

## Answer to Anonymous Referee #1

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**.

**It is an excellent paper, with stunning results on a new generation sea ice model.**

**We thank the reviewer for this nice and encouraging remark.**

**I have three main comments**

**1) there are lots of parasite words in the text. Either because of Frenglish use. Or because the authors abuse of "commercial" pieces of text such as "for the first time", etc. Remind that this is not a proposal and that the paper is meant to be read for long long times. I would argue that the paper would gain by being more sober in general.**

**We have carefully cleaned the text and removed too-"commercial" syntax.**

**2) The intro could be better balanced as well (see detailed comments)**

**We have restructured the introduction substantially to address this and the detailed comments.**

**3) I also have one very stupid question. Healing seems ok if the time scale is long enough, beyond which the model results do not seem to be affected anymore by the healing time scale. "Is healing useful in the end", is an obvious question that comes at the end of the reading. I know it is useful, but a good improvement of the paper would be to show or explain what happens if healing is too slow, or if there is no healing at all. Apart from these few remarks, the scientific changes that I propose are cosmetic.**

**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."**

### **1 Specific comments**

#### **1.1 Intro**

**• Arctic's. Check if ok: the sea ice cover does not belong to the Arctic.**

Corrected

- treacherous: first time I read this word.

We think this term is appropriate. It is often used in the context of exploration (see for example <https://www.youtube.com/watch?v=q0xDUAV3x4Q>). We have, however, removed it as a part of a general revision of the introduction

- page 5887, lines 5-9. Your wording is loose here. I would simplify the sentence. Because 1) Speaking of "the role of Arctic and Antarctic climate in the climate system" sounds bizarre, I would rather speak of the role of sea ice in the climate system; 2) Polar amplification of climate change is not only due to sea ice, and the primary drivers are atmospheric. Check e.g. Pithan and Mauritsen, NG2014. Maybe speak of the ice-albedo effect, or of the impact of sea ice growth and melt on ocean circulation ?

Yes, we agree. Moreover then we have removed this paragraph as a part of a general revision of the introduction.

- page 5887. L 9-25. "Earth" may need to be capitalized. Two times "in particular".  
corrected

I did not particularly fancy this paragraph. I think the justification that VP-like models are not in agreement with RGPS observations is sufficient and better than what is written now. Using climate models to justify the EB approach is not the best manner to motivate its development, in my view, as we rather suspect the sea ice mean state, the atmospheric and oceanic forcings in climate simulations to be responsible for sea ice model misbehaviour.

Yes, you are right, this is not the best way to motivate the development.

**Changes:**

We removed this paragraph and added a discussion about models vs RGPS below, as per the following comment. This then acts as the motivation for the model development.

- The review on sea ice models is too long and the choice of references is debatable. I would shorten in one paragraph. I would cite Coon, first. Then explain that existing large-scale sea ice models (CICE, LIM, MITgcm, MPI) virtually all base on Coon's approach. Describe VP, EVP in Eulerian framework. Then cite the papers that show that these models were found inadequate wrt RGPS observations. It is enough to justify your study. You don't need to write the full history of sea ice models.

Yes, it is too long and goes into unnecessary details, especially concerning the thermodynamics, which are of minor importance here.

**Changes:**

We have condensed the review to a single paragraph, which is now followed by a short paragraph discussing models vs RGPS.

- page 5887-5888. last paragraph of 5887 continuing on 5888. I would remove or rewrite this paragraph. Your choice of references seems either old-fashioned or bizarre. I like a lot Ukita and Martison and Huwald's papers, but these models are not used in coupled modelling studies. Recent research focused on adding ice salinity to thermodynamics (Vancoppenolle et al, Turner et al, Griewank and Notz, Rees Jones and Worster, : : :).

We have removed this paragraph, since it goes into unnecessary details, especially concerning the thermodynamics, which are of minor importance here.

**Changes:**

We have removed the paragraph in question

- following paragraphs. You speak a lot of large-scale models, but you don't cite

any reference where these can be found.

These have been merged with other paragraphs or removed. However, we do now give references to papers and manuals for the CICE, LIM3, MITgcm, and MPI-ESM models.

**Changes:**

We have shortened and removed much of this discussion and added the following:

**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) and variously detailed sub-grid scale parametrisations (for commonly used large scale models see for instance Hunke et al., 2015; Vancoppenolle et al., 2008; Adcroft et al., 2004; Notz et al., 2013).**

• page 5889. Your statement that ice thickness distribution is needed for low resolution only is not backed by any reference. I think your statement is neither proven nor a priori true, since such formulations are meant to capture 1m-scale variations in ice thickness. ITD schemes enhance ice growth (growth-thickness feedback) and melt (ice-albedo feedback). How much they bring is detailed in e.g. Holland et al (JCLim 2006).

Yes, you are right it is not proven.

**Changes:**

We removed the statement on the low resolution models. We also added the proposed reference.

• page 5889 Lines 12 and following. Ice dynamics is plural.

Yes.

**Corrected in 3 places in the text**

**Is formation of new ice really slow (count the time is required to form 10cm of new ice at -10\_C)**

You are right, the formation of thin ice is not slow and the sentence was misleading. We realized that this discussion on the healing versus damaging timescales is actually not needed and may have hidden the main point of this paragraph, which is to explain that having 2 competing processes (healing and damaging) may be the cause of many complex characteristics of sea ice dynamics. We decided to remove the references to the timescales to make this paragraph clearer.

**Changes:**

**We remove the terms “quick” and “slow” of the sentence to just keep : “In addition to the damaging processes the formation of new ice is also important.” We also remove the sentence: “However, one should note that this recovery is much slower (i.e. several days or weeks) than the damaging process (i.e. several seconds or hours).”, which is not needed. We added a reference to an example where the competition between healing and damaging processes generates complex behavior, here the scale invariance of the floe size distribution.**

• page 5890. Remove all "for the first time", "crucial", : : : Not that I contest, but your paper is convincing enough without them. They could have irritated me in a bad day.

Corrected

## 1.2 Model description

• As I understand, damage is an extensive variable. What do you do when new ice is added at the base and in open water?.

Yes, you are right. The formation of new ice impacts the damage variable. We take that process into account and that what explained on lines 18-20: “The damage is an ice volume tracer and is equal to 0 for newly formed ice. When new ice is formed, the new damage is calculated as a volume weighted average over the old and new ice.”

**Changes:**

In order to be more clear, we replaced that former sentence with this one: “When new ice is formed, the new damage is decreased and calculated as a volume weighted average over the old and new ice, meaning that the sea ice partly recovers its mechanical strength.”

See also answer to remark on page 5897

- page 5892. first line, is "P" or "grad P" the pressure term ?

P is a vertically integrated pressure term. Grad P is the gradient of this pressure term.

**Changes:**

We added the following sentences to the paragraph describing the pressure term in order to clarify its definition and its role:

\$P\$ is a vertically integrated sea ice pressure term that is set to avoid excessive convergence when the ice concentration in a cell is at 100\% and at the same time highly damaged.

Without this term, one obtains unrealistically large local thickness of the ice cover, for example north of Greenland and the Canadian Archipelago.”

- equation 5. I think the reader would benefit if you told that this is simply the representation of an elastic deformation.

Yes, thank you for the suggestion.

**Changes:** Two sentences have been adapted two make the paragraph clearer.

“The first step accounts for the elastic deformation without considering the damaging process and gives a first estimate of the internal stress,  $\sigma^{\prime}$ , by”

“The second step accounts for the damaging process.”

- equation 11. Could you plot that relation schematically, if not done anywhere else ?

This representation of the failure envelope is the same as the one presented in Weiss et al. (2007, <http://www.sciencedirect.com/science/article/pii/S0012821X06008430>) .

**Changes:** We add a reference to this paper.

- page 5894. line 17. "led" -> "lead"

Of course.

**Corrected**

- page 5894. I was curious to know why you still need a pressure term to prevent unrealstic local thickness accumulation, whereas your model is elastic. Hence, the sea ice, if thick enough, should in principle be able to oppose to compressive deformation. Could you remove the pressure term if the elasticity depended in  $h_2$  ? Did you make tests on this ?

In our model, the effective elasticity depends on the damage. If the damage is close to its maximum value ( $d=1$ ), then the effective elasticity is close to zero. It corresponds to a situation where the ice is highly broken and cannot build up elastic stress. However, even in this condition, sea ice should not converge with no resistance, this is why a pressure term is added. This term implies no memory effect, meaning that it cannot be included in the evolution equation of the internal stress. When running with no pressure term, the sea ice tends to accumulate north of the Canadian Artic Archipelago and Greenland. This accumulation is not realistic compared to observation (ICESAT) and also impacts the mean drift north of Greenland. The pressure term has been designed to cure this problem. We ran simulations with a pressure term having a linear and quadratic dependence in  $h$  and with several values for the pressure parameter  $P^*$ . These simulations were not done with exactly the same setup as the one presented in the paper. We ran the simulations from mid-September 2007 to April 2008 and compared the simulated sea ice thickness field to the ICESAT campaigns of October-November 2007 and February-March 2008. We also computed the mean drift in a similar way as what is presented in the paper and compared to the mean field

build from RGPS and GlobICE datasets. We found out that the values  $P^*=12\text{kPa}$  and the quadratic relationship were well suited to avoid excessive accumulation along the CAA and Greenland coasts and allowed the model to simulate very well the mean drift (as shown in the paper). These simulations are not presented in the paper because the setup is not the same as the one used for the other analysis.

**Changes:**

We add this sentence to explain why this term cannot be integrated in the evolution equation of the internal stress.

“This term implies no memory effect, meaning that it cannot be included in the evolution equation of the internal stress.”

The discussion on the sensitivity to the pressure term is not added to the paper because it was not done with the same setup.

• page 5896, first line. are you sure "neg. feedback" is a proper wording to describe the role of healing on deformation?

Of course, this term is not appropriate.

**Change:** We just use “impact” instead of “have a negative feedback on”.

• page 5897. Do you change damage if concentration decreases ?

No, we don't.

**Changes:** It is now specified and we also adapted the paragraph to make it clearer:

“In neXtSIM damaged sea ice recovers its mechanical strength (i.e., decrease of the damage) through time via two processes: new ice formation and thermodynamical healing. Sea ice melting is supposed to have no direct impact on the damage. New ice formation is naturally treated by updating the value of the local damage as a volume weighted average over the old and new ice. When sea ice volume in a cell increases due to ice formation, the damage then automatically decreases as new ice is supposed to have a damage equal to zero. For the thermodynamical healing process, more assumptions need to be done. We here assume that the thermodynamical healing process is driven by the local temperature gradient between the bottom of the ice and the snow-ice interfaces and decreases the effective compliance, defined as  $C=\frac{1}{1-d}$ , at a constant rate.”

• Equation 24, 25, and 26 are not consistent in terms of units.  $Sh$  in is m/s,  $SA$  is in  $s^{-1}$ ,  $Shs$  is in m/s : :

Of course. That was a typo in the text, but there is no such mistake in the code.

**Changes:**

We corrected the equations 24, 25 and 26. We also modified equation 20 so that they all look the same.

### 1.3 Other sections

• page 5902. Why do you use different bathymetries for TOPAZ4 and the basal stress term ?

There is no specific reason. We could have chosen GEBCO for the basal stress. The two bathymetries are very similar and seem to be very good

([http://www.gebco.net/about\\_us/gebco\\_science\\_day/documents/gebco\\_fifth\\_science\\_day\\_abramova2.pdf](http://www.gebco.net/about_us/gebco_science_day/documents/gebco_fifth_science_day_abramova2.pdf)).

**No change**

page 5910. What is  $D$  ?

A proxy of the deformation. That was not clearly specified in the text.

**Changes:** We modified the paragraph to make it clearer.

“For each pair of vertices/RGPS points initially separated by a distance  $L$  of  $\sim 30\text{ km}$  on average, a proxy of sea ice deformation  $D$  is measured by looking at the relative variation  $\Delta L/L$  of the distance between the two

vertices/RGPS points for different time intervals  $\Delta t$ . The deformation rate  $\dot{D}$  is estimated as  $\dot{D} = \frac{\Delta L}{\Delta t}$ .”

Why capitalizing Summer and Winter ?

**Corrected**

"one 3 days period to the other" is bizzare wording.

Yes, you are right.

**Changes: This term was not needed and has been removed.**

• **Section 3.4** The section on ice albedo is without interest, I would remove. The albedo param you use is super simple. And the conclusions are obvious and known for a long while.

Yes, you are right. The idea was to show that we have plenty of room to tune the model, but the discussion was too long.

**Changes:**

We have removed the paragraph describing the albedo parametrisatons and added the following to the preceding paragraph:

**For this purpose results from three runs, in addition to the reference run are shown: A run with fixed albedos of  $\alpha_i = 0.7$  and  $\alpha_s = 0.9$  (high albedo case), a run with temperature dependent albedos (Hunke et al., 2015), and a run forced with the ERA-Interim reanalysis results.**

We also modify the end of the following paragraph so that it now reads:

**With these caveats in mind we see that the performance of the reference run is acceptable when it comes to ice volume. The melt rate can also be substantially affected by tuning the albedos, as expected.**

• **Page 5912.** If you cite the study, there is no scientific reason for the pers. comm.

**Corrected**

• **page 5913, line 9.** dependant should be spelt dependent

**Corrected. We also changed “quadratic dependance” into “quadratic dependence”.**

• **"nextSIM performs very well wrt the most important metrics we can impose on sea ice model performance".** I agree, as long as you restrict this to scales smaller than a year. For longer time scales, you may find out that you need better thermodynamics

or thickness distribution. Read Semtner 1984 for example to find out that the 0-layer model induces a shift of 1 month of the seasonal cycle.

Yes, you are right.

**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.**

• **page 5915, line 22.** proof -> prove

**Corrected**

**Other changes:**

$\nabla \cdot \mathbf{u}$  is the divergence of the horizontal velocity