observations and using the observed total deformation.” This must be clarified. The optimisation is done in the space of reconstructed diagnostics (concentration and damage) from observed total deformation (see Figure 14), not “in the space of observations”.

Option A: Proof of concept that assimilation can improve LKFs forecast

Remove all statements from the paper that imply that the simulated LKFs shown are the result of the BBM rheology: any sea-ice model (with both compressive and shear strength) where we set the ice strength to zero along a line will deform along that line.

Option B: BBM is responsible for improved LKF forecast and correctly filling the gaps in sentinel coverage**.**

If the authors want to pursue that route, the following should be addressed

Line 407: “We expect that the model equipped with the mEVP rheology will not be capable of spatial extrapolation of the assimilated ice weakness (lowered A or enhanced d), and that further tuning of the BBM rheology can improve the practical predictability of LKFs”. This is an unsubstantiated statement, i.e., the statement of a belief rather than a statement supported by facts. This does not have its place in a scientific paper. The authors mention on line 405-406 that the mEVP is an available option in neXtSIM: “The BBM rheology can be further tuned and compared to the modified Elasto-Visco-Plastic rheology (mEVP, Bouillon et al., 2013) that is already an available option in neXtSIM (Olason et al., 2022) for estimating the impact of rheology on the sea ice predictability.” I propose that the authors run the same experiment using mEVP in order to support this statement. My own “belief” is that they will find that this is not the case. My “belief” is based on results from Ringeisen et al. (2019) showing how a standard VP rheology joins random weakness in a sea ice slab together into single LKFs feature even when the weaknesses are not oriented along lines where the maximum internal stress are present.

Minor Points:

1- Line 6: “We show that high values of ice deformation can be interpreted as reduced ice concentration and increased ice damage - scalar variables of neXtSIM”. It can be interpreted/simulated this way but this is not the case in reality and this may be hiding a defect in the formulation of the damage parameterization. See major point above.

2- Line 45-47: “In a recent model intercomparison paper (Bouchat et al., 2021) neXtSIM results ranked among the best for simulating the observed probability distribution, spatial distribution and fractal properties of sea ice deformation, even though it operates on a low resolution grid of 10 km.” The spatial resolution is lower, but the effective spatial resolution is higher given that a finite element model can resolve discontinuities at the model grid scale, contrary to a finite difference model where 7-8 grid points are needed to resolve a discontinuity.

3- Equation 5: Are the results sensitive to the choice of exponent in the Pmax equation? I.e. power 3/2 as opposed to linear.
4- Line 196: This sentence should read: “Only the older [sea] ice [concentration] is updated in the assimilation…”. The word “concentration” is missing.
5- Line 234-235: “The effect of assimilation on the prediction skill is evaluated by comparison of the simulated and observed total deformation fields as it is crucial information for safe navigation, ecological and climate studies.” Ecological and climate studies have much longer time scale than the 2-days improved predictability from an assimilation approach. Only safe navigation should be kept as a motivation in this sentence.
6- Line 244: The Kolmogorov-Smirnov Distance is the difference between cumulative distribution functions (CDFs) not PDFs. This should be corrected. It is however correct that the KS distance can be used to compare two distributions and assess whether two PDFs were drawn from the same underlying probability distribution.
7- Line 371: The free run was not defined.
8- Line 378: “In the second element (orange lines on Fig. 10) the initial deformation at the break-up event is larger, the concentration decreases rapidly and, as a result, the deformation on later steps reaches much higher values.” Define “rapidly”. A lead opening over a 6-day time scales does not seem particularly rapid.
9- Bouchat et al. reference. Update this reference from the ESSOAR to the published version.
10- Line 561-562: “However, in Eq. 21, concentration is a function of ε_{tot} assuming that ice breaks and becomes weaker due to both convergence and shear.” But in observations ice become weaker when it deforms whether there is a decrease of concentration (divergence) or not (convergence).