Answer to tc-2018-192-RC1 – Harry Heorton

February 14, 2019

Note:

- The referees comments are shown in black.
- **The authors answers are shown in bold typeface and colored in blue.**
- **The modifications brought to the manuscript are shown in bold typeface and colored in gray.**

Global comments

R1#1, Modelling Sea Ice fracture at very high resolution with VP rheologies This paper documents idealised very high resolution (25 m) numerical simulations of deformation in sea ice using the viscous plastic VP rheology. The sea ice is predominantly put under uniaxial compression from the top and bottom boundaries and the resulting deformation features are documented. Simulations are performed with longer model run times, decreased spatial resolution, modified boundary conditions, biaxial compression, imposed flaws within the sea ice and alternate rheologies by changing the yield curve shape. The results are well documented, though further detail is required on the model setup. Particular emphasis is put on the resulting deformation or linear kinematic feature intersection angle, as a means to provide a link between simulated and observed deformation features, and thus provides insight into how to select an appropriate sea ice rheology for simulation within climate models or for future studies. This method presents an exciting way to link observations and simulations of sea ice deformation, but there are certain aspects of the study that, in my opinion, need addressing.

We thank the reviewer for a thorough review and for highlighting grammatical mistakes in the original manuscript

R1#2, Firstly is selection of model resolution, and consideration of whether to simulate a single floe or a continuum of many floes. The selected 25 m resolution makes this a simulation of a single floe, and the paper describes the study as such, but there is little discussion of whether the selected VP rheology is a valid representation of a single floe. The VP rheology was developed to simulate a continuum of floes over ocean length scales, and it is not immediately obvious to me, that the VP rheology is a valid representation of the deformation of a single floe just by reducing the model resolution to that of single floe, and setting the ice concentration to unity as performed in this study. I am unaware of previous simulations of solid body deformation from other fields, and it may be the case that VP like rheologies are well studied, but this needs to be discussed in this paper. On the other hand this paper may be be a proof of concept that the VP rheology can be used to simulate the deformation of a single floe, and if it is, then it needs to be worded as such.

Typical floe size in the Arctic Ocean are of the order of 10 km, so that the continuum assumption can only be valid for model spatial resolution of 100km (Overland et al., 1995). Despite this, the use of VP rheologies with spatial resolution lower than 100km is now common practice even in lower resolution Global Climate Models. This raises the valid concern expressed by the reviewer. We argue that if the modes of failure in a single ice floe are the same as those of an aggregate of floes, then the continuum assumption can be used at spatial resolution higher than the 100km barrier. This is the implicit assumption currently made in the community with the use of higher spatial resolutions even in lower resolution Global Climate Models. We note that the mode of fracture in continuum models is independent of the scale of the problem. For instance, the results presented in the paper would not change if the domain and spatial resolution were increased by a factor of 500 (i.e. dx = 125km).

We added on page 2 of the revised manuscript :*"It can be argued that if the mode of deformation of a single floe is similar to that of an aggregate of floes, a given rheology developed for a continuum can still be applicable at spatial resolutions of the order of the floe size (Overland et al., 1998), but the validity of a given flow rule across scales is not clear."*

R1#3, A further implication of simulating a single floe, rather than a continuum floes, is the observations that the paper discusses when validating the results. The RADARSAT and RGPS data is given as a means to validate the yield curve selected. But the RGPS data is given on continuum length scales, so to validate a selected yield curve the comparison will need to be made with simulations over continuum length scales, not on the floe length scales presented in this paper. To validate the simulations of this paper observations of individual floe shape, aspect ratio and floe-to-floe crack intersection angle from aerial images of floes will be best suited. Secondly the presented results are often from the order of 1-10 seconds into the simulations. This is in contrast to idealised deformation experiments of Hutchings et al. (2004) and Heorton et al. (2018) where the results are given at 1 - 24 hours of simulation, and explored over multiple days. Within these studies the initial conditions and early stress/deformation states are explored and documented. Also in both of these cases, the initial state of the ice over was seeded with noise (in strength, or thickness and forcing) to allow for features to develop and stop an unrealistic uniform sea ice cover. Were such considerations performed in this study? Are the initial conditions uniform? What are the implications of using results from 1-10 seconds of model run? I would like to see documentation of the time series of stress/strain state in order to validate the idealised experiments and the initial conditions. I personally found the longer documented run of 45 minutes interesting, with individual floe-like shapes appearing that can be compared to images of floes.

- We did not use observations of angles of fracture from RADARSAT and/or RPGS dataset to validate the model results. Instead, we use a comparison study by Hutter et al. (2018b) between observed (RGPS) and simulated Linear Kinematic Features as a motivation for the present work. As mentioned above, the results presented are independent of spatial resolution, and the conclusions are applicable to continuum scale observations. This shown in Section 3.2.1 and on Figure 5.

- The goal of the study is document the fracture angle as a function of mechanical strength parameters, boundary conditions, and to compare the rheology-dependent model of deformation from those induced by inhomogeneities in the initial thickness field (Fig. 9) as in Heorton et al. (2018), and Hutchings et al. (2005). The fracture angle provides, for the first time, a meaningful diagnostic (since it is related to deformation and motion of sea ice) that allows a discrimination between different rheologies (yield curve and flow rule). Note that prior attempts using sea ice drift and PDF of sea ice deformation did not allow for this discrimination (Kreyscher et al., 1997; Bouchat and Tremblay, 2017; Hutter et al., 2018a). For this reason, we only integrate the model for a few seconds - until the fracture is apparent in the thickness or deformation field. Results from longer simulations were also performed to demonstrate that VP rheologies can simulate secondary fracture lines similar to observations with lead opening and ridging (Fig 7).

- The initial conditions are uniform in ice concentration and ice thicknesses. This has been clarified in Section 2.3 of the revised manuscript. Ice strength was not modified.

- As it is now explained in section 3.1 on page 9 of the revised manuscript, the ice floe fails starting from the first time step with stress states on the edge of the yield curve.

R1#4, Also there is little discussion of the implications and ‘robustness’ of the presented model results. The authors described the results as ‘robust’ on multiple occasions but make little effort to inform the reader why they are robust. Figure 5 is presented as a domain/resolution study but only mentioned once in passing as an indication of the models robustness. I’m assuming that the model domain/resolution/run time has been investigated but there is no documentation or discussion of the models limitations. A section describing this is required in order to allow the other results to be published.

As stated above, we are not interested in subsequent deformation after ice fracture. For this reason, we do not present results on the sensitivity to time of integration. We changed the organisation of the results section of the manuscript by regrouping all sensitivity experiments in one section (3.2), including a short description on the sensitivity of the results to spatial resolution and domain size on page 11, Section 3.2.1, of the revised manuscript.

R1#5, Thirdly it is not obvious how the deformation or linear kinematic feature intersection angle were calculated. I would like to see this information given in an appendix, such a method is a very useful contribution to studies of sea-ice rheology and one that I would like to use in the future. A citation to another study where this method was performed and is described is another option. The given appendix showing the theory behind internal friction and Mohr-Coloumb failure criterion, whilst interesting to see, does not appear to be original theory for this paper and is not required and the dependencies can be stated and cited.

- The angle of fracture is measured using a free image processing software (GNU Image Manipulation Program, GIMP). The small number images to treat did not call for a special program to measure angles as done in Linow and Dierking (2017), Mohammadi-Aragh et al. (2018) and Hutter et al. (2018b) was not necessary because the number images to be processed was small. **To clarify this issue, we** added the following text on page 7 of the revised manuscript: *"The angle of fracture is measured with the angle measuring tool of the GNU Image Manipulation Program (GIMP, https://www.gimp.org/). A special automatic algorithm to measure angles is described in Linow and Dierking (2017); Hutter et al. (2018b).".*

- We decided to keep the Mohr-Coulomb theory in Appendix A for the sake of completeness. We have added the following reference in Appendix A for readers who want to see a related description of the theory (Hibler and Schulson (2000) Pritchard (1988).

Specific comments:

R1#6, Page 1 Line 6-8 What are the dependencies of typical granular materials? The sand castle analogy is not useful.

We decided to remove the sand castle analogy from the abstract but to keep it in the discussion/conclusion because it is something that anyone within or outside the community is familiar with; it was also well received at the future of Earth System Modeling workshop at CalTech in November 2018. We rewrote the abstract.

The new abstract is *"Recent high resolution pan-Arctic sea ice simulations show fracture patterns (Linear Kinematic Features or LKFs) that are typical of granular materials, but with wider fracture angles than those observed in high-resolution satellite images. Motivated by this, ice fracture is investigated in a simple uni-axial loading test using two different Viscous-Plastic (VP) rheologies: one with an elliptical yield curve and a normal flow rule, and one with a Coulombic yield curve and a normal flow rule that applies only to the elliptical cap. With the standard VP rheology, it is not possible to simulate fracture angles smaller than* 30°*. Further, the standard VP-model is not consistent with the behaviour of granular material such as sea ice, because: (1) the fracture angle increases with ice shear strength; (2) the divergence along the fracture lines (or LKFs) is uniquely defined by the shear strength of the material with divergence for high shear strength and convergent with low shear strength; (3) the angle of fracture depends on the confining pressure with more convergence as the confining pressure increases. This behavior of the VP model is connected to the convexity of the yield curve together with use of a normal flow rule. In the Coulombic model, the angle of fracture is smaller (*$\theta = 23^\circ$*) and grossly consistent with observations. The solution, however, is unstable when the compressive stress is too large because of non-differentiable corners between the straight limbs of the Coulombic yield curve and the elliptical cap. The results suggest that, although at first sight the large scale patterns of LKFs simulated with a VP sea ice model appear to be realistic, the elliptical yield curve with a normal flow rule is not consistent with the notion of sea ice as a pressure-sensitive and dilatant granular material."*

R1#7, Page 1, Line 8 what model? this paper or previous?

We meant the mathematical description of the ice, the rheology, i.e. the elliptical hibler Viscous-Plastic rheology implemented in MITgcm.

The abstract was changed (see R1#6 above), this sentence has been deleted.

R1#8, Page 1, Line 10-14 More description of 'typical granular materials' that are not accurately described and all comparisons are difficult to follow. I would avoid these loose comparisons in the abstract and stick to definite results.

We wanted to give the reader a hint of the significance of our results by giving a comparison to granular material properties. But it might lead to confusion. We will limit the comparison to the discussion part of the paper.

We modified the abstract as specified above for comment R1#6

R1#9, Page 1, Line 24 are the two citations model studies or observations of sea ice floes? Observational studies are required for this sentence.

Overland et al. (1998) is an observation study base on buoy data, while Tremblay and Mysak (1997) is a modeling study. We remove this last reference and replace it by another observational study (Rothrock and Thorndike, 1984).

R1#10, Page 2 Line 5 Leads plural, dangerous to use the word memory when describing a computer model, 'emergent anisotropy' is more accurate.

Both have been corrected as suggested.

R1#11, Page 2, Line 8 The equations are difficult to solve due to their non-linearity and complexity not because of sea ice. It is however difficult to represent sea ice with simple, easily solvable equations due to it's non-continuous features.

We changed this sentence to express our thought more clearly.

The first sentence of the 2nd paragraph of the introduction is changed to : *"The sea ice dynamics are complicated because of sharp spatial changes in material properties associated with discontinuities (e.g. along sea ice leads or ridges) and heterogeneity (spatially varying ice thickness and concentration). The sea ice momentum equations are difficult to solve numerically because of the non-linear sea ice rheology."*

R1#12, Page 3 Line 24 argues - argued

Corrected as suggested.

R1#13, Page 3 Line 28 rheologies plural

Corrected as suggested.

R1#14, Page 3 Line 32 check citations and parenthesis

Corrected as suggested.

R1#15, Page 3 Line 33 check citations and parenthesis

Corrected as suggested.

R1#16, Page 3 Lines 34-35 'Based on these satellite observations, amongst others (provide some examples), and in-situ…'

The sentence was re-written as: *"Based on satellite observations (e.g. RADARSAT Geophysical Processor System, RPGS, or Advanced Very-High-Resolution Radiometer, AVHRR), and in-situ internal ice stress measurements (e.g. from the Surface Heat Budget of the Arctic Ocean, SHEBA,*

experiment),[. . .]"

R1#17, Page 3 line 6 space before 'Girard'
Corrected as suggested.

R1#18, Page 3 line 13 delete parenthesis
Corrected as suggested.

R1#19, Page 3 line 15 'are appear as line of' - 'appear as lines of', deformation singular, 'with the deformation' - 'with shear deformation', divergence - convergence
Corrected as suggested.

R1#20, Page 3 Line 18 leads - leading
Corrected as suggested.

R1#21, Page 3 line 19-20 check citation parenthesis
Corrected as suggested.

R1#22, Page 4 line 1 Wilchinsky et al. also deduced intersection angles between floes that are relevant to this paper.

The reference to Wilchinsky et al. (2010) has been added to the list of citation.

R1#23, Page 5 Figure 1. What is 'Mohr-Coloumb flat' In general I found the figure captions to be lacking in content. Can they all be expanded to directly describe what simulation they are from, and the part of the figure that is of interest? A reference link to where in the paper (section number) it is described and discussed is also required. I found myself flipping back and forward trying to work out what simulation was illustrated in which figure, please include more information to avoid this please.

We re-wrote the figure captions and made them self-sufficient as suggested by the reviewer. *Flat* refers to the linear part of the yield curve. We replaced the word *flat* by *linear limb* which is more accurate.

The captions of all figure have been extended to be self-sufficient. The caption of figure 5 now reads *"Elliptical yield curve (black) with ellipse aspect ratio $e = a/b = 2$. Coulombic yield curve (red) and elliptical capping with internal angle of friction (μ). Both e and μ are measures of the shear strength of the material. The normal flow rule applies only to the elliptical part of the yield curves. For the two straight limbs of the Coulombic yield curve, the flow is normal to the truncated ellipse (dash-dot line) with the same first stress invariant. Note that the axes σ_1, σ_2 and σ_I, σ_{II} do not have the same scale."*

R1#24, page 5 Line 5 to 15. I am assuming that this paragraph describes the physical phenomena that the viscous plastic rheology and associated yield curve are designed to replicate. However this paragraph is worded such that it is an accepted and proved fact that sea-ice is viscous plastic and has behaviour that follows all these rules. Also the paragraph contains no citations. Rewriting this paragraph to emphasise that the viscous plastic rheology is designed to simulate the stress/strain relationship of sea ice over continuum length scales is required. It will also help to address the theoretical implications of using a rheology designed for the continuum approximation of sea-ice to simulate the deformation of a single floe.

This paragraph was reworded taking into account the comment from the reviewer. Please see page 6 of the revised manuscript.

This paragraph now starts with *"The VP rheology was originally developed to simulate ice motion on a basin scale (e.g., Arctic Ocean, Southern Ocean) (Hibler, 1979). In this model, stochastic elastic deformation is parameterized as highly viscous (creep) flow (Hibler, 1977). Ice is set in motion by surface air and basal ocean stresses moderated by internal ice stress."*

Page 6 line 11 check parenthesis

The missing parenthesis have been added

R1#25, Page 6 line 15 Is this 'theoretical angle' the one used to retrieve the LKF intersection angles from simulation results, and also with previous studies? If so can you state it here. The paper has not informed me how this study, and previous studies obtained the intersection angles widely discussed.

Yes it the theoretical angle of fraction derived form the Mohr's circle and the Mohr-Coulomb yield criterion. It is described in several paper, e.g. in Ip et al. (1991); Hibler and Schulson (2000), and , as shown in appendix A and B, it is in agreement with the characteristics lines described in Pritchard (1988)

The 3 references above have been added on page 8 of the revised manuscript in the sentence : *"The theoretical angle of fracture θ can be calculated from the Mohr's circle of stress and yield curve written in the local (reference) coordinate system (Ip et al., 1991; Pritchard, 1988; Hibler and Schulson, 2000). Details are described in the appendix. For a Mohr-Coulomb yield criterion, θ follows immediately from the internal angle of friction, that is the available shear strength. An instructive analogue is the slope of a pile of sand on a table. Wet sand can support more shear stress and hence the slope angle can be steeper (smaller)."*

R1#26, Page 8 line 6 is the model domain used in all experiments? If there are exceptions please list them.

This model domain is used in all experiments, except for two experiments reported in Section 3.2.1 (Figure 5 and Figure 7).

We inserted : *"The model domain is a rectangle of size 10 km × 25 km, except for Sect. 3.2.1 and Sect. 3.2.2"* on page 8 on revised manuscript

R1#27, Page 8 line 10 - 11 This statement about the robustness of the results is not backed up. Please refer the reader to the results that back up this statement. The proof of the robustness of this model needs to come first before any other results.

See Section 3.2 on page 10 of the revised manuscript for a discussion on the sensitivity of the results to different boundary conditions. In our opinion, the demonstration that VP rheologies can simulate realistic fracture lines that have angles in accord with theory should be presented first. This is the reason why we present these results first, before the sensitivity of the results to the boundary conditions. The boundary conditions in this context can be seen as external forcing on the interior solution.

R1#28, Page 8 Lines 11 - 17 Please state how the model time step works? I am not familiar with how the LSR solver for the VP rheology works. How does the model work in time? I am familiar with models that have a constant time step with solution iterations per time step. The model then continues time stepping for the required simulation period. Does your model work in the same way? Or is there a selected simulation time, and then the documented 1500/1500 iterations performed to cover the simulation time? If so then can you describe why the simulation time was selected as you have done with the spatial resolution. Is the simulation time and temporal resolution/number of iterations the same for all simulations?

The non-linear momentum equation is solved iteratively until a converged solution is obtained. Typically 1500 iterations are required to reach convergence. Then the external forcing is then updated and a new solution calculated. This has been clarified on pages 7-8 of the revised manuscript.

We modified the text describing the numerical solver :*"We solve the non-linear sea-ice momentum equations with a Picard or fixed point iteration with 1500 non-linear or outer-loop (OL) iterations. Within each non-linear iteration, the non-linear coefficients (drag coefficients and viscosities) are updated and a linearized system of equations is solved with a Line Successive (over-)Relaxation (LSR) (Zhang and Hibler, 1997). The linear iteration is stopped when the maximum increment is less than* $\epsilon_{LSR} = 10^{-11}\,m\,s^{-1}$*, but we also limit the number iterations to 1500. Typically, 1500 non-linear iterations are required to reach a converged solution. This is so because of slow convergence due to the highly non-linear rheology term and the high spatial resolution (Lemieux and Tremblay, 2009)."*

R1#29, Results section A paragraph describing which simulations have been performed will be useful here. This will save having to flip back and forward through the paper to match results discussion and figures. The simulation 'robustness' results need to come first in order to validate the following results.

A paragraph have been added at the beginning of the Section 3 (Results) on page 9 of the revised manuscript, stating the different part of the result section. A paragraph has been added at the beginning of Section 3.2 on page 10 of the new manuscript to list the sensitivity experiment that have been performed.

The first paragraph of Section 3.2 now reads *"We use simple uni-axial loading experiments to investigate the creation of pair of conjugate faults and their intersection angle. After presenting the results of simulations with the default parameters (Section 3.1), we explore the effects of experimental choices: confining pressure, choice of boundary conditions (i.e. von Neumann versus Dirichlet), domain size and spatial resolution and inhomogeneities (i.e. localized weakness) in the initial thickness and concentration field (Section 3.2). Finally, we study the behaviour of two viscous plastic rheologies with different yield curves and compare these dependencies to what we can infer from smaller and larger scale measurements from laboratory experiment and RGPS observations (Section 3.3)."*

R1#30, Page 8 line 29 Figure - figure (no capital), 'measured intersection angle' how was this angle measured?

We modified the manuscript to comply with the journal standards, i.e. using *Fig. or Figure, Eq. or Equation , Table, and Sect. or Section.*

The word *measured* is now replaced by *simulated*. We measured the angle using the GIMP software. See response to comment R1#5 above for more details.

R1#31, Page 8 line 27 quantify 'right away' and how can a fracture appear but not in the deformation field? what field did it appear in? Can you comment on this time scale compared to observations of floe fracture (Dempsey et al. 2011 Fracture of a ridged multi-year Arctic sea ice floe)

After 1 timestep, the stress states reach the yield curve and deformation occurs. We see this immediately in the strain rates (divergence and deformation). For the results presented in the paper, we have iterated for 10 additional seconds in order for the signal to also be seen in the thickness and concentration field. We do this to more clearly show the fixed link between sea ice shear strength and divergence in the standard VP rheology of Hibler. We removed the sloppy term "right away" from the text. The reference to Dempsey et al. (2012) was also added in the discussion.

The new text on page 9 of the revised manuscript now reads: : ***"After 1 timestep (or 0.1 s), the stress states already lie on the yield curve and the fracture is readily seen in the deformation fields (divergence and shear). We iterate for a total of 20 seconds in order for the signal to be apparent in the thickness and concentration fields. We do this to more clearly show the link between position of the stress states on the yield curve and the normal flow rule in the standard VP rheology of Hibler (1979)."***

On page 22 of the revised manuscript (discussion section), we added the following sentence : ***"Observed time scales of fracture are on the order of 10 seconds for 60 m floe diameters (Dempsey et al., 2012, Figure 6 top right panel) and from typical elastic wave speeds of 200–2000*** $\mathrm{m\,s^{-1}}$***, large cracks of order 1000 km can form in minutes to hours (Marsan et al., 2012)."***

R1#32, Page 9 line 7 The 'robustness' results need to come first, then the model resolution and time period choice can be validated against them.

Please see response to comment **R1#27** above for a justification.

The angle of intersection between a pair of conjugate fault does not change with domain size and spatial resolution (see Fig. 5).

R1#33, page 9 line 9 extended time period, what was the original time period?

The original time period of the simulation is 20 seconds with a 0.1 second time step. The length of the simulations is not important here as we are showing only results from the first timesteps.

We added a reference to Table 1 in the revised manuscript on page 9 section 3.1. We also changed the sentence on page 7 (please see next comment R1#34 below)

R1#34, Page 9 line 10 total iterations or iterations per time step? what is the time step?

It is the number of sub-cycles used to solve the non-linear momentum equation. This should be clearer in the new version of the manuscript (see response to comment R1#28 above)

The first two sentences now read: ***"Continuing the integration to 2700 seconds (45 min), compared to 20 seconds in the reference simulation leads to the creation of smaller diamond-shaped ice floes due to secondary and tertiary fracture lines (Figure 6)."***

R1#35, Page 10 Figure 3 Bottom left pane. Please use a bipolar colour scale for bipolar data. As in a different colour for +/-, white for zero for example. These scales are easily selectable.

The colorbar were changed as suggested.

R1#36, Page 10 Line 8 'similarly as' - 'similar to'

Corrected as suggested.

R1#37, Section 3.3 If this section addresses the robustness then it needs to go first and also discuss the results in figure 5. Please also comment on the limitations of this model.

Please see the new Section 3.2 and the answer to the comment R1#27 above.

The limitations of the VP model are discussed in the introduction on page 2 of the revised manuscript. All sensitivity experiments - including sensitivity to spatial resolution and domain size - are now presented together in a new section 3.2 entitled: *Sensitivity experiments.* The limitations of the this study are discusses in the discussion section on page 22 of the revised manuscript.

We added in the discussion section on page 22 :*"The simulations presented in this study are not realistic and cannot be compared directly to observations of ice floe fracture. For instance, our idealized ice floe is homogeneous while sea ice is known to feature some weaknesses like thermal cracks or melt ponds."*

R1#38, Page 11 Figure 5 First change or rescale the colourscale to highlight features. The max deformation appears to be around 10^{-4}, so limit the colour to this point, Also please label the colour scale legend with units. Why have you selected only 2 seconds of model run. what happens later in the run? Is this on a similar time scale to observations of floe fracture? Consider plotting a later time point if available or discuss how the model proceeds.

We want to keep the same scale for every plot. Yes, the maximum value is approximately equal to 10^{-4}. We added color bar legends with units. We limited the simulation to 2 seconds because of computational constraints. After 2 sec, the fracture angle is already visible, and it is not necessary to run the simulation any longer time, because this would simply make the signal stronger in the thickness and concentration fields. This was tested using a smaller domain. We clarified this on page 11 of the revised manuscript.

We have re-written the captions of all figures including the reference to the appropriate section as suggested by the reviewer. We justified our choice of total integration of 2 sec for this experiment in the revised manuscript.

The caption was modified : *"Maximum shear strain rate (second strain invariant) after 10 seconds of integration for the default domain size and* $\Delta x = 100\,m$ *(a) and* $500\,m$ *(b), and for the default* Δx *and a doubled domain size of* $20\,km \times 50\,km$ *(c). Note that for case of the double domain (c), the southward velocity at the northern boundary was also doubled to keep the deformation rate constant, and that this simulation is limited to 2 seconds for numerical efficiency."*

R1#39, Page 12 again bipolar colour scale for bipolar field would be appreciated. All of your plotted fields so far have been for deformation, a plot of a stress field, if available, will be nice to see. There are lots of crack intersections in this plot. Is it possible to obtain all of these intersection angles? A distribution of angles could then be presented.

While these simulations are indeed cool to look at (especially when you animate them!), it does not seem useful to us to investigate the angles of all of these leads in detail. Most of them were created after the initial fracture, therefore the direction of stress and magnitude of stress have been modified by ensuing fractures and deformations so that the analysis would be confounded. We can see that the fracture pattern is not absolutely symmetrical. This means that the converged solution is not reached. In principle, this is possible, e.g. with the software of Hutter et al. (2018b). We find the stress field not to be helpful in our case, the deformation field showing the fracture lines is more important for us to explore the effects of the rheology.

The colorbar has been corrected as suggested.

R1#40, Page 13 Line 5 Comment on ice strength/thickness vs fracture angle. Is this a result you have observed? Or is it a theory that you are testing? Is there a citation for this theory?

Our statement is a little misleading and has been rephrased to express that because ice strength in the model is a linear function of the ice thickness, see equation 4 on page 5, or (Coon et al., 1974; Hibler, 1979), and the fracture angle depends on ice strength, it implicitly also depends on the ice thickness.

We included on page 13 of the revised manuscript the sentence :*Note that the ice strength is linearly related to the ice thickness (Eq. 4). Therefore the normal stress at the edge of the floe is completely defined by the thickness of the surrounding ice.*

R1#41, Page 13 Line 17 You start this paragraph with statements about the link between initialised faults and deformation. Is this a theory you are testing? If so give a citation. Is it your interpretation of the results? if so you need to state the results and the reference the figure first.

This is a hypothesis that we can support by our simulations shown in Fig.9. The hypothesis was formulated previously and also tested (Aksenov and Hibler, 2001) and (Hibler and Schulson, 2000). It has not been tested using models whether the angle of fracture is dictated by in-homogeneities in the sea ice cover or the yield curve and flow rule (see appendix B1 and B2 for the elliptical and Coulombic yield curve in the VP rheology).

We rewrote the text of this section on page 14 of the revised manuscript : *"So far, all initial conditions have been homogeneous in thickness and concentration within the ice floe. In practice, sea ice (in a numerical model, but also in reality) is not homogeneous. A local weakness in the initial ice field is likely the starting point of a crack within the ice field (e.g., Herman, 2016, her Figure 5c). Local failures raise the stress level in adjacent grid cells and a crack can propagate. Note that the crack propagation in an "ideal" plastic model such as the VP model is instantaneous and this propagation is not seen between time steps. As a consequence, lines of failure will likely develop between local weaknesses. The location of weaknesses in the ice field together with the ice rheology (yield curve and flow rule) both determine the fracture angles (Hibler and Schulson, 2000; Aksenov and Hibler, 2001).*

To illustrate this behavior, we start new simulations from an initial ice field with two areas of zero ice thickness and zero ice concentration, hence

weaker ice (Figure 9a). After $5\,s$ *these simulations yield fracture patterns that are dramatically different from those of the control run simulation (Section 3.1): the fracture lines now start and terminate at the locations of the weak ice areas. Still, changing the shear strength of the ice (by changing* e*) changes the fracture pattern (Figure 9b and c). With* $e = 1$*, the angles are much wider than with* $e = 2$*, which is consistent with the general dependence of fracture angles on* e *(see Sect. 3.3.1). Our simulations cannot lead to conclusive statements about the relative importance of heterogeneity of initial conditions and yield curve parameters for the fracture pattern, but we can state that both affect the simulations in a way that requires treating them separately to avoid confounding effects. Details are deferred to a dedicated study."*

R1#42, Page 14 Figure 8 caption is lacking detail. Is this the figure for the lateral confinement experiments, or ice thickness experiments? Please describe what is shown in every pane.

All captions have been re-written. This is the lateral confinement experiment. Thanks for pointing this out.

The caption of figure 8 have been modified to be : *"Maximum shear strain rates (left) and stress state in stress invariant space (right) after 5 seconds of integration for different confinement pressure:* $h_c = 0.05\,m$ *(a) and* $h_c = 0.3\,m$ *(b). Note, how stress states with divergent strain rates (a) migrate left towards convergent strain rates (b)."*

R1#43, Page 15 Figure 9 please label colour scale legends.

Corrected as suggested.

R1#44, Page 16 Figure 11, again please describe the simulation this figure corresponds to? The colorscale are saturated so consider rescaling.

In the case of the top left panel (now panel a)), the log-scale of the colorbar does not allow me to use a non-saturated colormap. There is lot a simulated ice that deforms really slowly (viscous creep) These areas are not interesting for us, so it is not necessary to display them in the colormap. Additionally, logarithm of values close to zero are close to $-\infty$, so impossible to display on a colormap. We use a logarithmic colormap to have a better contrast of value of deformation, that have a really steep changes. We rewrote the caption, see below.

The caption for this figure now read *"Maximum shear strain (a), ice thickness anomaly (b), divergence (c) and stress state in stress invariant space (d) after 5 sec of integration for a smaller ellipse aspect ration (*$e = 0.7$ *compared to* $e = 2$ *in the reference run in Sect. 3.1). Compared to the control run on Fig. 3, the angle of fracture is larger (*$\theta = (61 \pm 1)^\circ$*), the stress states are in the second half of the ellipse (with strain rates pointing into the convergent direction) and there is convergence along the fracture lines (panel b) in agreement with the schematic in Fig. 4"*

R1#45, Page 17 line 18 A lot of LKFs - how many? Or is it more than compared to another simulation? rephrase or quantify.

We do not expect uniform piece of modelled ice to break in any other way than with 2 fractures. We rephrased this sentence to explain why the creation of more than 2 conjugate faults is problematic

We rephrased and changed the sentence by *Sea ice shear strength is small*

for small stresses, and ice deforms strongly along the ice edge. Many small LKFs develop, but no large fractures spanning the entire floe, as expected in a uni-axial compressive test with an homogeneous plastic material.

R1#46, Page 17 line 21 space before theta
Corrected as suggested.

R1#47, Page 18 Figure 12 are these Coulombic curve simulations? again this caption needs more detail.
Corrected as suggested
The captions of all figure have been extended to be self-sufficient and the suitable sections referenced.

We changed to caption to *"Maximum shear strain (top) and stress state in stress invariant space (bottom) for different internal angles of friction. (a) $\mu = 0.7$ or $\phi = 44°$, (b) $\mu = 0.85$ or $\phi = 58°$ and (c) $\mu = 0.95$ or $\phi = 72°$ after $5\,s$ of integration. The angles of fracture are $\theta = 23°$, $(28 \pm 2)°$ and $41°$. Fig. 10 illustrates how θ depends on μ for a Coulombic yield curve."*

R1#48, Page 18 line 4 'realistic manner' how are they realistic? what simulations are you comparing to what observations? A figure reference and a citation are both needed for this statement.
We mean "look realistic" when compared with observations from RGPS and reported in Hutter et al. (2018b) or lab experiments reported in Schulson (2004). We wanted to express the way the model produces small floes that appear realistic, but may be not so. Still, we can compare to small lab experiment from Schulson and Duval (2009) and see several similarities, although at different scales.
We changed the sentence to: *The fracturing of the ice floe creates smaller floes in a realistic manner, for example, compared to to Landsat-7 images (Schulson, 2004, Figure 2)*

R1#49, Page 19 line 12 please refer to the section or figure or both that show the resolution and scale non-dependance.
The section and figure is now referred to on page 20 of the revised manuscript.

R1#50, Page 19 line 15 'appear' - 'appears'
Corrected, as suggested

R1#51, Page 19 line 26 please give a citation for the statement on granular material.
The citation Balendran and Nemat-Nasser (1993) was added

R1#52, Page 20 line 22 Citation required for 'Arctic-wide simulation'
We added a citation to Hutter et al. (2018b)

R1#53, Page 20 line 28 why is it unsurprising? Do mean that your results fit with previous theory and results? If so can you say this and cite them?
This is not surprising because it is the role of the yield and the flow rule to determine the deformation of the solid. **We modified the sentence to clarify our opinion: *"Unsurprisingly, the yield curve plays an important role in fracturing sea***

ice in a numerical model as it governs the deformation of the ice as a function of the applied stress."

R1#54, Page 20 line 33 'The ice open and create leads' - The simulated sea ice opens and creates leads

Corrected as suggested

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