Answer to tc-2018-192-RC2 – Jennifer Hutchings

February 14, 2019

<u>Note:</u>

- 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

R2#1, I am really pleased to see a paper that is making suggestions for idealised experiments we can use to differentiate between rheological models for sea ice. This in itself is worth publishing. The main result of the paper is that the elliptical rheology is inappropriate for representing observed cracking orientation in the ice pack, which is interesting and helps motivate changing sea ice rheological models used in climate and weather prediction.

We thank the reviewer for the numerous interesting comments about our work. We tried below to answer and address all of them in this new manuscript.

R2#2, I do have some concerns that the interpretation of observational data needs sharpening, and the results must not be overly interpreted given limitations of the use of RGPS data. Identifying intersection angles for lead pairs actually requires more work (and dedicated field data collection) than this paper warrants. You are motivated by the fact that a simulation shows larger intersection angles than RGPS, and I agree that this is something to address. I just do not think you can use RGPS to determine what the intersection angle should be, just that it needs to be smaller. Note, there are others in the community funded to do this work of identifying fracture patterns associated with particular modes of failure. For example I have an NSF and NASA project that is looking at identification of modes of failure from satellite imagery. There are also upcoming field experiments that could provide case studies to constrain the actual behaviour of sea ice, which should provide further guidance for use of your idealised cases to constrain rheological model design. I would be happy to talk to you in person about using this analysis and data to support future model validation efforts. I would also caution you to be more careful in your description of the differences between VP and granular models. Some clarification missing from the manuscript is provided in my comments. I also have suggestions for why the VP model creates LKFs, which I feel is important for understanding the validity of LKFs in the viscous plastic sea ice model representing nature.

We agree with you that determining the intersection angle of conjugate faults from the RGPS LKF data-set has a few limitations (only large cracks, temporal resolution of 3 days,...). Given the large variety of forcing conditions, the RGPS LKF data-set includes LKFs originating from multiple modes of failure, but also shows conjugate fault pairs. We, here, name two advantages of using the RGPS LKF data-set to evaluate intersection angles in Pan-Arctic sea-ice simulation: (1) the data-set covers 65% of the Arctic Ocean and spans over twelve years, which is a much higher coverage compared to hand-picked studies in satellite imagery (Erlingsson, 1988; Walter and Overland, 1993). (2) The data-set enables a consistent comparison with model output as it is based on sea-ice deformation. As the intersection angles are consistent with other studies (e.g. Walter and Overland, 1993) we are confident that this approach can be used to determine whether a model is over- or underestimating the intersection angle. We, here, want to stress that we only use the misfit of intersection angles in the RGPS LKF data-set and in a Pan-Arctic sea ice simulation to motivate our work to further study the dependency of the rheology and yield curve on the intersection angle. In our manuscript, the RGPS LKF data-set is not used to evaluate our idealized experiments. We rewrote the corresponding abstract accordingly to make this point clear.

Specific comments:

R2#3, Check spelling throughout the manuscript. Also check for missing brackets throughout. Grammar can be improved in places, and sometimes words are repeated. Make sure you have someone very carefully proof read the manuscript. I did not correct all the typos I saw because I am short on time and wanted to focus my attention on the central messages in your paper.

We have carefully proofread the manuscript. We apologize for the many technical problems.

R2#4, In the introduction be specific that you are considering conjugate fault pairs that form under specific confining stresses, the orientation of which is controlled by the yield curve shape and flow rule. In particular reference Pritchard in the introduction. It is only when I got to the conclusion that I saw you were aware of this work and it was motivating your study. It is wise to point out that not all applied stress will result in intersecting fault pairs (for example tension and pure compression do not).

We included a citation of Pritchard (1988), and clarified that pure compression and tensile cracks do not form pair of intersecting fault. Thanks for pointing this out.

We added this text in the introduction : *"Pritchard (1988) investigated the yield curve's mathematical characteristics and derived angles between the principal stress directions and characteristics directions that depend on the tangent to the yield curve. These results show that stress states exist in plastic materials where no LKFs form and were later used to build a yield curve (Wang, 2007).".* We also changed the first sentence of the last paragraph of the introduction by : *In this paper, we simulate the creation of a pair of conjugate faults in an ice floe with two different VP rheologies in an idealized experiment at an unprecedented resolution of 25 m. We explore the influence of various parameters of the rheologies and the model geometry (Scale, resolution, confinement, boundary conditions, and heterogeneous initial conditions).*

R2#5, Page 2 line 4. The appropriate references for efficient solution is Hutchings et al. 2004 or Jean-Francois Lemieux et al. 2010, I would not call LSOR or Hibler's method, which I used in the 2005 paper, as efficient. This introduces the efficient solution method that correctly couples P and U, for a convergent plastic solution. This solution method was not used in Hutchings et al. 2005. Hutchings et al. 2005 is the correct reference for qualitatively reproducing LKFs in the viscous plastic model.

Thank you, we corrected the citations.

R2#6, Page 2 line 15: MEB? typo? I think you need to introduce the acronym for the Maxwell-Elastic-Brittle model here.

Corrected as suggested. We replaced "*Viscous*" by "*Maxwell (viscous)*"

R2#7, Page 2, line 24: argues → argued.

Corrected as suggested.

R2#8, Page 2, line 31: Flato and Hibler 1992 is not a mohr colomb relationship. The cavitating fluid behaves very differently and the first SIMIP (see work by Kreyscher and Harder) indicated this was not a suitable stress-strain relationship for sea ice. Also check that Ip et al. 1991 is not using a different flow rule to Tremblay's. I am wondering if you are missing text here, as these two references were left hanging

The mohr-coulomb yield curve was presented in the appendix of the Flato and Hibler (1992) paper as a possible extension to the cavitating fluid sea ice model. The way we referenced it in the paper was mis-leading.

We replaced the last sentences of this paragraph by : *" Alternative VP rheologies were never widely used in the community. These include a Coulombic yield curve with a normal flow rule (Hibler and Schulson, 2000), a parabolic lens and a tear-drop (Pritchard, 1975), a diamond-shape yield curve with normal flow rules (Zhang and Rothrock, 2005), a Mohr-Coulomb yield curve with a double-sliding deformation law (Tremblay and Mysak, 1997) or a curved diamond (Wang, 2007)."*

Page 2, some important points that I do not think are clear in your introduction:

R2#9, The Elastic-Plastic model developed during AIDJEX was based on assumptions of a material with embedded cracks in all directions that are sub-grid scale. This is closer to a ductile material than granular material.

Assuming that cracks are present in the pack ice in all direction was used to justify the isotropic assumption in the Coon et al. (1974) - later corrected in Coon et al. (2007) where the authors argued that an anisotropic assumption should be used instead. The coarse resolution of sea ice models did nothing to motivate taking into account the granular nature of sea ice in early works on sea ice models.

We have clarified this point in the revised introduction on page 2 of the revised manuscript as *"Originally, Coon et al. (1974) assumed sea ice to have cracks in all directions, justifying isotropic ice properties and isotropic rheologies."*

R2#10, The Viscous-Plastic model is only considered valid on coarse resolution (Hibler 1977). It is possible to consider this model with the ice always being in a state of plastic failure, until you get to high resolutions that allow representation of ice areas between fractures, when the viscous creep, while numerically small, is unphysical. At small scales an elastic model is appropriate for low stress states. The viscous behaviour inside the yield curve is often treated as regularisation required for numerical solution.

It is true that VP rheologies are valid only at coarse resolution, but a lot of recent works feature the use of high-resolution simulation with VP models that already break this assumption (e.g. Wang et al., 2006; Hutter et al., 2018) We also think that Viscous behavior is a regularisation of small deformations for the numerics. However it looks to us like a detail that may not need to included in the introduction. We propose to add this in the description of the VP rheology in section 2.2

We added in page 2 of the revised manuscript: *"At any scale, the assumption of viscous creep for small deformations is not physical and an elastic model would be appropriate for low stress states. The long viscous time scale, compared to the synoptic time scale of LKFs, of order 30 years (Hibler, 1979), however, allows viscous deformation to be viewed as a small numerical regularization with little implications for the dissipation of mechanical energy from the wind or ocean current (Bouchat and Tremblay, 2014), and the ice model can be considered as an ideal plastic material."*
We also added on page 6 of the revised manuscript the sentence *"Internal ice stress below these thresholds leads to highly viscous (creep) flow that parameterizes the bulk effect of many small reversible elastic deformation events. The timescale of viscous deformation is so high ($\simeq 30$ years) that viscous deformation can be seen as regularisation for better numerical convergence in the case of small deformation.."*

R2#11, Personally I think it is still not clear that the failure mode of a single floe is the same as an aggregate of floes. This has not been shown observationally or with models, and statements of scale invariance based on observed qualitative correspondence between failure modes in the lab (cm scale) and ice pack (10-100km scale) do not extend to the floe scale.

By conception, sea ice models used today are scale independent and are being used at resolution approaching the floe scale. We added a sentence to specify that the fracture process at the floe scale as not been shown to have the same failure mode as at arctic and lab scales.

We added a sentence on page 3 of the revised manuscript: *"The scale invariance of the fracture processes at the floe scale has not yet been shown, especially due to the lack of observations at both high spatial and temporal resolution."*
We also 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."*

R2#12, Page 3 Discussion regarding orientation of intersecting LKFs from RGPS: I performed a similar analysis back in the early 2000's and never published the result, which was a wide spread in intersection angle. The reason I did not publish this is because I realised that the RGPS product could potentially be capturing fracture zones that form at different times, and therefore in the product appear to be a conjugate pair because they intersect, but they are not because they were not formed under the same confining stress. This is really obvious if you spend some time on the ice pack in winter and observe leads forming and working. RGPS is not the right satellite product to use to identify conjugate fault pairs in sea ice. Hence I disagree that you can state "The wide range of intersection angles is presumably due to previous deformation history and associated hetrogeneity in the ice cover that dictates the strength locally".

Thanks for pointing this out here and also in a recent conversation with co-author Tremblay. It is correct that RGPS data represent a mean over 3 days and for this reason we cannot be certain that intersecting fractures were formed simultaneously. In revising the paper, we have downplayed the RGPS as a dataset used for validation/motivation as per your suggestion and that of the other reviewer.

R2#13, page 3, line 32: Just want to clear up one very important point about my 2005 paper. It is steep stress gradients in the model sea ice stress field that allow LKFs to form. I suspect this opening is related to an instability in the model identified by Nico Gray (Gray and Kilworth 1995). We seeded stress gradients though a random number being added to P* (which defines compressive strength). At the time I wrote this paper I was obsessed with plastic convergence of the VP solution, so made sure there were no spurious stress values due to the numerical error. The pan-Arctic model of Heil's included in this paper, and other VP models, are able to show LFKs because of the noise introduced by not converging fully to the yield curve. If you play around with a VP model you can create divergence related instabilities along gradients in forcing (e.g. non-smoothly interpolated wind fields), or even have the model blow up and crash due to one localised discontinuity in thickness (e.g. I have seen this when using a nudging method to assimilate data into the CICE model that created open water locally). The reason I bring this up is that I feel it is very important that people understand how the VP model can create LKFs. The mechanism is quite different from what might actually be happening in a granular or brittle material. I would be very happy to advise on experimental design, repeating and following up on my investigations 15 years ago.

Simulation by Lemieux and Tremblay (2009) show LKFs in a fully converged solution using the JFNK method (Lemieux et al., 2010) using a realistic but smooth thickness and concentration field. In the response to reviewer document for the Lemieux and Tremblay paper (not published), we also showed LKFs in idealized experiments with a constant thickness and concentration field. In the present paper, we also show clear discontinuity in the strain rate fields that becomes apparent in the thickness and concentration field after some integration. We see the VP model as an ideal plastic model as opposed to a viscous plastic model given that the time scale associated with the viscous term (for the default $\eta_{\max}$ an $\zeta_{\max}$) is $\sim$ 35 years and LKFs form over time scale of a few days. So, for all practical purposes, the viscous term does not operate on time scale of interest to LKFs formation. Ideal plastic material in turn can be viewed as an elastic-plastic material with an infinite elastic wave speed (stresses adjusts instantaneously with the forcing in in the "elastic" regime and can form linear kinematic features. For all these reasons, we think that the instability described in Gray and Killworth (1995) is not responsible for the formation of LKFs in a VP model. A formal comparison between elastic-viscous-plastic (MEB) model and a viscous-plastic model is underway by one of the co-authors. This will be studied in more details in that paper. Further discussion with the reviewer on this topic will be very welcome.

R2#14, page 3, line 34: The original study on shape of yield curve and ice arches is in Billy Ip's thesis, that was published later by Hibler in Hibler et al. (2006).

This reference was added to the introduction on page 3 of the revised manuscript. Thanks for pointing this out.

R2#15, page 8, line 14. A reader unfamiliar with numerical solution of the VP model will need some guidance as to what non-linear and linear iterations are. I know you are talking about the sub-cycling to reach plastic equilibrium (or close to it) and converge the velocity solution at each time step. Perhaps use language that is more obvious to a casual reader. Incidentally, did you check convergence properties? Just curious. I think you point out somewhere that the modified coulombic rheology is slower to converge -

I found that solutions for yield curves with corners never converged fully. A frustrating reality! If you follow my suggestions to delve into why the model creates LKFs you will need a full description of the interative process and convergence characteristics.

Our theory of yield curve added in appendix B gives an explanation of why a yield curve with corners gives poor convergence. We modified this paragraph to improve clarity about the LSR solver scheme and the presence of sub-cycles (or outer-loops), as also asked by the other referee.

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).***

R2#16, page 8, results section: Describe what the applied strains are in the numerical experiments (magnitude, not just direction).

The specified strains are described in equation 17 on page 8 of the original manuscript (or page 8 of the revised paper), and their magnitude is documented in Table 1. We use the same strains for all experiments, excepted for the up-scaled experiment where the magnitude of the strain is up-scaled as well.

R2#17, Page 8 line 26: What are the default parameters? I think you forgot to reference table 1.

We added the reference to Table 1 on page 9.

R2#18, page 9, line 4: Regarding your statement "Fracture occurs when the stress state intersects the yield curve". Plastic failure occurs then. The fact that a "fractures" form is because the ice deforms at a stress discontinuity where the stress accumulates and reaches yield. You are correct in pointing out that the strain-rate has characteristic directions along which divergence will occur, defined by the shape of the yield curve and flow rule. It is this divergence, relative to the confining stress, that defines the directions of the linear deformation features in the model runs.

We agree on this, this paragraph have been modified to clarify this point.

This sentence has been rephrased as ***Fracture occurs after plastic failure when the stress state reaches the yield curve and the ice starts to move in divergence.*** **for clarity.**

R2#19, Comment on differences between 3.1 and 3.2: The change in nature of cracks when you decrease the number of internal iterations (linear iterations) is probably related to the fact that the ice stress field is more heterogeneous (further from the converged solution) and the LSOR method tends to create noise in the stress field and smoothes with increasing number of iterations (unlike the SIMON method I proposed, Hutchings et al. 2004, that has a smoother convergence to the yield curve). Hence there are more points where LKFs can nucleate when you reduce the number of internal iterations. This is just a suggestion, with out looking at the stress fields in your experiments I can not tell you if this is what is actually happening. Incidentally, another unpublished result that I presented at AGU in 2003: The VP model can create intersecting deformation features across the entire Arctic Ocean is you do not converge to plastic equilibrium and are not careful in smoothing the solution between time steps (which can be done numerically through the choice of advection scheme or Bill's introduction of artificial diffusion in his 1979 paper). I never followed up this work. I suspect that this is a direct consequence of Gray's instability. This instability is damped by the addition of numerical diffusion (or artificial diffusion) in the solution proceedure. We might think the resultant strain-rate fields are more realistic, I just do not believe using the non-convergence and numerical instability is an appropriate way to model the process because we are not controlling the nature of the stress concentrators or stress propagation in the model appropriately. The key point is that the ice pack strength is highly heterogeneous and while we do not know the nature of the stress concentrators in the ice pack, they are likely to be more randomly distributed (which non-convergence to the yield curve might be approximating, but is not controlled for). There is a need to understand the nature and distribution of the stress concentrators in sea ice, so we can appropriately model this. And it would really help future sea ice modellers to point out this issue more clearly in papers that investigate LKFs in the viscous-plastic model.

We are afraid that there is a misunderstanding. The results of section 3.2 (3.2.1 in the revised manuscript) do not critically depend on the number of non-linear iterations. The section 3.2 uses less iterations because we wanted to run the idealized experiment for a longer time, but we do not intend to compare the effect of the number of iterations. If we use the same low number of iterations for the experiments in section 3.1, we obtain almost the same shear pattern although the shear lines are not so well defined. We improved the description in the beginning of 3.2 to try to avoid any misunderstandings. We more than agree with the last statement in this comment.

The description of this experiment is now in Section 3.2.1 of the revised manuscript on page 12 and reads *"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 5). The openings are visible in the thickness and concentration fields with thinner, less concentrated ice in the lead. In this longer experiment, the sea ice also ridges, for instance at the center of the domain where the apex of the diamonds fails in compression. There is also some thicker ice at the northern boundary induced by the specified strain rate at the northern boundary. The fracture pattern and presence of secondary and tertiary fracture lines are in line with results from laboratory experiments Schulson (2004) and with AVHRR and RGPS observations. "* .Figure 6 is on page 14 of the revised manuscript

R2#20, Figure 7: Nice illustration of the role of boundary conditions on the stress solution.

We thank the reviewer.

R2#21, Section 3.5: Good illustration. I would suggest you critically look at the stress fields in your previous experiments to identify what the stress concentrators are there. Did you forget to reference figure 9 in this section. Finally, Bill Hibler has shown similar results where embedded fractures of different orientations would join together to form larger scale fracture patterns. Not sure he published that, but I think he did. Look at the papers he wrote with Aksenov and his first anisotropic paper with embedded leads in grid cells. Unfortunately I am on an airplane right now and don't have access to his papers.

A reference to figure 9 is present on line 17 of the original manuscript. We added the reference to Aksenov and Hibler (2001) In the revised manuscript on page 15. Thanks for pointing this out. We do not have stress concentrators in the previous experiments, except for Figure 7b where the no slip southern boundary forces the fracture to take angle solely determined by geometry.

R2#22, page 16 line 1: Here and in other places you confuse the simulation with reality. "This is in contrast with other granular materials". Remove "other", as this is in contrast with granular materials. The VP model is not modelling a granular material. While sea ice may be a granular material, the rheology is designed for different behaviour.

"Corrected as suggested. Thanks for picking this up. We do understand that model and reality are different!.

R2#23, page 16 line 5: "larger that what" → "larger than that"

Corrected as suggested.

R2#24, page 16, line 9: I would like to see the strain-rate field for the longer simulation with e=0.7 where deformation is in convergence. Where in the field is the ridging occurring? What do the intersection angles look like?

Figure 11 shows the fracture for $e = 0.7$ after 5 seconds. We added a figure showing the ice field with e=0.7 for 2700 seconds (45 minutes) o, similar to Figure 6.

R2#25, page 17 line 2: "and individual floes form" could be clarified as "4 separate floes form". **Corrected as suggested.**

R2#26, page 17 line 15: Please clarify the statement "the fracture pattern is very sensitive to coefficient of internal friction. This makes measuring the fracture angle very difficult." Surely the sensitivity will help you differentiate fracture angles. Incidentally mu was not defined in section 2 or here. What causes the spread in the stress state? Is this related to the opening/ridging and subsequent ice strength changes? So the spread in stress state is controlled by the strength parameterisation. I feel this is important to point out, because it is another control we have on the spread of intersection angles you might see under a particular confining stress.

We wanted to express that we observe different behavior depending on the value of μ. We revised the text by rewording this part along a whole paragraph on page 20 of the revised manuscript.

we also added a sentence in section 2.2 defining μ : *"[..] where μ is the slope of the Mohr-Coulomb limbs (Fig. 1), c is the cohesion value (the value of σ_{II} for $\sigma_I = 0$) defined relative to the tensile strength by $c = \mu \cdot T^\star$."*

R2#27, page 17 line 25: Clumsy language: "the stress state touches the yield curve on both parts of the yield curve." Very unclear that you mean the stress state falls on the coulombic limb and the ellipse cap. rephrase.

We modified the whole section describing the Coulombic yield curve experiments, on page 18-19 of the revised manuscript. We hope it clearer now.

R2#28, page 19 top paragraph: This is a matter of opinion. I disagree that the ice pack is characterised by diamond shaped floes. Yes, diamond floes form under certain confining stresses, but is this the most prevalent mode of failure in winter? That needs to be proven. This point does not discount your use of your numerical experiments to differentiate between rheologies, but it does question if an anisotropic rheology based on diamond shaped floes is appropriate for all space and time.

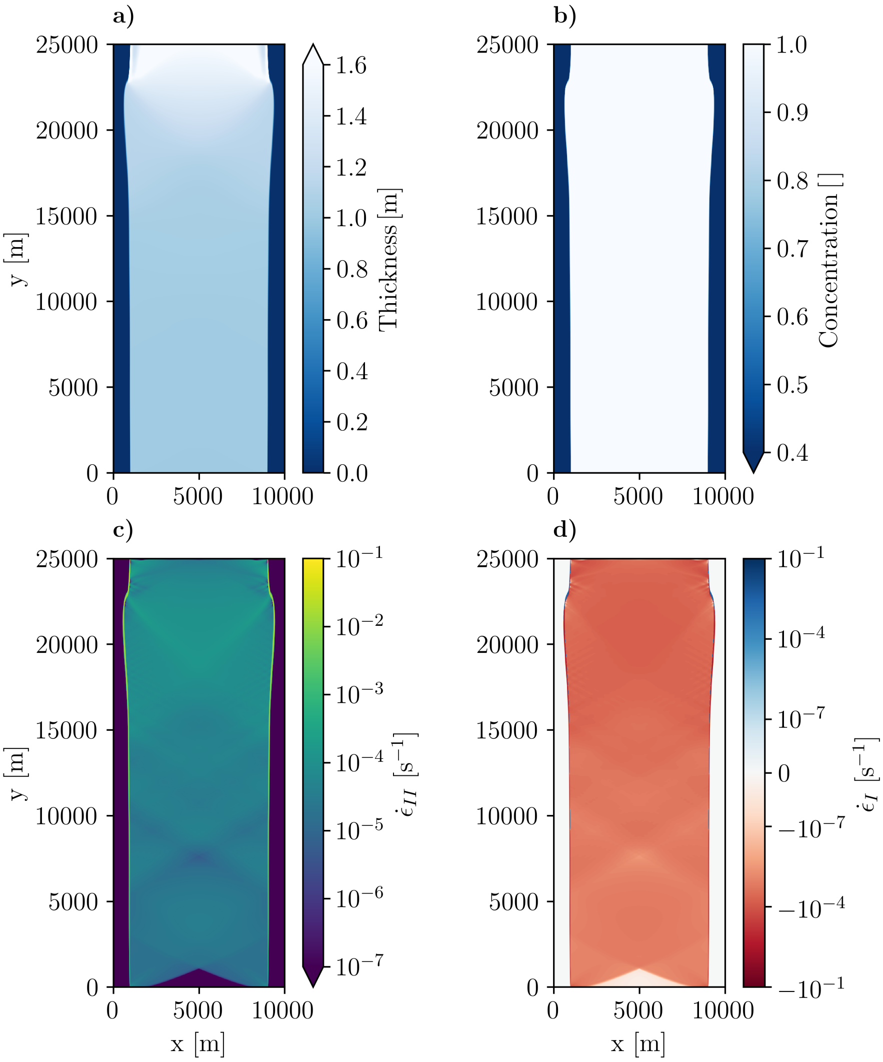

Figure 1: Sea ice thickness (a), concentration (b), maximum shear strain rate (c) and divergence (d) after 45 min of integration (2700 sec) in a uni-axial loading test with an ellipse ratio $e = 0.7$. To make these longer simulations possible, both non-linear and linear iterations are limited to 150 per timestep. Results show that no fracture lines are created, but the ice is pilling close to the northern boundary and the ice got broader without creating open-water.

We clarified this point in the revised manuscript on page 21 of the revised manuscript.

We modified the text on page 20-21 of the revised manuscript to state : *"The Elastic Anisotropic Plastic (EAP) rheology assumes predominately diamond shaped floes in sea ice (Wilchinsky and Feltham, 2006). A sea ice model with EAP creates sharper fractures than a model with the Elastic Viscous Plastic (EVP, Hunke and Dukowicz, 1997) rheology (Heorton et al., 2018). The authors concluded that the anisotropic model may improve the fracturing process for sea ice, especially by creating areas of oriented weaknesses, and particularly at coarse resolution where the fracture is not resolved by the grid spacing. In the experiments presented here, the VP rheologies lead to sharp and anisotropic fracture lines without any additional assumptions."*

R2#29, Also, did you calculate characteristic directions for the VP model to confirm these are controlling the diamond structures in your simulations? I have a code somewhere (from 15 years ago) that does this. If I can find it I can give it to you.

No we did not. But using the theory we described for the Coulombic yield curve in the original manuscript, we can know predict the fracture angle for the elliptical yield curve. This theory is presented in Appendix B of the revised manuscript.

R2#30, page 19 line 12: This sentence is miss-representative: "Thus, the rheology is shown to be scale independent ... in line with observations". Your numerical experiment is set up to ensure the behaviour is scale independent from the scale of the grid size to the domain size. You would really hope your numerical experiment results do not depend on resolution (good practice to check this) and there is no reason scale should change intersection angles for the reasons you have stated previously. Rephrase this statement so someone does not quote it as evidence supporting Schulson's hypothesis.

We modified the sentence on page 21 of the new manuscript to be *"The fracture angles do not depend on the spatial resolution and domain size as expected in our idealized numerical experiment setup (Sect. 3.2.1, Fig. 5)"*

R2#31, page 19, line 20: Just to clarify, the reason the experiments with thin ice change the fracture angle is because the presence of the thin ice modifies the stress state across the domain. So with an ellipse this will change the intersection angle, with a coulombic rheology it would not. I feel this part of the paragraph needs more clarification.

we modified the sentence on page 21 of the revised manuscript :

"The confining pressure (i.e. thin ice imposed on the side of the domain) changes the distribution of stress within the domain. This results in different deformation patterns (shear and divergence) and different fracture angles because the yield curve is convex and uses a normal flow rule."

R2#32, page 19 line 22: Your interpretation of the RGPS data (if one believes the intersection angles are at conjugate pairs and not leads formed at separate times) would lead one to believe that there is not a constant fracture angle independent of confining stress.

The macroscopic angle of friction in a granular material is not constant and depends on the distribution of the contact normal between floes (Balendran and Nemat-Nasser, 1993). A consequence of a variable macroscopic angle of friction is fracture angle that is not constant. This work is beyond the scope of the present paper.

R2#33, page 20 line 2: Perhaps clarify that the Miller et al. (2005) experiments were using metrics of ice thickness, area and velocity to determine the optimal yield curve shape. It is my memory they did not consider the form of the ice strength parameterisation as an alternative to changing shear strength, or yield curve shape, just the eccentricity of the ellipse.

We think that the metrics do not matter in this context. We would like to keep it simple here, because we are only referring to change of e.

We added on page 21 of the revised manuscript *"Arctic-wide simulations improve metrics of sea ice concentration, thickness and velocity by decreasing the value of e of the standard elliptical yield curve, that is, by adding shear and bi-axial tensile and compressive strength (Miller et al., 2005; Ungermann et al., 2017)."*

R2#34, page 20 line 5: Can you show that your numerical experiments are consistent with Pritchard (1988).

Yes, we can. The fracture angle relative to principal stress corresponds to the angle between the principal stress and characteristics directions given by Pritchard (1988) or Wang (2007). We have added an appendix B with a theory explaining the fracture angle of the yield curve using Mohr's circle. Theory that gives the same relation between the slope of the tangent to the yield curve and the fracture angle.

R2#35, page 20 line 8: questions → questioned

Corrected as suggested.

R2#36, After reading your discussion I wondered if the tear drop yield curve (originally proposed by Pritchard) might be more appropriate that the Hibler modified ellipse / mohr coulomb.

We note that the kink in the MC yield curve of Hibler and Schulson (2000) cannot be eliminated by choosing appropriate values of P* and e. Also, both tear drop and ellipical yield curve use the normal flow rule and have a convex yield curve, which gives the non-physical behavior of the fracture angle as a function of shear strength and confining pressure. It is also known that a normal flow rule with "straight limbs" gives too much dilatation when the stress states are lying on the straight limbs (as stated in Flato and Hibler, 1992). For this reason we believe that a Mohr-Coulomb or a tear drop with a non-associated (normal) flow rule (e.g. Tremblay and Mysak, 1997) would be more appropriate. This is the subject of future work.

R2#37, Also, Hibler and others recognise that you must have a closed cap on a coulombic rheology to allow ridging. Perhaps the ellipse is not the best choice for this. In engineering it is more common to have a flatter closure to the yield curve.

The flatter closure actually would lead to other problems, if we look at the framework we develop in the new version of the appendix ??, and also with the theory of the characteristics of the yield in Pritchard (1988). A slope of yield curve higher than $|b'| = 1$ does not have solution for fracture.

R2#38, page 20 line 26: sensible → sensitive

Corrected as suggested.

R2#39, page 20 line 26: Another example where it is not so clear you are talking about the VP rheology problem: "The fracture angles is also sensitive to the surrounding sea ice cover, in contradiction to the granular nature of sea ice". Also, the stress field is going to depend on surrounding ice even when the ice is modelled as granular, which I am not sure is what you were meaning to imply is not true for granular materials. I think you need to clarify the language, and I think I disagree with you that this test is suggesting ice is granular - it would be something we could test in an ice tank to find out what the actual behaviour is though. I feel you do not highlight a key result in the paper: That fracture angles below 30o are not possible with the elliptical rheology, and that this is in direct conflict with observational evidence for smaller fracture angles. Even in light of errors of interpretation of the RGPS intersection angles this result still holds.

Thanks for pointing this out. We have made this result (fracture angle below 30deg not possible with ellipse) more prominent on page 22 of the revised manuscript. If we think of the nature of sea ice, it is a granular material composed of floes. At no or small confinement, the ice dynamic is mainly governed by the floes lateral interaction. The difference with "classic" granular material (sand, clay,...) the ice is a 2D material bounded to float on the ocean. So at high confinement the ice can "escape" in 3D and ridge or raft. We agree that sea ice should have different behavior for high and low confinement. This is the conclusion also reached by Wang (2007) if we look at their Figure 5, even if we disagree with the shape of their yield curve.

The first paragraph of the conclusion have been replace by the 4 following :

In our experimental configuration with uni-axial compression, fracture angles below 30° are not possible in a VP-model with an elliptical yield curve. Observations suggest much lower values. We find an empirical relationship between the fracture angle and the ellipse ratio e of the elliptical yield curve that can be fully explained by the convexity of the yield curve (Appendix B). In contrast to expectations, increasing the maximum shear strength in the sea ice model increases the fracture angle. Along a fracture line, there can be both divergence and convergence depending on the shear strength of the ice, linked to the flow rule. The simulated ice opens and creates leads with an ellipse ratio $e > 1$ (shear strength is smaller than compressive strength), and ridges for $e < 1$ (shear strength is larger than compressive strength).

With a modified Coulombic yield curve, the fracture angle can be decreased to values expected from observations, but the non-differentiable corner points of this yield curve lead to numerical (convergence) issues and, for some values of the coefficient of internal friction μ, to fracture patterns that are difficult to interpret. At these corner points, two different slopes meet and give two non-unique solutions for fracture angles and deformation directions. We recommend to avoid non-differentiable yield curves (with a normal flow rule) in viscous-plastic sea ice models.

More generally, the model produces diamond-shaped fracture patterns. Later the ice floe disintegrates into several smaller floes develop. The fracturing process in the ice floe in our configuration is independent of the experiment resolution and scale, but sensitive to boundary conditions (no-slip or free-slip). The fracture angle in the VP-model is also sensitive to the immediate environment. This is not consistent with the notion of sea ice as a granular material. 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.

R2#40, page 21 line 3: Note that at cusps in a yield curve two possible solutions are possible. I feel you can clarify your point about not using non-differentiable yield curves. They are also numerically unstable. The unclear fracture pattern is not something I have issue with. Perhaps this exists in reality when the stress state can spread across opening and closing modes.

Unclear (or chaotic) fracture pattern could happen in reality when there is an high heterogeneity in ice strength, concentration and thickness, but it shall not happen in a uni-axial compression experiment with uniform ice field.

We added the following sentence on page 22 of the manuscript : *"At these corner points, two different slopes meet and give two non-unique solutions for fracture angles and deformation directions."*

R2#41, page 21 line 10: I feel you can be stronger here in stating that the ellipse with normal flow rule can be discounted as unphysical.

We added a sentence in the revised manuscript:

We modified the text on page 22 of the revised manuscript *"In our experimental configuration with uni-axial compression, fracture angles below 30°*

are not possible in a VP-model with an elliptical yield curve. Observations suggest much lower values. We find an empirical relationship between the fracture angle and the ellipse ratio e ***of the elliptical yield curve that can be fully explained by the convexity of the yield curve (Appendix B)."***

R2#42, page 21 line 11:
Scale is really unimportant in these experiments. You can perform them on any scale. The more important question is what scale do these types of fracture events actually occur on and can that be resolved in models?

As stated in comment R2#30, we agree on the fact that the scale of such idealized experiment is not important. It appeared important to us to show this fact with simulations. The standard VP model is used by the community at various scales and resolution depending on the goal of each particular studies, i.e. paleoclimate studies and sea ice prediction for ships operations, so the fact that the VP rheologies are scale-independent is important to point out. The observations of spatial power-law scaling in sea-ice deformation down shows that the fracture does not have a preferred scale. The scaling behaviour is seen at lengths ranging from basin scale in satellite observations (Marsan et al., 2004) down to 50m in ship radar observations (Oikkonen et al., 2017). At high resolution VP-simulations are able to reproduce this spatial scaling behaviour while underestimating the intermittency in temporal scaling (Hutter et al., 2018). The resolution used in our study is in between the one used in (Hutter et al., 2018) and the lower limit of scales where we observe power-law scaling.

We replaced the aforementioned sentence by *"If Arctic-wide sea ice simulations with a resolution of 25 m are not feasible today because of computational cost, we can still imagine small experiments to be useful for process modeling on small scales when local and high-resolution observations (e.g. wind, ice velocities) are available. For example, such process modeling studies could be used to constrain the rheology with data from the upcoming MOSAiC campaign (Dethloff et al., 2016) that will provide a full year of sea ice observations in pack ice."*

R2#43, I see you did not reference work by K. Wang (2007) who used lead intersection angles to try to estimate the shape of a yield curve. He also has a paper were he performed a similar study to you (Wang and Wang 2010), however for the pan-arctic and perhaps with convergence issues that make his findings hard to interprete. While this work suffered from problems of representativeness of the observational data (how can you be sure fractures formed at the same time), as you do, I feel you should consider Wang's papers in light of your findings.

We now included a discussion of the results and findings of these the first references On page 21 of the revised manuscript. We did not wish to include the second one because of the strong convergence issues.

we added the text *"Based on the results of Pritchard (1988), Wang (2007) used observed fracture patterns to design a* Curved Diamond ***yield curve. But this yield curve also contains a non-differentiable point, which will be problematic for numerical reasons."***

R2#44, Finally, I believe that the stress state between fractures in your numerical experiments is inside the yield curve (viscous), and the motion close to zero. Is this correct, it was what I found when I was working on this. Just a point to clarify that the accumulation of stress along fractures is due to the yield curve discontinuity, and the associated characteristic directions in the strain field that control the propagation of fracture direction. This accumulation of stress needs to be nucleated at a location with high stress gradient (such as a corner on the boundary or strength/stress difference between grid cells). Once the stress reaches the yield curve, the numerical instability is probably put into play during the inner iterations. You do not see LKFs in VP models that have smooth boundaries and strength fields. The formation of LKFs is grid resolution dependent (as the linear instability identified by Gray is). You have speculated on why LKFs form in the VP model only at higher resolutions in a previous paper and I would suggest the place to look is in the convergence of the solver, and the splitting of velocity solution from the ice strength (pressure). I do not think it is just the fact that divergence (and strength reduction) can be greater at higher resolution. Clarifying this mechanism will help readers understand why VP models show this behaviour. It will also hopefully get people thinking about how to represent stress accumulators in the model, because many people using the VP model and studying fractures are unaware of how the model produces these.

Jenny

The stress states outside of the LKFs are effectively inside the yield curve, at the exception of the cells on the border of the ice floe, that move into open-water. We think that the creation of fractures in the model VP is explained by the Mohr's circle and failure envelope theory and we create LKFs in our uniform ice strength field. We have added a new appendix discussing this, Appendix B.

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