Review, round 2 (reviewer 2)

Note bis:
- The referees comments are shown in black.
- *The authors answers are shown in italic.*
- *The proposed modifications for the manuscript are shown in italic as well.*
- The referee's new comments are shown in red.

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 (eg., 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 strenghten the manuscript.

More generally, my point is: your paper adresses 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.

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 lenghten 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.

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.

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).

3) my question in R1-5 remains unanswered and I have rephrased it below to make it clearer.

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

R1#1, This paper presents an implementation of a non-normal plastic flow rule in a Viscous-Plastic model with the goal of better representing the observed angles between Linear Kinematic Features in sea ice at the geophysical scale. The paper is overall well written, in a pedagogical way for the theory (section 2) section, which could however be a little more concise in some places. The figures are, for the most, clear.

We thanks the reviewer for her thorough review of our manuscript. Her comments will improve the clarity and quality of our manuscript. We hope that we address all comments in a satisfactory fashion.

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 needs 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).

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).

Why? You need to extend on this.

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 fahilure 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.

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.

∗ 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).

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).

∗ 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, the following sentences were added:

• "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. Because sea ice floes are densely packed, we can 2consider the creation of an LKFs as a fracture process with both fracture and friction (Wilchinsky and Feltham, 2011)." L26 of the original 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 and its use as a means of separating the state of stress (at failure) and the post-fracture deformation. The novelty of this paper is that 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) it is widely used in the community, and (2) its behavior is well documented (compared to other models), providing a solid basis for comparison. This paper provides a new generalized theoretical framework for developing any viscous-plastic material with normal or non-normal flow rules. To this end, 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.

Please note that at least *Desrues and Hammad, 1989* used sand in their (3D, not 2D) experiments which is very different asa 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.

• 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; 3Cunningham 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/convergent motion at the slip lines are a function mainly of the shear strength of the material and orientation of the contact normals (or dilatation angle), and that the confining pressure has only limited impact (Alshibli and Sture, 2000; Han and Drescher, 1993; Desrues and Hammad, 1989)", L79 of the original manuscript.
• The sentence on L321 would now reads "... unlike laboratory experiments with granular materials (e.g. sand) where the fracture angle is weakly sensitive to the confining pressure (Han and Drescher, 1993; Desrues and Hammad, 1989; Alshibli and Sture, 2000).".
• We add 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, when the ice is susceptible to ridging (Schulson et al., 2006)." L325 of the original manuscript.

Again, my point is that you need to state and explain, **in the text,** the assumptions you make and then refer to the litterature 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, eg., of the predominant angle you say is observed by Niels Hutter. Would that be possible? Or is it the range 20-25 you later cite in your paper from Hutter and Losh 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.

• 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 use the elliptical yield curve because it is widely used and its behavior better documented than any other models in use in the community. This provides a known reference to study the use of non-associative flow rule. We do no aim to propose here a new VP rheology here but to study the effect of the non-normal flow rule as it could be used in future rheology." on L335 of the original manuscript.

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).

• 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 L69 of the original manuscript: "In the case of a non-normal flow rule, it is unclear which of the three theory (Coulomb, Roscoe, Arthur) predicts the modeled angle of fracture."
• For comparison and clarity, we add the Coulomb angles predictions on a new version of Fig. 7a, shown below (Figure 1).
5a)
e F = 0.7
e F = 1.0
e F = 2.0
e F = 4.0
e G = e F
Roscoe
Coulomb

80
70
60
50
40
30
20
10
0
0
0.5
1
1.5 2 2.5 3 3.5 4 4.5
Plastic potential ellipse ratio e G
5
Figure 1: The new Fig. 7a Caption: (a) Fracture angles as function of the plastic potential ellipse ratio e G for different yield curve ellipse ratios (e F = 0.7, 1.0, 2.0, and 4.0). The markers with ranges are the mean and two standard deviations of the fracture angles. The dashed lines show the prediction from the Roscoe angle (Eq. 28). The arrows indicate the angles predicted by Coulomb theory, which are constant with respect to e G . Colors indicate the value of e F for lines and markers. The R 2 between theory and modeled angles for e F = 0.7, 2.0, and 4.0 are 0.97, 0.95, and 0.97.

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)?

• R1#6, Finally, I find that a discussion of previous studies that have presented similar interests and analyses is lacking from the discussion. Hibler and Schulson (2000) have indeed implemented a non-normal flow rule in the VP model, using a Mohr-Coulomb yield curve with an elliptical cap ("modified Coulombic" curve). They have also found that a non-normal flow rule affects the orientation of deformation features in the VP rheology. This work is cited in the discussion section, but not really discussed in terms of the differences or similarities between both approaches, nor in terms of the advances of the present study compared to this previous one. I suggest clearly stating that is new here and what is the broad relevance of the results. The model of Hibler and Schulson (2000) has also been used by Hutchings et al. (2005) who have looked at intersection angles. They have compared simulated angles between the modified Coulombic and the elliptical yield curve. Mentioning these previous results and comparing them with the current study would be interesting and would strengthen the literature review and Discussion part of the paper.

• Hibler and Schulson (2000) effectively used a yield curve with a non-normal yield curve. Nevertheless, they link the fracture angles to the slope of the Mohr–Coulomb limbs of the yield curve (μ), and not to the orientation of the flow rule. Also, they did not show an actual fracture creation at high-resolution.
• Hutchings et al. (2005) investigated the fracture angles with the modified Coulombic but did not explain the variations of the fracture angles, and only explained that the difference between theory and experiments comes from numerical convergence.
• In Ringeisen et al. (2019), we also investigated a modified version of the modified Coulombic yield curve.
• An investigation of Mohr–Coulomb yield curve with non-normal flow rule (Ip et al., 61991) in a similar setup is underway, but lies outside of the focus of this work.
To improve our manuscript, we make the following modifications:
• "Previous studies with a non-normal flow rule (e.g., Hibler and Schulson, 2000; Hutchings et al., 2005) did not explore the effect of a non-normal flow rule on the fracture angles." on L97 of the original manuscript.
• "According to Hibler and Schulson (2000), the flow rule may have an effect on the angle of fracture, but the authors limited their case to the framework of flawed ice and did not consider Roscoe's theory of dilatancy. The rheology of Hibler and Schulson (2000) was tested in an

idealized experiment more complex than ours (Hutchings et al., 2005), but the effect of using a non-normal flow rule was not explored. The complexity of their setup may explain the observed difference between simulated and predicted angles. Note that the rheology in Hibler and Schulson (2000) was built by changing the shape of the yield curve a-posteriori, while the rheology presented here solves the constitutive equations rigorously." on L361 of the original manuscript.
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#7, Page 1, lines 8-9: "A newly adapted theory (...) predicts numerical simulations of the fracture angles (...) with a root-mean-square error below 1.3 degrees." This formulation is unclear and needs rephrasing: a newly adapted theory is implemented in the VP model and leads to prediction of the prescribed fracture angle with a RMS error below 1.3 degrees"?. Also, see my main comment about the agreement of the theory with your modeled angles.

We rewrite the abstract. Also, see our answer to the the main comment R1#5. The new abstract reads " The standard viscous-plastic (VP) sea ice model with an elliptical yield curve and a normal flow rule has at least two issues. First, it does not simulate fracture angles below 30 ∘ in uni-axial compression leading to a stark contrast with observations of Linear Kinematic Features (LKFs) in the Arctic Ocean. Second, the tight coupling between the fracture angle, post-fracture deformation, and the shape of the yield curve was identified as the reason for this behavior. In this paper, these issues are addressed by removing the normality constraint on the flow rule in the standard VP model in a uni-axial compressive loading setup. To this end, an elliptical plastic potential – that defines the post-fracture deformations, or flow rule – is defined independently of the elliptical yield curve. As a consequence, the post-fracture behavior is decoupled from the mechanical strength properties of the ice. In a newly adapted theory – based on one developed from observations of granular material – the fracture angles de-pend on both yield curve and plastic potential parameters. This theory predicts fracture angles well below 30 ∘ . Numerical experiments confirm that the flow rule details determine the fracture angle. For instance, a plastic potential with an 7ellipse aspect ratio smaller than two (i.e., the value of the standard ellipse) gives fracture angles as low as 22 ∘ . Implementing an elliptical plastic potential in the standard VP sea ice model requires only small modifications to the code. The model dynamics with the modified rheology, however, are more difficult to solve numerically. An independent plastic potential provides a solution to two issues with the standard VP rheology: it allows for smaller fracture angles that fall within the range of satellite observations, and it decouples the angle of fracture and post-fracture deformation from the shape of the yield curve. The orienta-tion of the post-fracture deformation along the fracture lines (convergence and divergence) is controlled by the shape of the plastic potential. An orientation that is different from the standard VP rheology requires a non-elliptical plastic potential. "

R1#8, Page 1, line 11: I suggest dropping "In conclusion" from your abstract.
Corrected as suggested

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

R1#10, Page 1, lines 19-20: “narrow lines of deformation observed in the Arctic sea icecover, 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).
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.

R1#11, Page 2, line 23: “The ice strength locally depends on the ice thickness”. This is only partially true: local ice strength does not depend only on local ice thickness. This sentence perhaps needs some rephrasing.
Corrected as suggested
The sentence in the revised manuscript now reads: “Locally, the ice strength depends on the sea ice state (thickness, concentration, . . .), which in turn . . . ”

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 8straight lines, but will most probably follow the network of weaknesses.
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.

R1#13, Page 2, line 28: “This anisotropy”. This is unclear. Please define this anisotropy and better explain how it emerges.
We modified the text: “The intersection angles between the LKFs have an influence on the deformation field and, hence, on the local sea ice strength and the emergent sea ice anisotropy (Aksenov and Hibler, 2001). This anisotropy, which emerges as sea ice develops weak and strong areas along LKFs as leads or ridges form locally, then influences . . . ”

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.
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).

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.
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 :)

R1#16, Page 2, lines 48-49: Yes, granular media indeed present shear bands, which are not the same as fractures. Again, please clarify what you want to represent in your model. What is the link between LKFs in sea ice, shear bands in granular media and fractures in solid materials?
See our answer to the comment R1#2.

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).

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.

R1#18, Page 3, line 56: I think it would be relevant to make some space and re-introduce the definition of the dilatancy angle here: it would make life easier for the reader and avoid the need to dig for it in another article.

Added as suggested

We add the following sentences "Dilatancy refers to divergence along along shear bands or LKFs that is a function of the distribution of contact normals between individual floes at the sub-grid scales. A positive angle of dilatancy is associated contact normals that (on average) opposes the macroscopic shear motion and divergence along the shear band; while negative dilatancy is associated with a closing of the fracture line and ridging " on L55 of the original manuscript.

R1#19, Page 3, line 58: "A general theory derived from experiments with sand that takes into account both the angle of friction (...)". In the case of sand, contact and friction are indeed at play and shear bands are formed. This again adds to the confusion: internal angle of friction or angle of friction? i.e., fracture or friction? Please clarify.

Please see our answer to the major comment R1#2

R1#20, Page 3, line 60: based on the grain size.

Corrected as suggested

R1#21, Page 3, lines 67-68: "a larger dilatancy angle implies a larger grain size, more contact normals, hence more friction". Can you please include some references that support this?

We add a citation.

We now refer to Vermeer (1990) at the end of the cited sentence.

R1#22, Page 3, line 73: There is a mistake here, as Weiss and Schulson (2009) reported observed fracture angles between 20 and 50 degrees. Or did you derive this directly from their estimated internal friction angle, which is fit to in-situ stress measurements? In the later case, this is then not an observation of fracture angles but a derivation based on some physical assumptions, which are moreover debatable (see Dansereau et al. (2019) and many others), and it should be removed from the list of observations of fracture angles.

Corrected as suggested.

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: "the fracture angle is a function of the con-fining pressure. The confinement was achieved by adding thinner ice on either side of an ice slab subjected to uni-axial loading."

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.

R1#24, Page 3, line 75: the ''gradient" of shear to compressive strength. Did you mean the ratio?

The fracture angles does not depend on the ratio, but the slope of the tangent to the yield curve (Ringeisen et al., 2019; Pritchard, 1988). This slope determines where the ice will break on the Mohr's circle of stress, i.e., the fracture angle.

We changed the sentence to: ". . . the angle of fracture is a function of the gradient of shear strength with respect to compressive strength (i.e. the slope of the yield curve) . . . "

R1#25, Page 3, line 76-79: See again my major comment about the apparent confusion between fracturing, friction, granular media, sea ice and a viscous-plastic continuum rheology.
I think it is crucial to clarify the links you make between these processes and the motivation of your approach here. This passage in particular leads the reader to believe that your goal is that the VP rheology complies with observations of granular media behavior, because you consider that sea ice at the geophysical scale, in all its different states, is a granular media. If this assumption is at the very basis of your approach, it should be stated earlier in the introduction, (very importantly) along with supporting arguments. This would make the reading and the assessment of your assumptions and methods by the reader much easier.
See our answer to the comment R1#2.

R1#26, Page 3, line 82: "The ratio of shear to divergence along the LKFs allows to infer the dilatancy angle." Again, if one assumes sea ice in any state behaves as a granular material.
We clarify this in the revised manuscript. It is important to note that dilatancy (dilatancy can be positive or negative) in leads is a known fact. If most of the deformation happens in
shear, LKFs play a predominant role in thick ice formation (ridging) as well as in thin ice formation
"The ratio of shear to divergence along the LKFs allows to infer the dilatancy
angle when considering sea ice as a granular material."

R1#27, Page 3, lines 86-87: "Separating the link between the fracture angle and the flow rule from the yield curve is necessary to design rheologies that are consistent with observed sea ice deformations". Please note that this would be only true for plastic flow rheologies and not applicable nor necessary for rheologies based on elasticity (EB, MEB, Elastic-Decohesive). To be objective, this statement should therefore be modified as "necessary to design plastic flow rheologies that are consistent (...)".
We correct as suggested.
the sentence in the revised manuscript now reads ". . . to design VP rheologies
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 be-havior) 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.
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''.

R1#29, Page 4, lines 96-97: See my major comment above. Hibler and Schulson (2000) have indeed used a VP model with a non-normal flow rule and a Mohr-Coulomb yield curve with elliptical cap, or "modified Coulombic" curve, as cited in your Discussion section. This model has also been used by (Hutchings et al., 2005) (https://doi.org/10.1175/MWR3045.1) who have looked at intersection angles and compared them between the modified Coulombic and the elliptical yield curve. As their approach is therefore close to yours, it would be important and certainly interesting to explain the similarities and difference between your work and theirs in the literature review (introduction) section. Please also note that Hibler and Schulson (2000) do not seem to share your view that the angles of fracture in sea ice at the geophysical scale are independent of confinement, which would be an important point to discuss further.
See our answer to the major comment R1#6.

R1#30, Page 4, line 100: "viscous-plastic materials" or "a viscous-plastic material ",
"with any flow rules".

Corrected as suggested

R1#31, Page 4, line 100: “from the yield curve”.
Corrected as suggested

R1#32, Page 4, lines 101-102: “The new model is tested in simple uni-axial loading experiments”. See my major comment above: a quick addition to your work would be to test if your numerical implementation also holds under bi-axial loading conditions, that is, if the angles vary or not with confinement.
See our answer to the general comment R1#3 as well as comment R2#2 and R2#39 from Reviewer #2

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

R1#34, Page 4, line 113: In your case, the constitutive equation links the vertically integrated stress tensor to the deformation rate, which you introduced on the previous line.
Yes exactly. For clarity, we prefer repeating “stress tensor ”. However the term “rate” with “deformation tensor ” was missing.
We modify as suggested: “The constitutive equations link the vertically integrated stress tensor σ to the strain rate tensor #”

R1#35, Page 4, lines 117-119: Representing small deformations with a viscous model is rather counter-intuitive, especially for a reader that is familiar with viscous-plastic rheologies (plastic for small, viscous for large deformations). I believe it is important that you explain in more details how a viscous rheology is expected here to represent the small deformations of a solid (time scales, viscosities, etc).
Effectively, this VP models differs from other Viscous-Plastic models, e.g. Bingham plastic, which include a yield condition (rigid solid) and then deforms as a viscous plastic with a linear relationship between viscosity and strain. We add more details to our description of viscous behavior in the last paragraph of Sec. 2.1, on L155
We add the following text on line 157 of the original manuscript “VP sea-ice models typically cap the viscosity at
ζ max =
#
#
P
$= 2.5 \times 10$ 8 s · P
2Δ min
12and η max = ζ max
to regularize the momentum equations. When this regulariza-
e 2 G
tion is in effect, ζ and η are independent of the deformation field (Δ) and the stress divergence reduces to harmonic viscosity with constant coefficients.
Δ min = 2 × 10 −9 s −1 (Hibler, 1979, 1977) translates to a deformation time scale of almost 16 years. Therefore, viscous deformations are slow and negligible with respect to the plastic deformations, and VP rheologies are almost purely plastic. The viscous behavior is a consequence of regularizing the viscosities rather than an implementation of a physical behavior.”

R1#36, Page 5, line 130 to page 6, line 149: These paragraphs could be shortened by removing or presenting in a more concise manner some general pieces of information.
We would like to keep it in the present form because we think it is a useful description of VP rheology.

R1#37, Page 5, lines 130–131: As it is not the states of stress that are deforming plastically, but the material, this sentence needs some reformulation.
Corrected as suggested by the reviewer.

"The yield curve represents the stress states for which sea ice deforms plas-
tically while enclosing the stress states for slow viscous deformation."

R1#38, Page 9, line 204: "The slope of the yield curve". And many other missing "the" throughout the text.
Corrected as suggested. We thank the reviewer for pointing all these out to us.

R1#39, Page 10, line 223: How does the no-slip condition at the bottom boundary affect your results compared to the case in which slip is allowed in the x-direction (i.e., by holding only one of the two bottom corners of the domain fixed in x and y)? Such boundary conditions are maybe less representative of a floe that sticks to a coast but would not lead to as much concentration of stresses on the bottom corners of your ice floe (here your Bcs imply some bi-axial compression at the bottom) and hence would put less constraint on the appearance of conjugate faults and on their orientation. I think this would be an interesting and not time-consuming test.
In Ringeisen et al. (2019), we already investigated the effect of the no- and free-slip condition, and we showed that the configuration used here does not influence the angle of fracture, as indicated on L248 on the original manuscript.

R1#40, Page 11, line 240: I suggest "more numerically challenging".
Corrected as suggested.

R1#41, Page 11, line 256: ''laboratory experiments". If you compare your results with laboratory experiments, please provide more details on these experiments (e.g., boundary conditions? biaxial or uni-axial compression? on samples with an aspect ratio similar to sea ice, i.e., virtually 2D? on fresh or sea ice?) Were such experiments made by Erlingsson (1988) and Wilchinsky et al. (2010)?
Corrected as suggested by the reviewer.
In the corrected manuscript, this sentence now reads: "The fractures forma diamond shape, similar to the shapes observed at large scales (Erlingsson, 1988), in laboratory experiments (Schulson, 2001), and modeled with DEM mod-els (Wilchinsky et al., 2010) or other continuous sea ice models (Ringeisen et al., 2019; Heorton et al., 2018)."

R1#42, Pages 11-13 and caption of figure 6: What is the field represented in figure 6? I assume from the color scale that it is a deformation rate?
The field shown here is the shear deformation # ˙ II .
We clarify this in the caption: "Diamond-shaped fracture pattern in the shear deformation field # ˙ II for e F = 2.0 and three different values of e G after five seconds of simulation."

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

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

R1#45, Page 15, line 300: ''the shape of the plastic potential".
Corrected as suggested.

Page 15, line 305: "this allows decoupling the mechanical strength properties of thematerial (ice) from its post-fracture behavior". Again the contradiction with the assumption of a granular material, i.e., an already fractured/fragmented material. How do you reconcile these ideas?
See our answer to general comment R1#2

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 andplastic 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
Please see my major comment above.

R1#47, Page 15, point 2: About confinement, shear bands and fractures, see my major comment above.
As for the other comments raised about relationship between fracture angles and confinement (R1#3, R2#2), this behavior is linked to the elliptical nature of the yield curve. We add a reference to our study showing how the confinement changes the fracture angles with an elliptical yield curve: "This behavior cannot be eliminated with an elliptical plastic potential, as the normal stress along the LKFs increases with confining pressure and the flow rule changes from divergence to convergence (Ringeisen et al., 2019)."

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 L382 of the 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 the 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)."
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 dependancy 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?

R1#49, Page 17, lines 387-388: "So is the combined knowledge of the failure stresses and their associated deformation of sea ice as a 2D granular material". This is confusing: why then do you base your approach on the assumption of a granular material? This goes along my main comment and really needs to be clarified.
If deformation data are available from satellite observations, we still have little knowledge about the stress associated to these observations. This is especially true when these deformation lead to ridging and creation of open-water. Also, most of the laboratory data investigate 3D continuous ice, we are not quite sure if these results can be extrapolated to sea ice, i.e. we are 15missing knowledge about 2D fractured materials behavior. See also our answer to comment R1#2.
We reformulate". . . higher temporal resolution of sea ice deformation and flow size distributions is still unavailable. There is also a knowledge gap in the inter-

play between yield stresses and the post-fracture deformation in a 2D granular material such as sea ice. This interplay is likely different than for the well studied case of a solid homogeneous 3D block of ice (e.g. Schulson, 2002)." on L387 of the original manuscript.

Other minor comments

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...''

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

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.

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.

Page 2, lines 49-50: ''It is important to note that two PLASTIC models ...''

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 beleive just an example of the previous one.

Page 4, line 93: ''The theory'' change to ''the concept''?

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.

Page 4, line 122: ''for comparision'' add *with previous simulations*.

Page 4, line 123: ''sea ice as a granular material ~~like sea ice~~''.

Page 4, line 125: ''uni-axial compression ~~experiments~~ VP simulations''.

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).

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)'' Thes sentences is not directly relevant to what you do in your paper and could be cut.

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 mentionned that you consider sea ice to be 2D in the intro it is already implied.

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.

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).

Page 12, line 294: ''The angle of each fracture line~~s~~''.

Page 18, lines 420-423: Thank you for adding a comparision to the work of Hibler and Schulson, 2000 and Hutchings et al., 2005. However, I think it would be more relevant to present it in the introduction, to position your work in the context of what has been done in the sea ice community.