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

In this paper, damage and concentration – assumed to be a function of satellite-derived total sea ice deformation – are assimilated using a simple nudging method in the Brittle-Bingham-Maxwell (BBM) model – an off-spring of the EB/MEB model – for short-term sea ice forecast of Linear Kinematic Features. Results show that assimilation of sea ice concentration improves the skill of a short-term forecast for up to five days lead-time in comparison with persistence or a free simulation. The assimilation of damage on the other hand does not improve the skill of the forecast. It is argued that that the reason for the lack of sensitivity to damage assimilation is because the healing of ice (that reduces the damage) has a timescale that is too fast (~24 hrs) compared with the desired lead time of the forecast (O(days)).

The paper is well written. The organisation and depth of the paper however is lacking. The text refers to a non-existent appendix on several occasions; the sensitivity to assimilation parameters consists of four 1 or 2-line paragraphs; there are multiple unsubstantiated statements; the results section presents inconsistent results (sea ice concentration larger than one) and focuses on small scale features in order to justify broad conclusions. The authors state that neXtSIM is not ready to assimilate deformation yet and that the paper is a proof-of-concept. There is no problem with this approach, but the authors have pushed this paper out for review much too early. I recommend that the paper be rejected for the moment and that authors be encouraged to resubmit later, a substantially different version of the paper that addresses (not rebut) the comments below. I am recommending a "reject with encouragement to resubmit" simply to give the author time to properly address the comments.

**Overstatements**:

1- Line 44: "In a recent model intercomparison paper (Bouchat et al., 2021), only one model, neXtSIM (neXt Generation Sea Ice Model, Bouillon and Rampal, 2015a; Rampal 45 et al., 2016), proved to be capable when run at the same spatial resolution as the available observations (i.e. ∼ 10km) to simulate the observed probability distribution, spatial distribution and fractal properties of sea ice deformation." This is incorrect. One 10-km McGill run was able to reproduce the PDF of divergence and multiple 10-km models were able to reproduce temporal scaling and spatio-temporal coupling. The only place where neXtSIM did perform better compared to other 10-km models is the spatial scaling.
2- Line 304: "The viscous-plastic (VP) rheology used in MITgcm is known to have a less realistic slower time evolution of LKFs (Hutter et al., 2018) than the BBM rheology in neXtSIM (Olason et al., 2022). As a result, the sea ice simulated by the BBM rheology has more rapid error growth (loses skill faster) due to the correctly resolved intermittent ice motion and localised ice deformation." If the intermittent motion is correctly resolved, why is the error growing faster? Intermittency does not come from the brittle parameterization in the EB/MEB/BBM. All models participating in SIREX (VP, EVP,

EB) show intermittency (Bouchat et al., 2021). The source of intermittency in observed deformation is still an unresolved issue. The authors must clarify what they mean by "intermittency. If the temporal scaling exponent is used to discuss the LKFs intermittency, this is incorrect. If instead the authors are referring to LKF growth rate and lifetime, this is also incorrect. In SIREX2, it is shown that no apparent link is present between the LKF growth rates, lifetimes and the temporal scaling/multi-fractal parameters. The intermittency is revealed by the quadratic nature of the structure function.

3- Line 209: "Thus, due to its rheology, neXtSIM is able to extrapolate and create realistic connections between the observed and assimilated pieces of LKFs." This is not demonstrated in the manuscript. I believe any rheology that assimilates sea ice contration (A) will show LKFs in line with observations. See Major Points below.

Major Points:

1- On two occasions, the authors are referring to an Appendix that is not included in the paper. The appendix must be included.
2- Equation 7-10: Sea ice concentration (A) increases in convergence (until A=1) and decreases in divergence; sea ice concentration can also increase or decrease in shear. A single dependency of A on eps_tot is therefore missing events where convergence is present along LKFs. The damage (d) dependency on eps_tot is more realistic, but the authors argue that assimilation of A is useful to increase predictive skill, whereas d is not. See below for more on this topic. The single dependence of A on (eps_tot) must be justified.
3- Line 145: The value of a1 in F_A(eps_tot) is found through sensitivity experiments using the same BBM model. This is a circular argument. This functional dependence must be derived from sea ice concentration and total deformation derived from passive microwave and Sentinel. I believe the author will find that the functional dependency is not a simple linear relationship. The author must at least show this relationship from observations and acknowledge the simplicity/caveat of the approach.
4- Fig 5a: The skill of the model is assessed using the fraction of points where the correlation between observed and simulated deformation is significant. Statistically speaking, there will always be some points that will remain significantly correlated. The statistical significance of the signal must be shown in the figure. I also see no spatial structure in the regions of high correlations which suggest that the high-correlation points are just random events.
5- Fig 5b: I would have expected that the root mean square difference of the forecast run would asymptote to the free run. The fact that it does not is suspicious. This must be explained.
6- The discussion of the sensitivity of the forecast on epsilon_min and the weighting factor w_d appears in two one-line paragraph. I suggest removing them, or a more in-depth discussion should be provided.
7- Figure 6. The constant a1 is negative. This means that the sea ice concentration is larger than 1 (see Equ 10 and since eps_tot > 0). This is not physical. The results in the figure cannot be correct.

8- I am assuming that the sign of a1 is incorrect. If so, A = F_A(eps_tot) = 1 – a1 * eps_tot ~=0.76 for a1 ~= -1.2 (Line 232) and eps_tot ~= 0.2 (Fig 3). For A=0.76, P* is scaled down two orders of magnitude and the ice has no strength. Any rheological model where the ice strength is set to nearly zero along a line (an observed LKF in this case) will deform along that line – this can be tested simply. The author instead argue that the brittle rheology is key to the correct simulated location of the LKFs. This is another unsubstantiated statement.

9- Results: The simulated concentration and thickness fields after assimilation should be presented. Reading from the deformation fields and the a1 constant derived from sensitivity experiments, we should see concentration of ~0.8 along LKFs, something that is not accord with RGPS observations at 10km scale resolution. I suspect assimilating damage would help producing more realistic fields. The authors give reasons for why damage assimilation is not successful, but those are not convincing. See below for more details on this topic.

10- It is argued that the damage does not increase predictive skill of LKFs because ice heals too rapidly (~24 hours). In the real world, ice heals through thermodynamic processes on much longer time scale. This choice of short healing time scale must be justified. Perhaps this is the cause of the lack of predictive associated with the assimilation of the damage.

11- The error is shown as ¼ sigma. This is highly unusual. Typically, one would show an error envelope equal to one sigma (four times larger than what is shown in Figure 5).

Minor Points:

1- Line 23: “Under external forcing the ice deforms primarily as an elastic material.” Most deformations in the pack ice are plastic and occurs along LKFs. This sentence also contradicts the next sentence.

2- Line 26: “…start deforming along multiple narrow and elongated cracks formed and does so until these later refreeze”. Or when the load (winds) on the ice changes. This should be added.

3- Line 48: :”… the exact timing and position of strong deformation zones, or LKFs, is not yet predicted precisely”. The exact position of LKFs will never be located precisely because it depends on unresolved weaknesses within the ice pack. What we can hope to reproduce is the timing, the orientation of the LKFs with respect to the large-scale forcing and their statistical distribution (width, length, density, angle of fracture, etc.). This should be corrected. This is another sentence that suggests incorrectly that BBM could eventually simulate LKFs position correctly.

4- Line 93: “The observed variables vo (damage and concentration) is computed…”. “Damage” is not observed. The word “is” should read “are”.

5- Line 94: Sometimes, the Greek lowercase epsilon symbol is used and sometimes the lunate epsilon symbols is used. The author needs to choose only one form for consistency. See Line 94 and Equations 6-7 for examples. This needs to be corrected everywhere.

6- Line 102: It is said that CNEMS has a temporal resolution of 12 hours on Line 102; and it is said in the next sentence that it is “observed nearly every day”. This sounds contradictory. This should be clarified. See Line 115 as well where 24 hours is specified.

7- Line 105: “The model is forced with the European Center for Medium-Range Weather Forecasts (ECMWF)”. The version of ERA should be specified: e.g. ERA5, ERA-interim, etc.
8- Line 117: “total” should read “total deformation”.
9- Line 203: The units of eps_tot must be given the first time it is introduced (Table 1, Line 197). At present it is only given on Line 203.
10- Line 235: eps_tot and divergence is used interchangeably; yet they are very different. One includes shear the other not.

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