Authors answer to Veronique Dansereau comments for tc-2020-153 - Round 2

March 24, 2021

Dear Editor,

You will find below our answers to the referee's comments. This answer document is composed of a main part summarizing the changes made to the manuscript, and a supplement which includes a detailed point-by-point answer to the reviewer.

We thank Véronique Dansereau for her additional comments and thorough review of the revised manuscript. Her comments and suggestions improve our manuscript.

Yours sincerely,

Damien Ringeisen,
On behalf of all three authors.

Main modifications

The comments from the reviewer can be summarized as follows:

1. State clearly which process is modeled here, brittle behavior or granular behavior.
   - We added a statement clarifying that we model sea ice as a granular material, or in other terms, a fractured system. (L35–41)
2. The definition of "rheologies" is not clear and needs to be improved.
   - We have included a clearer definition of the terms *rheological models* and *rheologies*. Specifically, we define the physical behavior as the "*rheological model*" (e.g., Viscous-Plastic (VP)), and "*a rheology*" as a given set of constitutive equations within a rheological model. A VP rheology is defined by the shape of the yield curve and the orientation of the flow rule (i.e., the shape of the plastic potential). (L43–47)
3. State why the viscous-plastic rheological model is suitable for modelling sea ice as a granular material.
   - We added a statement explaining why the VP rheological model is suitable for modeling sea ice as a granular material (L55–60):
     (1) It includes a yield condition for the transition between small quasi-rigid viscous deformations and large plastic deformations.
     (2) It includes a plastic flow rule which allows to represent divergence or convergence along the shear lines, i.e., the dilatancy observed in granular materials.
4. Explain why you would expect an angle different from the Roscoe angles θ_R.
   - We compare different concepts (Coulomb and Roscoe) for the orientation of LKFs with a non-normal flow rule. In previous studies with non-normal flow rules, the effect of the non-normality was not considered. We rephrased two sentences to make this point clearer. (L100–102) and (L224–L226)
5. Add a statement justifying why LKFs intersection angles should not depend on confinement.
   - We added references reporting similar fracture angles at different geographical locations, indicating that the fracture angle may not be influenced by confining pressure. (L116–L117)
6. Make Section 2.1 shorter for your specialized audience.
   - We decided not to shorten Section 2.1 for the sake of completeness and to make the article more accessible to the general reader.
7. State how the angles are evaluated and what is the accuracy.
   - We now specify that the angles are measured manually and that the accuracy is $\pm 1°$. (L301–302)
8. Describe what happens in the first five seconds of the simulation.
   - We include a sentence describing that the fracture is created on the first timestep and develops during the rest of simulation. (L303–307)

Other minor answers and corrections are kept in the supplement.

Supplement – Point-by-point answer

Note:

- The referees comments from the 1st round are shown in black and numerated with **R1** .
- *The authors answers and modifications from the 1st round are shown in italic black.*
- The referee's comments for the 2nd round are shown in red and numerated with **R1.2** .
- The authors answers for the 2nd round are shown in blue, *with the modifications shown in italic.*

We also remove the comments from the 1st round that were not subject to a new comment or answer from the reviewer during the 2nd round.

R1.2#1, Thank you for your response to my comments on your paper. The modifications you have made improve readability. In particular, the abstract is much clearer and the addition of the definition of the different angles makes the reading easier.

You have provided some elements of answer to my main comments in your responses, in particular, to the first comment (R1-2). However, I find that the changes you have made accordingly (in the introduction in particular) are not sufficient to address the point I wanted to make with this comment, which is: you need to state and explain clearly the assumptions behind your work.

In other words, the goal is not to present these assumptions to me, as a reviewer, but to your readers. Therefore, I would strongly suggest that you put the list of arguments you have presented to me in R1-2 in the text (e.g., around page 2, lines 32 to 39), to explain that sea ice present both brittle and granular behaviors, but that here you consider it to be a granular (already fractured) media in the context of shearing band angles at the regional to global scale (i.e., the scale of sea ice models). In think this would really help following your line of thoughts, understand your approach and strengthen the manuscript.

We have restructured the introduction and included new material to address the reviewer's comments – without making it longer. The revised introduction includes the fact that sea ice has granular material properties as well as brittle behavior and that we model sea ice as a granular material.

We rewrite the 1st and 2nd paragraph of the introduction on L35 of the revised manuscript:

"Sea ice plays a significant role in the energy budget of the climate system and therefore has a strong influence on future climate projections. Sea ice dynamics are located primarily along narrow lines of deformation, called Linear Kinematic Features (LKFs), where floes slide along and grind against each other. LKFs can form in divergence, creating stretches of open water or leads, or in convergence, creating piles of ice or ridges. LKFs in the Arctic sea ice cover influence the Earth system in many ways: heat and moisture exchange take place primarily over open water (Badgley, 1965), and salt rejection during ice formation in leads creates dense water and influences the thermohaline circulation (Nguyen et al., 2011, 2012; Itkin et al., 2015). Locally, the ice strength depends on the sea ice state (e.g., thickness, concentration, and damage), which in turn is affected by sea ice fracture with thermodynamic growth in opening leads and with local dynamical growth during ridge formation. One observable and quantifiable feature of LKFs in Arctic sea ice is the intersection angles between individual LKFs. The LKFs have

an influence on the local ice strength, emergent anisotropy and future deformation in the pack ice, and therefore sea ice mass balance (Aksenov and Hibler, 2001). Reproducing the LKFs patterns, density, and orientation is important for accurate sea ice and climate projections at high-resolution.

LKFs are ubiquitous features of granular media, and sea ice is often described as such a granular material (Overland et al., 1998; Erlingsson, 1988; Anderson, 1942; Schall and van Hecke, 2010). Similar to the crumbling of rocks, sea ice also exhibits brittle fracture, as floes break into smaller pieces. Brittle behavior adds a level of complexity because it implies that models must represent both the dynamics of intact ice (brittle — fracture or elastic regime) and the dynamics of a fractured system (granular — friction or plastic regime) (Handin, 1969). The dominant deformation process along LKFs is shear. Sometimes this shear is associated with non-zero divergence, and this divergence along shear bands is referred to a dilatancy (Stern et al., 1995). Granular theory can explain the dilatancy along LKFs. In this work, we consider sea ice as a granular material and focus on the dynamics of the fractured system."

More generally, my point is: your paper addresses an issue that will be of interest for a very specific group of sea ice modellers concerned with the details of its mechanical behavior and numerical representation. These people know what sea ice is, know about VP and will most probably be knowledgeable in mechanics (i.e., on granular vs plastic vs brittle behavior and models). I believe that what they need is to be guided through the physical assumptions that you make to be convinced that your approach is physical and relevant to their modelling.
We decided to keep most of the material in section 2.1 for the sake of completeness and to make the article more accessible to the general reader, as opposed to more specific and targeted at a small audience.

I therefore suggest a "major" revision in the sense that I think some, perhaps locally substantial, changes need to be made to the introduction in particular, but you already have brought up some references and a bullet point list of your arguments to me in your review, so introducing them in the text to support your approach should not be too time consuming. This will likely lengthen the text. Consequently, and in the line of idea of my previous comment (that people who will read your paper to improve their sea ice simulations will probably know VP), I suggest below some cuts to generic elements in section 2.1 that would make it shorter, while still keeping in mind that you wanted to keep a full description of VP.
We remove small parts of Section 2.1 as suggested by the reviewer (See comments R1.2#23–R1.2#39).

There are three more precise points on which I would like to have your comments or answer:

1. one unanswered question: what is the method to evaluate the angle from your simulated fields (i.e., what does the Measure Tool from GIMP, what is the method and the related errors)? Please briefly summarize it in the text.
   The angles are measured manually and the accuracy is $\pm 1°$. We modify a sentence in the revised manuscript to clarify this (See R1.2#19 below).

2. in my point of view, the introduction (around page 2, lines 40-52) still lacks an explanation on why you think VP is an appropriate rheology for a granular media (could be short).
   We now include a sentence clarifying why VP is an appropriate rheology for a granular media
   We add on L55 of the revised manuscript: "*The Viscous-Plastic rheology is an appropriate*

continuum rheology for modelling sea ice as a granular material because it includes (1) a 2D yield condition for plastic deformation defining the internal stress stress for fracture medium starts deforming, and (2) a flow rule that allows to represent the divergent and convergent motion along shear lines, that is, the dilatancy. Continuum plastic flow models are often used in other scientific fields to model granular geo-materials (Vermeer and De Borst, 1984; Mánica et al., 2018)."

3. my question in R1-5 remains unanswered and I have rephrased it below to make it clearer. Here, we compare two concepts for the orientation of shear lines in sea ice VP models with non-normal flow rules (Coulomb and Roscoe). Previous studies did not consider the effect of non-normal flow rules of the orientation of LKFs. We revised the introduction to make this point clearer (See R1.2#9 below).

I have also added some questions and comments about your responses and put some minor comments at the end of this review.

Here are my major comments/concerns :

- **R1#2**, It does not appear clear in the paper what physical process(es) the authors really want to model. In the introduction, it is mentioned that sea ice, both in the pack and the marginal ice zone, is considered as a granular material. No physical justification is offered for this assumption. The rheology used to model this granular material is one of plastic flow, but the authors do not explain how they reconcile their continuum viscous-plastic model with a granular behavior. The aim is apparently to reproduce fracture angles (repeated terminology for the features simulated by their model), but the authors do not explain the link between plastic flow, fracturation and the mechanical behavior of a granular material, which is an already fractured/fragmented material in which contacts and friction dominate. Later, it seems that the authors refer to shear bands in granular materials as if they were associated with the same processes as a fracturing solid. The Coulomb theory is invoked but it is not clear if it is in the context of friction or fracture. There is therefore much confusion throughout the paper as to what the authors consider is the mechanical behavior of sea ice : is it characterized by fracturation? By friction and contacts between already broken up floes? Granular materials like sand are invoked, but is sea ice really assimilated to a sand-like material here? Whatever is assumed, it crucially need to be clarified and all physical concepts untangled throughout the text in a way that makes physical sense.

  - *Sea ice is composed to individual floes that vary in size and thickness along seasons and conditions. Sea ice has often been described as a granular material (Overland et al., 1998; Mcnutt and Overland, 2003; Tremblay and Mysak, 1997). In other fields, granular material has been modeled with continuum plastic flow models, considering both the Coulomb theory or the Roscoe theory (Vermeer and De Borst, 1984; Vermeer, 1990; Balendran and Nemat-Nasser, 1993; Mánica et al., 2018).*
    R1.2#2, Yes indeed.
  - *We think that we need to consider the ice as a granular material if we want to explain divergence along fracture lines (Stern et al., 1995; Bouchat and Tremblay, 2017).*
    R1.2#3, Why? You need to extend on this.

    We observe far-field compressive fractures that create LKFs with opening/closing, i.e., some dilatancy. Granular mechanics can explain these non-zero divergence along LKFs during far-field convergent events.

We add on L39 of the revised manuscript "*The dominant deformation process along LKFs is shear. Sometimes this shear is associated with non-zero divergence, and this divergence along shear bands is referred to as dilatancy (Stern et al., 1995). Granular theory can explain the dilatancy along LKFs.*"

The fact that the elliptical yield curve with normal flow rule (Hibler, 1979) feature compressive states with divergent opening (also when low confinement is applied) (Ringeisen et al., 2019) shows that we can consider granular dynamics to already be present in current VP models. In this manuscript, we investigate a modification of the VP model with elliptical yield curve.

- *We do not consider sea ice to behave like sand, but still as a granular material: a 2D granular material. Sea ice is peculiar in the world of physics, because (1) it is bound to the 2D ocean-atmosphere interface by gravity, but can "escape in the vertical dimension" (page 17, line 389) and ridge when bi-axial compression exceeds a critical threshold. Also ice floes, the "grains" of sea ice, can brake or refreeze. Therefore, sea ice dynamics exhibits a large spectrum behaviors, including characteristic granular dynamics, for example dilatancy, as well as brittle behavior.*
- *The terms referring to brittle behavior, such as fracture angle or fracture lines, might be slightly confusing with the idea of sea ice as a granular material, but we would like to keep them as it is. Here is our reflection:*
  - *If we agree on the fact that sea ice is already a fractured medium, we study the large scale deformation of a compact ice field, process similar to the creation of fracture in continuous solid.*
  - *In that case, it makes little sense to us to make a distinction between fracture and friction. This is well described in the abstract of (Wilchinsky and Feltham, 2011): "Sea ice failure under low-confinement compression is modeled with a linear Coulombic criterion that can describe either fractural failure or frictional granular yield along slip lines." The assemblage breaks and floes interact with one another, which can be seen as the microscopic behavior of friction.*

    R1.2#4, Of course both fracture and friction are present within sea ice. But please note that the Coulomb theory has a very different interpretation for fracture than for friction, although the equations are the same. I made that comment because is a difference that you should be aware of and not mix-up in the text because it brings a lot of confusion.

    We now state in the revised introduction that we model sea ice as granular material (See R1.2#2 above)
  - *Furthermore, the creation of LKFs in sea ice was already associated with breaking behavior (Erlingsson, 1991; Marko and Thomson, 1977), the term fracture is repetitively used (Hutchings et al., 2005; Hibler and Schulson, 2000), as well as the fact sea ice is granular medium (Wilchinsky and Feltham, 2011; Hopkins, 1996).*

    R1.2#5, Which part are you modelling? The "breaking" behavior or the granular regime? I assume it is the granular regime, but please make this distinction in the text (see my my comment above).

    We model the granular regime. We clarify this in the revised introduction (See R1.2#1 above)
  - *Furthermore, for clarity, we would like to keep the same terminology as in the Ringeisen et al. (2019), on which this study is based.*

In order to address these points, we modify the manuscript:

- *"Note, that in this study, we consider sea ice to be of granular nature not only in the marginal ice zone, but also in pack ice, where ice floes are densely packed. For this reason, we can consider the creation of an LKF as a process that involves both fracture and friction (Wilchinsky and Feltham, 2011)." on L33 of the revised manuscript.*
- *We modify the penultimate paragraph of the introduction (see also comment R2#4). It now reads "In this paper, we investigate the effects of a non-normal flow rule on fracture angles. We use the non-normal flow rule as a means of separating the state of stress (at failure) and the post-fracture deformation. To this end, we study the non-normal flow rule in the context of the standard VP rheological model using a similar shape for the plastic potential (i.e., an ellipse) because (1) the ellipse is widely used in the community, and (2) its behavior is well documented (compared to other models), providing a solid basis for comparison. For these two reasons, we use the elliptical yield curve despite the fact that it is not the most appropriate yield curve to model sea ice as a granular material like sea ice. This paper provides a new generalized theoretical framework for any viscous-plastic material with normal or non-normal flow rules. Following Ringeisen et al. (2019), we test the new model in simple uni-axial loading experiments where the relationship between fracture angle and flow-rule can be easily identified."*

- **R1#3**, In the same line of ideas, the authors seem to base their assumption of sea ice being a granular material on observations supporting fracture angles that are independent of confining pressure. It appears that they aim at developing a model that complies with these observations. However, no reference of observations, neither at the lab nor the geophysical scale, is clearly associated with this statement. One can reasonably wonder if making such observation would be possible in the case of sea ice at the geophysical scale: how would it be possible to determine far field stresses and distinguish between unconfined and confined states? Do unconfined compression leading to fracture even occur in circumstances other than an individual ice floe crashing into a coast? References are lacking here to support this assumption of independence of confinement and should crucially be added.

  Concerning the granular matter behavior:

  - *Fracture angles (or orientation of the shear bands) that are independent of the confinement pressure are characteristics of granular material, and lead to the use of the Mohr–Coulomb yield criterion.*
  - *More recent studies showed that shear bands orientations in granular materials increase slightly with confining pressure (Alshibli and Sture, 2000; Han and Drescher, 1993; Desrues and Hammad, 1989, Note that some of these studies show a decrease, but only because they use the complementary angles.). However, this change is very limited: of the order of 5°, with a stress confinement ratio of in the range [0.05-0.5] depending on the confining pressure and the grain size.*

    R1.2#6, Please note that at least Desrues and Hammad, 1989 used sand in their (3D, not 2D) experiments which is very different as a material than sea ice (in terms of the dispersion of grain sizes, friction, 3D vs 2D), hence you should be carefull with the statement that shear band angles in granular material do not vary with confining pressure.

We agree. Unfortunately, to our knowledge, there is no laboratory experiments looking at the deformation of sea ice with different confinement pressure.

- *The magnitude of the change of angle contrasts with the effect of confining pressure with the elliptical yield curve, where a stress-ratio of 0.3 changes the fracture from divergent to convergent and the fracture angle from ca. 34° to 46°.*

Concerning the sea ice behavior:

- *The observations of the same fracture angles at different scale (so probably different stress conditions) by several studies (Erlingsson, 1988; Marko and Thomson, 1977; Cunningham et al., 1994) is an indication that fracture angles might be independent of the stress conditions, i.e. different confining pressures. New datasets of intersection angles from LKFs tracking show that coulombic fracture in the Arctic sea ice shows a predominant angle (Nils Hutter, personal communications)*
- *It is correct that, at high confining pressure, the fracture angle probably changes, especially when sea ice reaches a ridging state. This can be seen with the shape of the yield curve observed in Schulson (2004); Weiss and Schulson (2009). Please see also our answer to Reviewer#̃2 in comment R2#40.*
- *See also our answer to comment R2#39 of Reviewer#̃2.*
- *Finally, we agree that far field stresses are difficult (or close to impossible) to determine, this is why observing the angle of dilatancy along LKFs could be a good metric to improve sea ice models.*

To clarify our manuscript, we make the following modifications:

- *We modify our statement: "...namely that shear band orientations and divergent or convergent motion at the slip lines are a function mainly of the shear strength of the material and orientation of the contact normals (or dilatancy angle), and that the confining pressure has only a limited effect (Alshibli and Sture, 2000; Han and Drescher, 1993; Desrues and Hammad, 1989).", L107 of the revised manuscript.*
- *The sentence on L369 now reads "... unlike laboratory experiments with granular materials (e.g., sand) where the fracture angle is only weakly sensitive to the confining pressure (Han and Drescher, 1993; Desrues and Hammad, 1989; Alshibli and Sture, 2000).".*
- *We modify the following statement: "...A 2D material, such as sea ice, can ridge and "escape to the 3rd dimension" after fracture. Therefore, we expect a change in the fracture angles at large confinement. Laboratory experiments show this behavior and yield stresses in sea ice change above a critical confinement ratio (Golding et al., 2010; Schulson, 2002). It is still not clear whether these results can be extrapolated to the modeling sea ice as a 2D medium at the geophysical scale, although several common features can be found (Schulson, 2002)." L375 of the revised manuscript.*

R1.2#7, Again, my point is that you need to state and explain, **in the text,** the assumptions you make and then refer to the literature supporting your approach. For instance, here, you start by your main statement, "we consider sea ice as a granular material", then, "and as such we consider that shear bands vary weakly with confining pressure", citing the references you give here in your response.

I really believe that this will help the reader understand your thought process **and relate to studies they already know of**.

To make the link between studies on granular media and the behavior of sea ice and to support your assumption, it would be highly relevant to include a figure, e.g., of the predominant angle you say is observed by Nils Hutter. Would that be possible? Or is it the range 20-25 you later cite in your paper from Hutter and Losch 2020? Otherwise, citing what you included here in bullet points ("The observations of the same fracture angles at different scale (so probably different stress conditions) by several studies (Erlingsson, 1988; Marko and Thomson, 1977; Cunningham et al., 1994) is an indication that fracture angles might be independent of the stress conditions, i.e. different confining pressures.", etc) would be a start.

The revised introduction clarifies that we model sea ice as granular material. (See R1.2#1 above).

The distribution of intersection angles is subject two studies currently being written. Therefore, we would prefer to keep this private for now. But citing the aforementioned studies is this context is a good option, we thank the reviewer for the suggestion.

We add in L115 of the revised manuscript "*The fracture angles are similar in different regions of the Arctic with different background stress conditions (Erlingsson, 1988; Marko and Thomson, 1977; Cunningham et al., 1994). This observation supports the hypothesis that the angle of fracture is independent of the confining pressure.*"

- **R1#4**, Also somewhat contradictory is the fact that the authors use an elliptical yield curve and plastic potential to model a material that they consider as a granular. I understand this is perhaps temporary and other criterion will eventually be investigated, but in the meantime, are there examples of granular materials that have been observed to follow this kind of yield curve/flow rule? References of such examples would strengthen the paper.

  - *As the reviewer stated, the use of elliptical yield curve is transitory, but practical for the main goal of this study: that is, studying the effect of a non-normal flow rule on the angles of fractures, and provide an theoretical explanation for this effect.*
  - *We use an elliptical yield curve in this study for 2 reasons: (1) Because it is widely used in the sea ice community, for instance 30 out of 34 sea ice models in GCMs participating in CMIP5 use the standard VP model or a modification thereof (Stroeve et al., 2014), and (2) because the behavior of the elliptical yield curve with normal flow rule in uni-axial compression has been recently investigated (Ringeisen et al., 2019), and we want to isolate the effects of using a non-normal flow rule.*
  - *Elliptical yield curve, like the Von Mises yield curve, are used in material modeling, especially for ductile materials. Although their formulation is different that of in the sea ice models. Granular materials usually use an incompressible formulation, while sea ice needs a non-zero divergence term to represent open water formation and ridging.*

  To clarify our manuscript, we make the following modifications:

- *"We discuss the elliptical yield curve here because it the most commonly used one and its behavior is better documented than any other model in use in the community. This provides a known reference for studying the use of non-associated flow rules. Our goal is to provide a reference for the future development of viscous-plastic rheologies with non-normal flow rules rather than suggest a new VP rheology." on L390 of the revised manuscript.*

R1.2#8, Thank you for this addition. I would modify the sentence for improved clarity as "it is widely used for sea ice and its behavior is better documented than any other yield curve used in the sea ice community" and add "because the behavior of the elliptical yield curve with normal flow rule in uni-axial compression has been recently investigated (Ringeisen et al., 2019), and we want to isolate the effects of using a non-normal flow rule" so that the reader understands that your papers are related (and that you want to use the same terminology).

This is clarified later in the same paragraph. For this reason, we opted to keep this paragraph as it is.

- **R1#5**, Another concern is in the interpretation of the results. A model of plastic flow is used here, not a model of fracture (neither heterogeneities, nor elastic interactions, nor a mechanism representing breakage of bonds or damage is included here). In such model, one expects the simulated macroscopic behavior (that of the ice floe in this case) to coincide with the theory prescribed at the local scale, i.e., the constitutive equation, flow rule, etc. Therefore, as pointed out by Hutchings et al. (2005), if deviations between the simulated angles and the predicted values occurred, they would be indicative of numerical errors. Hence, while it is good to verify that the model does indeed reproduce the Roscoe angle within a small RMS error, doesn't it just show that the numerical scheme of the model works? This point needs to be clarified in the text. It would also be important to mention what method is used to estimate the angles from fields such as the ones shown on figure 6.

  - *In sea ice VP rheology, the angle of fracture is not yet understood. For instance, Roscoe and Coulomb theories gives different angles for the same process. We show here that the flow rule affects the fracture angles, and we explain this influence with a theoretical model, adapted from the Roscoe angle. Similar investigations of the angle of deformation features can be found, for example, in the field of lithosphere geophysical modeling: Lemiale et al. (2008); Kaus (2010).*
  - *The method used to estimate the angles is presented at the end of Sec. 3.*

To clarify our manuscript, we make the following modifications:

  - *We add on L94 of the revised manuscript: "The effects of a non-normal flow rule for sea-ice rheologies (as in e.g., Hibler and Schulson, 2000; Hutchings et al., 2005) on the fracture angles have not been explored. Therefore, it is unknown which of the three theories (Coulomb, Roscoe, Arthur) provide the most accurate prediction for this case."*

- *For comparison and clarity, we add the Coulomb angles predictions on a new version of Fig. 7a, shown below (Figure 1).*

R1.2#9, I will try to formulate my question more concisely : I wonder why, if you prescribe e_G and e_F locally in your model, you do not necessarily expect the macroscopic behavior (in terms of the simulated angle in your rectangular sample) to correspond to your equation 30? What are the reasons why the simulated and theoretical angle could differ, if any?

See my related question below: how does the fracture evolves in your model (in the first 5 seconds of the simulation)?

As we stated before, we compare the results of experiments with two different theories, Roscoe and Coulomb. We show that Roscoe fits the experimental data better. Other papers using a non-normal flow rule do not consider the effect of the flow rule on the angles, only the effect of the yield curve.

Concerning the behavior before 5 s, the fracture is immediately created at the 1st timestep (See R1.2#20 below).

We add the following sentence on L224 of the manuscript, at the beginning of Section 2.3 : "*The Roscoe angles can then be compared to the Coulomb angles, as defined in Ringeisen et al. (2019), and the results from the idealized experiments in Section 4.*".

We rewrite on L100 of the revised manuscript "*So far, only the yield curve has been thought to affect the orientation of LKFs (as in e.g., Hibler and Schulson, 2000; Hutchings et al., 2005; Wang, 2006), and the effects of a non-normal flow rule for sea-ice rheologies on the fracture angles have not been considered.*"

I therefore recommend major reviews to clarify the important points above before a resubmission. More specific comments that are often linked to these major comments are listed below.

Specific comments:

R1#9, Page 1, lines 14-15: "*to make the fracture angle independent of (not on) the confining pressure (as in observations)*". This relates to another of my main comments : what sea ice observations support that fracture angles are independent of the confining pressure? Please give supporting references. Is it even possible to distinguish between fracturing processes occurring in confined and unconfined conditions in the sea ice cover at the geophysical scale?

Please see our answer to the main comment R1#3.

We replace "independent on" by "independent of"

R1.2#10, See my response to R1-3: support for this assumption and references should be included in the text (intro).

We included some references in the introduction that support these assumptions (See R1.2#7 above).

R1#10, Page 1, lines 19-20: "*narrow lines of deformation observed in the Arctic sea ice cover, emerge in high-resolution simulations (Kwok, 2001; Hutchings et al., 2005)*". It would be relevant to cite more up-to-date works on high-resolution simulations here.

The idea is here to cite the seminal studies about LKFs, we are now also citing more recent literature.

We add the following references: (Hutter et al., 2018; Koldunov et al., 2019; Heorton et al., 2018).

R1.2#11, Page 1, line 33: LKFs do not emerge only in high-resolution simulations (e.x., 10, 20, 40, + km is sufficient in NeXtSIM) depending on the rheology used. You should modify this sentence accordingly.

We add a sentence on L64 of the revised manuscript "*LKFs emerge clearly in plastic flow models at high resolution (Hutchings et al., 2005; Hutter et al., 2018; Koldunov et al., 2019). VP models reproduce observed intermittency and spatial localization even without brittle fracture dynamics (Bouchat and Tremblay, 2017; Hutter et al., 2018), albeit at higher resolution than Maxwell-Elasto-Brittle models (e.g., Rampal et al., 2019).*"

R1#12, Page 2, lines 25-27: "*In granular media like sea ice (...) Note, that in this study, we consider sea ice to be granular not only in the marginal ice zone, but also in pack ice, where ice floes are densely packed*". This again one of my major concern: what is the basis for this assumption? How do you reconcile this assumption with the fact that your goal is to reproduce fracture angles in sea ice? Does pack ice, newly-formed ice or any ice that is not yet fractured into floes or constituted of agglomerated, refrozen floes always present the characteristics of a granular media? Please explain and also give some support for this assumption.

We argue that yes, "pack ice, newly-formed ice or any ice that is not yet fractured into floes or constituted of agglomerated, refrozen floes" still carry granular characteristics. The anisotropy at subgrid scale is still present in a way that fracture will rarely be created in straight lines, but will most probably follow the network of weaknesses.

R1.2#12, Agreed, but non-straight fracture lines are not a characteristic of granular material only: they occur in any heterogeneous quasi-brittle material. See my response to R1-3: you need to state clearly that sea ice present both brittle and granular behaviors and in which of these regimes you place your study.

In this paper, we consider the dynamics of sea ice as a fractured system, or granular material (See R1.2#1 above)

R1#14, Page 2, line 37: The brittle model used in (Rampal et al., 2016) is the EB model of Girard et al. (2011). Please modify the reference.

Corrected as suggested by the reviewer.

R1.2#13, Page 2, line 70: The rheology in Rampal et al., 2016 being the same as in Girard et al., 2011, I would remove the reference to Rampal et al., 2016 (repetition).

Corrected as suggested.

R1#15, Page 2, line 39: I believe a simpler and scientifically more objective formulation would be "*most widely used*", instead of "*de facto standard*".

"De facto" means "in fact" or "in effect". We are just stating a fact here.

R1.2#14, Page 2, line 72: I still think that an objective sentence would replace "standard" by "most widely used" (your next sentence supports just that) or de facto by "practically". It is not a fact that the sea ice community has defined a standard rheology :)

We absolutely agree with this last sentence, and this is exactly why "de facto standard" is the good formulation. "*De facto: existing in fact, although perhaps not intended, legal, or accepted*" (Cambridge Dictionary), we mean that VP was not defined as a standard, but grew to be one (i.e., in almost every climate models — for now). This is a Bottom-Up decision, in contrast with a "*de jure standard*", which is a Top-Down decision, when the sea ice community would decide to make a rheology *the standard*. We do not wish here to say that the VP model is the best model, or that it is the one and only model to be used.

R1#17, Page 2, lines 48-49 vs line 50: "*Two classical solutions coexist and set two limit angles for the orientation of fractures: the Coulomb angle (...)*". There is something unclear and contradictory between this and the previous sentence. You invoke the Coulomb theory here, in the context of friction or fracturing? I understand it is the later, but please make that clear by answering my previous comment.

We consider the case of fracture, but this applies also a dense pack of ice floes. We do not understand why these two concepts should be separated. The creation of LKFs in sea ice has been referred to as "fracture" in several preceding publications (e.g., Hutchings et al., 2005).

R1.2#15, The Coulomb theory (originally for friction) has been adapted and extensively used to describe fracturing in brittle materials, but these are two completely different phenomena (friction and fracture) and so is the interpretation of this theory in terms of angles. This is why these two concepts should be separated. See my response to R1-3: you just need to state more clearly in a short sentence what you are describing: shear bands in a granular media or brittle fracturing, so that the reader follows your line of thought.

The revised introduction now states that we describe sea ice as a granular medium (See R1.2#1 above).

R1#23, Page 3, lines 74-76: You state that uni-axial compression experiments showed that (3) the fracture angle is a function of the confining pressure. How did you determine that without performing bi-axial compression experiments? Is there a typo here?

No, this is no typo. Ringeisen et al. (2019) showed that the fracture angles changes with the confining pressure when a elliptical yield curve is used, the forcing was uniaxial but the ice was confined, hence similar to a bi-axial loading.

We modify the text to now read: "In Ringeisen et al. (2019), the confinement was achieved by adding thinner ice on either side of an ice slab subjected to uni-axial loading." on L105

R1.2#16, I see. It would be clearer and shorter if you wrote "compression experiments with uni-axial loading and laterial confinement added via the addition of thinner ice (Ringeisen et al., 2019)" because uni-axial compression experiments with confinement are in fact bi-axial compression experiments.

We rewrote this part.

This paragraph now reads on L109 of the revised manuscript "*In addition, uni-axial loading compression experiments with lateral confinement (achieved via the addition of thinner ice surrounding the ice slab, Ringeisen et al., 2019) showed that:...*"

R1#28, Page 4, line 90: "*In these different classes of models, various rheologies can be defined*". This is not true and/or not clear: these are rheological models and therefore they do not include different rheologies. I think that you mean that these different models require the definition of different components: a constitutive relation (all models), a yield/damage curve/criterion (all models including a threshold mechanism, i.e, a change in mechanical behavior) and a flow rule (only plastic flow models). I therefore suggest to rephrase and clarify this passage and the next sentence, that is "*in a VP rheology, a yield curve and plastic potential (flow rule) must be defined*". In the same line of idea, I do not really see the point of the last sentence of this paragraph. Maybe it can be cut if some rephrasing is made at the beginning of the paragraph?

A VP model with a different yield curve and/or a different flow rule can describe a different physics in the modeled material. A VP rheology with a Mohr-Coulomb yield curve (e.g. Tremblay and Mysak, 1997) will create different results than the one with an elliptical yield curve. The last statement is important for this paper, because it stresses the fact that changing the flow rule changes the system dynamics.

R1.2#17, Again this is not clear: a rheological model has its own rheology, that determines if it is elastic, plastic, viscous, etc. A model with a different yield curve will lead to different results with the same constitutive equation indeed but does not change the relationship stress-deformation. The flow rule problem concern plastic models only. The sentence should therefore read "In these different classes of models, various mechanical components can be defined" or "in the VP sea ice model, various yield curves and flow rules can be defined".

We disagree on this point, although we agree that semantics for sea ice models are globally unclear.:

- The reviewer is right, Viscous-Plastic is the *Rheological Model*. It can be seen as the physical behavior and can be described with basic mechanical elements like Dashpots Elements and Frictional Elements.
- Rheology is wrongly used in most of current sea ice literature, ours included. We could argue that rheology should probably only describe the science of flow and deformation of material. However, the sense of "VP rheologies" as a differentiation between different yield curves in the VP rheological model became dominant, e.g. König Beatty and Holland (2010); Zhang and Rothrock (2005); Ip et al. (1991).
- In some range, this distinction makes sense because changing the yield curve will change the stress-strain relationship. For example, with a Mohr–Coulomb yield curve the ice shear strength always increases as the compression stress increased, this is not the case with the elliptical yield curve.
- Changing the yield curve and/or the plastic potential modifies the formulation of the viscosities η and ζ, hence the constitutive equations. In other words, the stress–strain-rates relationship is changed, because the viscosities depend on the strain-rates as well. The behavior is still visco-plastic, but the constitutive equations are changed.

We rewrite this paragraph starting on L43 of the revised manuscript:
"*Different rheological models assume different material behavior before and after fracture. Common sea ice rheological models are, for example, Viscous-Plastic (VP, Hibler, 1977), Elastic-Plastic (EP, Coon et al., 1974), Elastic-Anisotropic-Plastic (EAP, Tsamados et al., 2013), or Maxwell-Elasto-Brittle (MEB, Dansereau et al., 2016), . In these different rheological models, various stress–strain(-rate) relationships, or constitutive equations, can be defined. In the following, we refer to models with different constitutive equations as different rheologies. We focus on the VP rheological model. A specific VP rheology is defined by a yield curve and plastic potential. The yield curve defines the stress criteria for the transition from small viscous deformations (creep) to the large plastic deformations (friction). The plastic potential determines the ensuing post-fracture deformation, called the flow rule. The flow rule is normal to the plastic potential (Drucker and Prager, 1952). The plastic potential can be independent of, or equal to the yield curve. In the latter case, the flow rule is also normal to the yield curve*

and is called a normal-flow rule or associated flow rule. Several yield curves have been used in sea ice VP models, some with a normal flow rule (Hibler, 1979; Zhang and Rothrock, 2005) and some with a non-normal flow rule (Ip et al., 1991; Tremblay and Mysak, 1997; Hibler and Schulson, 2000; Wang, 2007)."

R1#33, Page 4, line 108: "We consider sea ice as a 2D viscous-plastic material". See my previous major comment: please explain the physical link between this viscous-plastic assumption and that of a granular material.

See our answer to the general comment R1#2

R1.2#18, This comment is not clearly answered in R1-2 and should be included somewhere in the introduction (see my major comment above).

In the revised introduction, we clarify why the sea ice VP rheology is suitable for modeling sea ice as granular material (See R1.2#1 above).

R1#43, Section 4 and figures 6 and 7: How are the angles of the features observed on fields such as shown on figure 6 measured, i.e., estimated? It would be important to mention what method is used.

This is described in Section 3 Experimental setup and numerical scheme, Line 245 to Line 250.

R1.2#19, Please add a short description on **how** the GIMP Measure Tool estimates (automatically or not?) the angle from the simulated fields (method, errors?).

The angles are measured manually.

We modify the L301 of the manuscript: "*The intersection angles between the LKFs are measured manually with the Measure Tool from the GNU Image Manipulation Program (GIMP, version 2.8.16, `gimp.org`). We estimated the accuracy as $\pm 1^\circ$ (Ringeisen et al., 2019).*"

R1#44, Result section, figure 7 and page 15, lines 292 and 306-308: "the theory predicts the fracture angles accurately" and "The results illustrate clearly how the yield curve defines the stress for which the ice will deform, that is, the transition between viscous and plastic deformation, and how the relative shape of the plastic potential with respect to the yield curve defines both the type of deformation (convergence or shear) along the fracture line and the fracture angle. The resulting fracture angles are in excellent agreement with the Roscoe angle predictions (Roscoe, 1970)." There is my major comment about the results. In section 2.3, you describe how the yield curve, flow rule and angles are related in your model. By prescribing the yield curve and plastic potential ellipse ratios, you prescribe locally the angle (Roscoe) of "fractures". Figure 7 shows that at the macro-scale, i.e., the scale of the ice floe you indeed retrieve that angle. What is prescribed at the local scale is what you get at the macro-scale in your model, as expected in a model of plastic flow. Therefore my understanding is that these tests serve to verify that your numerical scheme is OK. Is that the case? To better illustrate that point, it would be relevant to show the (deformation?) fields at different stages of the compression experiment, to illustrate how the features arise in your model.

We show the fracture after 5 seconds of simulation, in order to get the initial fracture and avoid more complex interactions that might create more fractures (see Fig. 6 in (Ringeisen et al., 2019)). Please see our answer to the general comment R1#4

R1.2#20, The sense of my question was : what happens within the first 5 seconds of the simulation? (see my major comment above).

The fracture is created instantly, i.e., at the first timestep. Because the viscosity for the viscous behavior is so large (deformation timescale of 35 years), we do not see the fracture progression.

We add a statement on L303 of the revised manuscript "*Although the forced deformation is very slow, the stresses reach the yield curve already in the first timestep (0.1 s). The fracture is created immediately, but because of the large viscosity of the viscous states with a deformation timescale of approximately 35 years the fracture progression is not visible immediately (Ringeisen et al., 2019). Therefore we show the deformation after 5 s. During these 5 s there is no fundamental change other than the initial deformation becoming clearer.*"

R1#46, Page 15, lines 306-308: "The results illustrate clearly how the yield curve defines the stress for which the ice will deform, that is, the transition between viscous and plastic deformation, and how the relative shape of the plastic potential with respect to the yield curve defines both the type of deformation (convergence or shear) along the fracture line and the fracture angle. The resulting fracture angles are in excellent agreement with the Roscoe angle predictions (Roscoe, 1970)." But you prescribe the yield and plastic potential in your model: why would you not expect what you get to indeed be what you prescribe? In other words, you do not make any distinction between what you prescribe at the micro-scale (scale of your discretization) in your model and your macroscale results and you do not discuss why you expect these behavior to be identical or not : that is missing from your work and interpretation of your continuum model.

See our answer to general comment R1#5

R1.2#21, Please see my major comment above.

We compared two different concept of the angles of fracture because it is not clear which one determines the angles of fracture in a sea ice VP model with non-normal flow rule. We modified the introduction to clarify this point (See R1.2#9).

R1#48, Page 17, line 382: "sea ice mechanical strength properties (yield curve) and deformation (flow rule)". Again, you write this with the perspective of a VP model, but mechanical strength properties and deformation are not only determined by the yield criterion and flow rules in other rheological models for sea ice. Please be specific and make this distinction clear. Also, I do not understand why Dansereau et al. (2016) is cited in this context.

We refer to Dansereau et al. (2016) in this context because the way the damage parameters act as the history of the model deformation is very interesting, and could be a representation of the state of the local ice (broken/unbroken), i.e. "sea ice mechanical strength properties (yield curve)" cited before.

We reformulate the sentence on L450 of the revised manuscript "...; the sea ice mechanical strength properties (i.e., yield curve) and deformation (i.e., flow rule for VP rheologies) should vary in time and space depending on, for example, the time-varying distribution of the contact normals, floe size distributions, or a damage parameter, as per observations and laboratory or numerical experiments (Overland et al., 1998; Hutter et al., 2019; Horvat and Tziperman, 2017; Roach et al., 2018; Balendran and Nemat-Nasser, 1993; Dansereau et al., 2016; Plante et al., 2020)"

R1.2#22, I see your point, but the numerical experiments in Dansereau et al., 2016 does not show that mechanical properties involved in the damage criteria should depend on the damage itself. Instead this dependency was not added in the model because of lack of agreement in (e.g., experimental) supports, hence my reaction to this sentence. Maybe this sentence is just not clear and should be rephrased?

I see your point, we rephrase this sentence. We do not point here to the idea of a mechanical properties being involved in the damage criteria, but only to the concept of damage for sea ice modelling.

We reorganize the mentioned sentence on L463 of the revised manuscript "*. . . ; the sea ice mechanical strength properties (i.e., yield curve) and deformation (i.e., flow rule for VP rheologies) should vary in time and space depending on additional variables or parameterizations, for example, the time-varying distribution of the contact normals (Balendran and Nemat-Nasser, 1993), floe size distributions (Horvat and Tziperman, 2017; Roach et al., 2018), or a damage parameter (Dansereau et al., 2016; Plante et al., 2020), as per observations and laboratory or numerical experiments (Overland et al., 1998; Hutter et al., 2019).*"

Other minor comments

R1.2#23, Page 1, line 32: "In granular media like sea ice"... then "Note that in this study, we consider sea ice to be of granular nature". See my response to your answer to my major comment above. You should first state that you make the assumption that sea ice is mostly of granular nature and give some references supporting this assumption. Hence reverse the two first sentences here. And then next sentence : "For this reason, we can consider here..."

We agree that this could be clearer and its arguments better connected. We rewrite this paragraph to describe the granular properties and the brittle behavior of sea ice and state that we model sea ice as a granular material (See R1.2#1 above).

R1.2#24, Page 2, lines 40-42: To avoid repetition: "Other models represent sea ice (. . .)" and then skip VP as an example.

The whole paragraph was rewriten and now avoid repetitions (See R1.2#17 above).

R1.2#25, Page 2, line 43: "In these different classes of models, various rheologies can be specified". ? This sentence still does not make sense. I think it could be just removed without impacting the text.

The whole paragraph was rewritten to clarify the terms *rheological models* and *rheology*, and the fact that (See R1.2#17 above).

R1.2#26, Page 2, line 44: "The yield curve defines the stress criteria for the transition from small viscous deformations to large plastic deformations". The deformations are not necessery small or large so I would remove these adjectives. Also, I would add at the beginning "In the VP sea ice model" so that the reader understand that this is inverted compared to standard visco-plastic rheologies (see my previous comment about this). Another solution is to move the sentence on page 2, lines 53 to 55 here to make this distinction clear.

The deformations are small from viscous behaviour (because $\dot{\epsilon}$ is small, resulting in small deformations), and deformations are large with the plastic behaviour ($\dot{\epsilon}$ is large, resulting in large deformation). Concerning the second point, this paragraph was rewritten.

R1.2#27, Page 2, lines 49-50: "It is important to note that two PLASTIC models. . . "

Corrected as suggested

R1.2#28, Page 3, lines 90-91: Again, and in agreement to your response to my main comment, I would not focus too much on sand and would remove this sentence, which is I believe just an example of the previous one.

We do not agree, this an important information to specify that δ and ϕ can be different, and have been measured to be different.

R1.2#29, Page 4, line 93: “The theory” change to “the concept”?

Corrected as suggested

R1.2#30, Page 4, lines 112-113: “in contrast to observing stress which requires in-situ measurements”. This comparison is not really relevant here or it would need a longer description. I think it could simply be cut to make the text shorter.

We think that this relevant and keep this short statement.

R1.2#31, Page 4, line 122: “for comparision” add with previous simulations.

Corrected as suggested.

R1.2#32, Page 4, line 123: “sea ice as a granular material like sea ice”.

Corrected as suggested.

R1.2#33, Page 4, line 125: “uni-axial compression experiments VP simulations”.

Corrected as suggested.

R1.2#34, Page 5, line 141: “In an ideal plastic model, the stresses are independent of the strain rates”. This part of the sentence could be cut (VP is by definition not an ideal plastic but a viscous-plastic model so this is implicit).

Corrected as suggested

R1.2#35, Page 5, line 152: “Some other state variables are a function of P ; for instance, the tensile strength T is usually defined as T = k t · P , where the tensile factor k t ¿ 0 (König Beatty and Holland, 2010). Others are not, such as the ellipse aspect ratio (Hibler, 1979) or the internal angle of friction (Ip et al., 1991)” These sentences is not directly relevant to what you do in your paper and could be cut.

Corrected as suggested

R1.2#36, Page 5, line 154: “For two-dimensional sea ice, stress is a rank two tensor; thus, it has four components.” This is very generic and can be cut: if you have mentioned that you consider sea ice to be 2D in the intro it is already implied.

Corrected as suggested

R1.2#37, Page 6, line 158: “The yield curve can be represented in principal stress (σ_1 and σ_2) or stress invariants space (σ_{I} and σ_{II}).” Again, this can be cut.

This is necessary, this is the first time in the paper that the principal stress (σ_1 and σ_2) and the stress invariants space (σ_{I} and σ_{II}) are defined. In order for the paper to be self-sufficient, this sentence is kept.

R1.2#38, Page 7, line 190-191: "and VP rheologies can be considered as ideal plastic". Please make the distinction! only the (converged) VP model for sea ice could be considered as ideal plastic, on the time scales relevant for sea ice modelling, not standard visco-plastic rheologies (like I said, the viscous vs plastic behavior is inverted with respect to sea ice VP).

Corrected as suggested

R1.2#39, Page 12, line 294: "The angle of each fracture lines".

Corrected as suggested

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