

Answer to Anonymous Referee #2

Dear referee,

First of all, we would like to thank you for your in-depth review, insightful comments and suggestions, which greatly helped us improving our manuscript.

Please note that a version of our manuscript containing all the changes we made is attached as a supplementary material.

The **comments of the reviewer** are in **bold**, our **answer** in **red** and the **added text** in **bold red**.

General comments:

The authors do a good job of describing previous sea-ice modeling efforts but they should spend more time criticizing these efforts and describe in more detail how their model corrects the deficiencies of previous models. Also they should compare their new rheology to the EAP and Elastic-Decohesive rheologies. How is the rheology described here superior to these other attempt to correct previous model deficiencies?

We restructured the introduction entirely in order to make the context, and the main goals of our study clearer (based partly on the comments of referee #1) and which we think will partly address your comment.

Note in particular the following change at the end of the introduction paragraph:

“As an example of this complexity, recent studies showed that the statistical properties of sea ice deformation are characterised by a coupled space-time scaling invariance (Marsan2004, Rampal2008), similar to what is observed for earth quakes (Kagan1991a, Kagan1991b, Marsan2010), and which is a fingerprint of the presence of long-range elastic interactions within the ice cover.”

In this new introduction, note also that we cite Schreyer et al., 2006 and Tsamados et al., 2013 (presenting the Elastic-decohesive and EAP rheologies, respectively) and say:

“These developments still need to be further evaluated regarding their contribution to better reproduce the complexity of sea ice dynamics mentioned earlier, in realistic setups”

Clarify whether this model has an ice thickness distribution and if it does how this is varied with time.

The model does not have an ice thickness distribution, it has only two ice classes, ice and open water, but this is not stated in the paper. Thank you for pointing this out. We have now added a single sentence to the model description section saying:

The model has two ice thickness categories: ice and open water.

I may have missed it but model resolution does not seem to be mentioned in this study. While it's hard to describe the resolution given the mesh moves some scale metric of the mesh should be given. $\sqrt{\text{mean cell area}}$ maybe?

Yes, the resolution was not stated. We now state the resolution of the set-up we use for the model evaluation in the second paragraph of section 3.1. We have been using the square root of the mean cell area internally, and agree that this is a suitable metric.

Changes:

The resolution of the finite element mesh is about 11 km, where the resolution is defined as the square root of the mean element area.

The conclusions talk about this model being useful as a general sea ice model. This model cannot realistically be included into a GCM without it being parallelized and having good performance. It's unclear if this model is parallelized at all or how the performance compares to other models. If those remarks are going to be made in the conclusion the authors should state whether the model is parallelized, if it will be and what its performance is. I imagine the changing grid could prove problematic for parallelization.

The wording in the conclusions is admittedly a bit too general. The point is that we can use the model to simulate most aspects of sea-ice behavior on a seasonal time scale (as demonstrated in the paper). The model has not yet been parallelized or optimized for a good performance (this is ongoing work) so we cannot include it in a GCM at this point – even though that is a future goal. At the moment the model is mostly written in Matlab, with the most time consuming routines written in C. This is very

limiting w.r.t. performance and parallelization, so we are working on a parallelized C++ port. We don't believe that this information belongs in the paper, since it should focus on the physics of the model and the scientific results. We expect that we will publish another paper on the C++ port focusing on the parallelization and performance, at a later date. For your information a one-year run of the model takes about three to four days on a high-end desktop computer.

We have changed the last paragraph of the paper in response to this comment and a similar one raised by reviewer #1

Changes:

In conclusion, for scales smaller than a year, neXtSIM performs very well with respect to several important metrics related to sea ice dynamics and thermodynamics. We believe that in its current stage of development neXtSIM may already be a useful tool for both the scientific and engineering communities. For longer time scales and to study the interactions between sea ice and the ocean, ecosystems, or the atmosphere, more developments are needed, especially on the coupling with other components and the use of a more advanced sea ice thermodynamics model.

Specific comments:

pg: 5888, line 7: Since the authors list a series of vertical thermodynamic models, they should list the two most state of the art ones: Vancoppenolle and Turner.

The introduction has been substantially rewritten in accordance with the wishes of referee #1. A reference to Vancoppenolle and Turner is now included:

Virtually all modern sea-ice models use either the VP or EVP formulation, combined with a thermodynamics model (e.g. Semtner, 1976; Bitz and Lipscomb, 1999; Vancoppenolle et al., 2009; Turner and Hunke, 2015) ...

Equation 5: The authors should explain the physical meaning behind this equation rather than just introduce without explanation.

A similar remark was also raised by reviewer #1. Note that we want to keep this description short as it is the same as in Bouillon and Rampal (2015b).

We now clearly state that: “The evolution of the internal stress is computed as in \cite{Bouillon2015b}” where the reader can find a detailed explanation of the dynamical core of the model. We also add for Equation 5, which is the first step of the internal stress evolution equation: “The first step accounts for the elastic deformation without considering the damaging process...”

Equation 7: Again some explanation of the origin/meaning of this equation is needed.

Same answer as for the previous remark.

We now say: “The damage source term Δd corresponding to the decrease of σ has been derived in Bouillon and Rampal (2015b) as ...”

Equation 9: Some explanation of the origin/meaning of this equation is needed.

Same answer as for the previous remark. Note that we split this equation in two. One equation for the general definition of the effective elastic stiffness (NOW EQUATION 6) and one for the effect of the concentration (NOW EQUATION 17).

We now say: “The effect of the concentration on the mechanical response of sea ice is here parameterised by a decreasing exponential function of the concentration: $f(A) = e^{-\alpha(1-A)}$, (17) where α is the compactness parameter (see Bouillon and Rampal, 2015b, for more details).”

pg: 5896, line 1: "i.e. increase ice conc." Wouldn't new ice formation lead to thicker ice as well or is this referring only to frazil ice formed in open water - clarify.

Here we are referring to ice formation in open water and leads. The sentence now reads

Changes:

... via two processes: **formation of new ice in open water and leads** and thermodynamical healing.

pg: 5896, line 9-11: What is the justification for relaxation time proportional to ΔT ? What other options are there?

The reason for a relationship with temperature is that we expect the ice to heal faster when it's cold, since refreezing of cracks and leads is faster then. We also expect the ice not to heal when it is melting, so the healing should be capped at zero for temperatures above the freezing point. We believe the relationship we propose is the simplest approach to get approximately the right behavior. More

research is required to determine how suitable this approximation is and whether other, more advanced alternatives give better results.

Changes:

... at a constant rate. **This is based on the fact that the cooler the environment the faster the ice will freeze, so presumably low temperatures result in fast healing and warm temperatures in slower healing, with no healing occurring for temperatures over the freezing point.** The damage relaxation term ...

pg: 5897, line 12: Explain why it is not possible to derive an equation for A from first principles. The change in A is essentially a subgrid-scale parametrisation. All the thermodynamic routines can give is the volume of ice created, based on the amount of heat lost by the ocean to the atmosphere. This volume then needs to be given a thickness and concentration, which can only be done in an empirical manner. Having said this, the wording of that sentence can be improved and we propose to replace the sentence starting at line 12.

Changes:

The change in A is calculated by assuming a given thickness for the ice forming over open water (Δh_{ow}). We use a constant, h_0 for this thickness, giving a source/sink term for A as

pg: 5898, line 2: Is the thermal conductivity and specific heat capacity the same as Semtner 1976?

The zero layer model has no heat capacity, but the thermal conductivity is the same. We have appended the following to the sentence starting on line 1

Changes:

... the zero layer model of Semtner (1976), , using the same parameter values, unless otherwise stated.

pg: 5899, line 5: What salinity is assumed for the ice? Does it vary? What vertical profile?

Given the simplicity of the zero layer model we simply assume constant salinity of 5 psu.

Changes:

The following has been appended to the paragraph starting on line one of page 5899:

We assume a constant ice salinity of $S_i = 5$ psu.

pg: 5899, line 16: Explain more fully what this means. I didn't understand this point.

This paragraph is hard to understand because the remeshing procedure hasn't been introduced. We have moved it towards the end of section 2.4 and edited it to read:

Change: The approach used for the slab-ocean is similar to that used for the forcing in that the fields are only interpolated after remeshing. The slab-ocean model resides on the same mesh as the ice model, but the relative displacement of ice and ocean is ignored between remeshing steps. When the model mesh becomes too deformed and therefore needs to be remeshed, the temperature and salinity are interpolated from the old onto the new mesh using a linear interpolation and ignoring the displacement of the old mesh. This ensures that the temperature and salinity fields do not drift with the ice as the ice-model mesh moves.

pg: 5899, line 25: Surely a discrete element sea ice model would be Lagrangian and not require an unstructured mesh?

That is correct. We were referring to continuous, purely Lagrangian schemes. Such schemes will always require remeshing since the mesh deforms as the nodes don't all move with the same velocity.

Change:

Continuous, purely Lagrangian schemes ...

pg: 5901, line 1-2: Explain more fully what the affect on the simulated results is of this assumption.

The effect is negligible. This is, because when a single element in the mesh becomes too deformed a new mesh is generated and the forcing must be re-interpolated. It is therefore only in a very limited area where the ice moves a sizable fraction of the grid cell size before re-interpolation, incurring a very small error there and even less error elsewhere.

Change: We've rewritten the relevant sentences to read: We checked that this method gives **virtually identical** results as when we interpolate the forcing fields every time step. Indeed as the remeshing criterion is global the error in the position of the forcing field is **in practice** never larger than a single model element. **Given the high resolution of the model grid in our tests, the forcing fields are too**

smooth and too coarsely resolved for this error to have any substantial effect.

pg: 5903, line: 4: "Progressively" - progressive in what way?

The wording is unclear here. What happens is that the wind and ocean currents are increased linearly from zero to their proper values over the period of one day, when the model is first started. If the wind and ocean currents are set to full strength at the first time step the ice becomes completely damaged everywhere. This is the same procedure as used to spin up ocean models (except they need a longer initial spin-up time).

Change:

We replace the relevant sentence with the following: **In order to prevent an initial shock to the system when the model is started the strength of applied wind and ocean currents is increased linearly from zero to full strength over the period of one day.**

pg: 5906, line 20-22: The plots seem to suggest that the an infinite healing time is not significantly better than the chosen one. This seems to imply that healing isn't really needed for good model results. Please comment much more fully on this important point!

In the model the healing of the ice is represented as a combination of two terms. The first term is based on the assumption that newly created ice has no damage, meaning that the damage decreases when the ice volume increases due to thermodynamics. The second term is meant to represent the refreezing of the fractures. This one is not related to the volume of created ice but to a healing time parameter. In our experiments the first term is always active as we focus on winter season. The question of the reviewer is related to the second term for which a discussion about its utility is needed. From our sensitivity analyses we see that the results with a healing timescale larger than 14 days (from 14 to 1000 days) are all very similar. We found that for the simulations with a healing timescale larger than 14 days, the spatial scaling of the deformation (not shown), the temporal scaling of the deformation (intermittency) and the comparison to observed drift are all similar. In conclusion, we cannot exclude that the second healing term may not be necessary. However, we prefer keeping this term in the description of the model for further sensitivity test in other configurations.

Changes:

We add these sentences to the end of the discussion on the sensitivity to the healing time scale: **"The low sensitivity to the healing time parameter for values larger than 14 days may indicate that this additional healing term is not needed and that the healing due to new ice formation is sufficient. However as this may not be true for all model configurations we prefer keeping this term in the description of the model."**

pg: 5908, line: 10-11: "the internal stress should also be correct". Its unclear what this means or why it should be true

This sentence is badly formulated. The point is that the leading terms in the momentum equation are the atmospheric and oceanic stresses and the internal stress. Seeing that we have calibrated the atmospheric and oceanic drag in a free-drift setup we can say that an evaluation of the ice drift and deformation amounts to an evaluation of the internal stress term.

Change:

... cannot be directly used for a complete evaluation. However, since we have calibrated the two other dominant terms of the momentum equation (i.e. the oceanic and atmospheric drag terms) then we can use an evaluation of the over all drift and deformation of the ice an evaluation of the internal tress. A good way to evaluate the new rheology is then ...

pg: 5908, line: 16: not sure what "point-to-point" means in this context

What we mean here is that it makes no sense to try to compare the location of the modeled deformation features with the observed ones. The strong localization means that it is virtually impossible to simulate the location of the deformation correctly. We can, however, compare the statistics of the observed and simulated deformation and this should give a very good idea of whether the model is doing a good job at reproducing the ice physics or not.

Change:

... and in time. This makes a comparison of the geographical location of the observed and simulated deformation features impractical, since small errors in the applied forcing are bound to result in significant changes in the simulated location, compared to the extent of the features. Instead we compare ...

pg: 5908: line 19-21: Explain this point more fully.

Sorry, but we realized that this sentence was not meaningful, we then decided to remove it.

pg: 5909: line: 12: Explain more fully why the snapshots were discarded.

We are not sure to understand what the referee wants us to say in addition to what is already in the text. When calculating the fit on the data corresponding to these 4 snapshots using a power law model, the fit was not significant in the least squared sense. This is why we discarded these snapshots.

Change:

We change the old sentence with this one: **Four snapshots had to be discarded because the power law model fit was not significant in the least squared sense due to the excessive noise in the observed deformation fields.**

Technical comments:

pg: 5889, line: 28-29: "days or weeks" -> "days to weeks"; "seconds or hours" -> "seconds to hours"

The introduction has been substantially rewritten in accordance with the wishes of reviewer #1. As a result these phrases are no longer used in the paper.

pg: 5894, line 1: "is represented by" -> "represents"

Fixed

pg: 5896, line 9: "supposed" -> "assumed"

Fixed

pg: 5900, line: 1: "It also allows" -> "They also allow"

Fixed

pg: 5900, line: 18: "every hour" -> "every model hour"

Fixed

pg: 5901, line 4: "mesher": This is not a real word

Changed to "mesh generator"

pg: 5901, line 11: ",...)" Not sure what this is meant to represent. Remove?

Change: Replaced " , ... " with "etc."

pg: 5907, line: 11: "X over Y" -> "ratio of X to Y"

Fixed. The sentence now reads: "In order to assess this effect Fig. 8 shows the ratio of drift speed to wind speed for the reference run, a model run forced with ERA-Interim, and the ratio of the climatology of IABP buoy drift speed to ERA-Interim wind speed climatology."

pg: 5907, line: 25: "much likely the mark of" -> "is likely to be caused by"

Fixed

pg 5910: line 3: "vertices" -> "vertex"

Fixed

pg 5910: line 19-21: explain this point more fully.

Indeed, this sentence was not very clear.

Change:

We changed the sentence with this one: **We note that the order of magnitude of the healing time scale obtained here is consistent with the optimal value obtained in the previous section when analysing the sensitivity of the model with respect to sea ice drift.**

pg 5912: line 12: "forwards" -> "forward"

This paragraph has been removed in accordance with the wishes of reviewer #1

pg 5913: line: 11: "therefore" -> ", therefore, "

Fixed

pg 5914, line 1-3: However is being used to link two sentences. Break into two sentences.

pg 5914, line 5: "it is reassuring ...in the extent". This sentence is not correct english.

Both issues have been fixed. The text now reads: This behaviour is largely controlled by the atmospheric and oceanic forcing. **However**, a poorly tuned or conceived ice model is still free to diverge considerably from the observed state, **and** it is reassuring to see that this is not the case here. The only genuine discrepancy ...

Figure 10: Make the y axis start at 0.

Done.

Figure 14: Describe the grey boxes in the caption

Fixed. The last sentence of the caption now reads: **Error bars and grey shading indicate observational uncertainties** (for further details see text).

Figure 16: "Ice thickness per unit area": What is that?

It should have said only ice thickness. **Changed to "Ice thickness"**.