Authors answer to reviewers comments for tc-2020-153

December 18, 2020

Dear Editor,

You will find below our answers to the referees' comments. We thanks both, Véronique Dansereau and Harry Heorton, for their detailed comments and thorough review of our manuscript. Their reviews will lead to an improved manuscript. We answered all their comments and we describe the modifications we made to the manuscript related to them. The modifications can be slightly different in their formulation than the one proposed in the answers that are already online, but they are similar in their content.

Yours sincerely,

Damien Ringeisen,
On behalf of all three authors.

Note:

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

Answer to tc-2020-153-RC1 – Véronique Dansereau

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 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).
  - 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). 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.
  - 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).
  - 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.
  - 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 $\tilde{\#}$2 in comment R2#40.
  - See also our answer to comment R2#39 of Reviewer $\tilde{\#}$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#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#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#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.

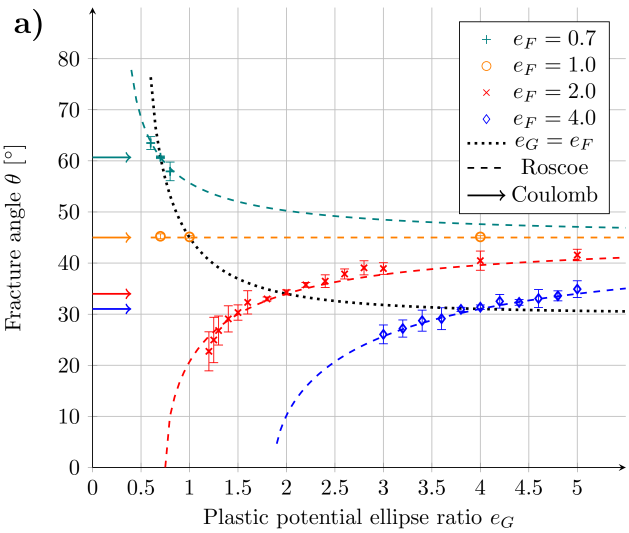

Figure 1: The new Fig. 7a *Caption:* **(a)** Fracture angles as a function of the plastic potential ellipse ratio e_G for different yield curve ellipse ratios ($e_F = 0.7$, 1.0, 2.0, and 4.0). The markers with ranges are the mean and two standard deviations of the fracture angles. The dashed lines show the Roscoe angle (Eq. 30). The arrows mark the Coulomb angles as a function of e_F, 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.

- 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., 1991) in a similar setup is underway, but lies outside of the focus of this work.

To improve our manuscript, we make the following modifications:

- **"*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.*" on L94 of the revised manuscript.**
- **"*Hibler and Schulson (2000) already inferred that 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 L420 of the revised 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, in contrast with observations of Linear Kinematic Features (LKFs) in the Arctic Ocean. Second, there is a tight, but unphysical coupling between the fracture angle, post-fracture deformation, and the shape of the yield curve. This tight coupling was identified as the reason for the overestimation of fracture angles. In this paper, these issues are addressed by removing the normality constraint on the flow rule in the standard VP model. The new rheology is tested in numerical uni-axial loading tests. To this end, an elliptical plastic potential — which defines the post-fracture deformations, or flow rule — is introduced independently of the elliptical yield curve. As a consequence, the post-fracture deformation is decoupled from the mechanical strength properties of the ice. We adapt the Roscoe's angle theory, which is based on observations of granular materials, to the context of sea ice modeling. In this framework, the fracture angles depend on both yield curve and plastic potential parameters. This new formulation predicts accurately the results of the numerical experiments with a root-mean-square error below 1.3°. The new rheology allows for angles of fracture smaller than 30° in uni-axial compression. For instance, a plastic potential with an ellipse aspect ratio smaller than two (i.e., the default value in the standard viscous-plastic model) can lead to fracture angles as low as 22°. Implementing an elliptical plastic potential in the standard VP sea ice model requires only small modifications to the standard VP rheology. The momentum equations with the modified rheology, however, are more difficult to solve numerically. The independent plastic potential solves the two issues with VP rheology addressed in this paper: in uni-axial loading experiments, it allows for smaller fracture angles, which fall within the range of satellite observations, and it decouples the angle of fracture and the post-fracture deformation from the shape of the yield curve. The orientation of the post-fracture deformation along the fracture lines (convergence and divergence), however, is still controlled by the shape of the plastic potential and the location of the stress state on the yield curve. A non-elliptical plastic potential would be required to change the orientation of deformation and to match deformation statistics derived from satellite measurements.*** "

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*"

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#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 on L24 of the revised manuscript now reads: "*Locally, the ice strength depends on the sea ice state (e.g., 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 straight lines, but will most probably follow the network of weaknesses.

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.

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#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).

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 shear bands or LKFs. This divergence is a function is a function of the distribution of contact points between individual floes at the sub-grid scales. A positive angle of dilatancy is associated with contact points that (on average) oppose the macroscopic shear motion and create divergence along the shear band; while negative dilatancy is associated with a closing of the shear line (ridging in the case of sea ice).*" on L77 of the revised 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: "*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#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.*" on L109 of the revised manuscript.**

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 on L117 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 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#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

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* $\dot{\epsilon}$"

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 184 of the revised manuscript "*VP sea-ice models typically cap the viscosity at*

$$\zeta_{\max} = \frac{1}{2\Delta_{\min}} \cdot P = \left(2.5 \times 10^{8}\,\mathrm{s}\right) \cdot P \qquad (1)$$

and $\eta_{\max} = \frac{\zeta_{\max}}{e_G^2}$ ***to regularize the momentum equations. When this regularization is in effect,*** ζ ***and*** η ***are independent of the deformation field (***Δ***) and the stress divergence reduces to harmonic viscosity with constant coefficients.*** $\Delta_{\min} = 2 \times 10^{-9}\,\mathrm{s}^{-1}$ ***(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 that operate on synoptic time scale, and VP rheologies can be considered as ideal plastic. The viscous behavior can be seen as 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 plastically 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 L279 on the revised 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.

On L303 of the revised manuscript, this sentence now reads: "***The fractures form a diamond shape, similar to the shapes observed at large scales (Erlingsson, 1988), in laboratory experiments (Schulson, 2001), and modeled with DEM models (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 $\dot{\epsilon}_{\mathrm{II}}$.

We clarify this in the caption: "***Diamond-shaped fracture pattern in the shear deformation field*** $\dot{\epsilon}_{\mathrm{II}}$ ***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.

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#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 the material (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 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#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 as one passes the maximum shear stress at $P/2$ (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 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#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 missing 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. The new Sentinel constellation and in-situ observations from the field program MOSAIC may bridge this gap. There is also a knowledge gap in the interplay 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 L456 of the revised manuscript.

Answer to tc-2020-153-RC2 – Harry Heorton

This paper describes the implementation of a non-normal flow rule in the VP sea ice rheology. The equational form of the new rheology is well described and several very useful diagrams are included. The numerical implementation is linked to a theory that links the flow rule and the intersection of failure lines within the medium described. A series of idealized numerical experiments are performed which show that the numerical rheology successfully recreates the fracture intersection angles predicted by the presented theory. The authors follow the experiments with a discussion on the implications of using a non-normal flow rule when designing future sea ice rheologies. They describe the various challenges when using non-normal flow rules. I find that this paper is well written and a valuable contribution to the modeling of sea ice deformation. It is a very useful introduction to use of non-normal flow rules for sea ice modeling for future work in this area. I recommend this paper for publication after a few questions I have.

We would like to thank the reviewer for the review of our manuscript. The many suggestions and comments will, without doubt, increase the quality of this manuscript.

R2#1, First of all can you explain why figure 7a contains both theoretical links between the plastic potential and intersection angle and many numerical experiments that back up the theory but 7b contains relatively few numerical results? I can see several cases where additional results from 7a can be copied to 7b and back up your results. Is it true that the full range of values for 7b are not obtainable due difficulties that the authors discuss in getting the model to converge to a solution for highly non-normal flow? If this is case then please tell us.

Figure 7b was intended to illustrate how the fracture angle changes when the plastic potential stays the same, but the yield curve changes. This was not the intended goal of this paper, as we wanted to focus on the effect of a varying flow rule at constant yield curve. We added the few point to show that the fit is still very good, only these few points could be reported. We would need to do many more simulations to populate this figure. To avoid confusion, we decided to remove the few points on this figure and emphasize the fact that it is shown for illustration. Please see our answer to comments R2#34 and R2#36.

R2#2, Several times in the discussion and results the authors say that the intersection angle depends on the confining pressure despite the varying non-normal flow rule. I can see no evidence of this in their results. The presented experiments show changing intersection angle with changing flow rule (varying plastic potential and yield curve eccentricity), but I see no results where they change the confining pressure. Is this from previous work? Or an interpretation of the results that they do present?

The fact that the angles depends on the confining pressure with a elliptical yield curve was explained in Ringeisen et al. (2019). Because the yield curve is still an ellipse here, there is no reason that this would change. We added a sentence to clarify this point. Please see our answer to comments R2#39 and R1#3.

General editing points:

R2#3, Can you please start the paper with a description of what a flow rule is. Then what a normal flow rule is, and the crucially what the main difference physically and theoretically is between a normal and non-normal flow rule. I see that a definition is on line 90, and then further physical descriptions of the flow rule are in the results. The introduction make much more sense if these can come first.

We followed the reviewer's suggestion and reorganize the introduction.

We reorder the introduction by moving the paragraph starting by "*This paper focuses on VP rheologies. Different...*" before the one starting by "*LKFs have been studied for...*"

R2#4, Can you describe what is documented in this study that is novel and new?
Corrected as suggested
We make the following modifications

- **We modify the abstract, see our modifications following comment R1#7.**
- **We modify the penultimate paragraph of the introduction (see also the answer to comment R1#2). 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.*".**

R2#5, L20 they are also, more importantly, observed

We are not sure what is meant here. We already state in the same sentence that LKFs are observed, and they emerge in high-resolution simulations.

We try to clarify: "*Linear Kinematic Features (LKFs), narrow lines of deformation, are observed in the Arctic sea ice cover, and also emerge in high-resolution simulations (Kwok, 2001; Hutchings et al., 2005).*"

R2#6, L21 Here your LKF's influence in many ways but what follows is not a list. Consider re-writing

Corrected as suggested.

We rewrite as a list "*...heat and matter exchange take place primarily over open water (Badgley, 1965), 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), which in turn is affected by sea ice fracture with thermodynamical growth in opened leads and with local dynamical growth during ridge formation.*"

R2#7, L22 Please define what a lead is. Consider starting with a definition of LKF's that are typically leads or ridges

Corrected as suggested.

"***LKFs can form in divergence, creating stretches of open water or leads, or in convergence, creating piles of ice or ridges (Stern et al., 1995).***"

R2#8, L70 Which is the 'standard rheology'? do you mean the VP rheology. Also can you further describe this result. How did Ringeisen find that the angle can't be lower than 30 degrees?

We meant the standard VP rheology, i.e. the VP rheology with elliptical yield curve and normal flow rule.

We clarify by adding "*Standard VP rheology*" on L95 of the revised manuscript. We also add "*, as shown by idealized experiments and theory (Ringeisen et al., 2019).*" on L96 of the revised manuscript.

R2#9, L71 the following list is hard to read. Consider reformatting. Also what does the $\mu = 0.9$, relate to with the Weiss and Schulson reference.

We decided to remove this citation from the list following the comment R1#22 of reviewer #1.

R2#10, L71 can you confirm that these angles are all comparable? I have found that studies document both the intersection and also the half angle, being the intersection between the fracture and the principal axis of stress.

We can confirm that these angles are measured the same way, i.e. they are the half angles, as for our study. (Hutter and Losch, 2020) used intersection angles, we divided the angles they reported by two in this list.

R2#11, L80 this paper requires a definition for a normal flow rule. This sentence and the following paragraph make little sense without it.

We reorder the introduction.

R2#12, L82 do you mean that the flow rule can be observed by measuring the ratio of shear a divergence along LKF.

Yes, the ratio of shear and divergence can be measure along the LKFs and give indications on the flow rule.

We modify the sentence as "*The ratio of shear to divergence along the shear bands or LKFs allows to infer the dilatancy angle of granular material.*"

R2#13, L85 were these laboratory observations performed the same way as those of Stern mentioned above?

Observations in Stern et al. (1995) are in Arctic sea ice, not from experiments, we add a sentence to clarify this. According to reviewer 1 (R1#13) the retrieval of the flow rule in Weiss et al. (2007) might be questionable and we decided to remove the sentence.

"*Observations of sea ice drift in the Arctic show that most of the deformation takes place in shear, that is, 98% of deformation show more shear than divergence or convergence (Stern et al., 1995).*" on L115 of the revised manuscript

R2#14, L89 it will be nice to have the Anisotropic Plastic (Tsamados et al., 2013) rheology listed here too

Added

We added the following entry to the list of rheological frameworks: "*. . . , Elastic-Anisotropic-Plastic (EAP) (Tsamados et al., 2013), or Maxwell-. . .*" on L41 of the revised manuscript.

R2#15, L92. Good to see a flow-rule definition here. How does the plastic potential determine the postfracture deformation? is this through the direction of the principal stress when the yield criterion is reached?

The flow rule is perpendicular to the plastic potential, as stated on page 3, L93. We will reorder the introduction and this will appear sooner in the introduction.

R2#16, L115 is f here the Coriolis acceleration as above? Actually can you tell what value was used for the Coriolis acceleration? If it is non-zero (valid to use zero and non-zero for these experiments) then asymmetry will be expected (see comments later)

Yes f is here the coriolis parameter. However we use $f = 0$ in this study.

In the Sec. 3, L277 of the revised manuscript, we add the sentence. *"For simplicity, $f = 0$."*

R2#17, L120 It is great to read this description of the VP rheology. A really helpful addition.

Thanks!

R2#18, L138 is it possible to have a physical description of the plastic potential here? The physical description of what the yield curve represents is very helpful. A similar description of the plastic potential here will be similarly useful. The flow-rule is a difficult concept that is explained well here. An additional physical description will make it even better.

In the revised manuscript, we add a few sentences describing the flow rule in physical terms.

"*The flow rule represent the direction of deformation in the grid cell. The orientation of the flow rule in the reference ($\dot{\epsilon}_{\mathrm{I}}$,$\dot{\epsilon}_{\mathrm{II}}$), as shown in orange in Fig. 1, indicates if the grid cell deforms convergence ($\dot{\epsilon}_{\mathrm{I}} < 0$) or divergence ($\dot{\epsilon}_{\mathrm{I}} > 0$) and shear ($\dot{\epsilon}_{\mathrm{II}}$).*" on L163 of the revised manuscript.

R2#19, L180 I see that the dilatancy angle was introduced earlier. However it would benefit the paper to include a physical description of 'dilatancy of a granular material' either before or here when it is implemented in the model equations.

We add a sentence in the introduction describing the physical process of dilatancy.

"*Dilatancy refers to divergence along shear bands or LKFs. This divergence is a function is a function of the distribution of contact points between individual floes at the sub-grid scales. A positive angle of dilatancy is associated with contact points that (on average) oppose the macroscopic shear motion and create divergence along the shear band; while negative dilatancy is associated with a closing of the shear line (ridging in the case of sea ice).*" on L76 of the revised manuscript.

R2#20, L180 and onwards. This section will benefit from an expanded introduction to the theoretical steps performed. From what I can tell, you use the theory that links dilatancy angle to fracture angle as discussed in the introduction. You have quantified the dilatancy angle using geometrical description of an arbitrary yield curve and plastic potential. This is expanded through the notation to express the fracture angle as a function of yield curve and plastic potential eccentricity. Is this correct? If so is the motivation behind the description that it is possible to show how the expected fracture angle is expected to change with changing plastic potential?

This is absolutely correct. The construction for the normal flow rule in blue on Fig. 3b shows what happens if the plastic potential is different, the fracture angle θ is different.

We add the following text on L218 of the revised manuscript: "*To adapt the Roscoe angles to sea ice modeling, we proceed as follows: (1) the stress state on the yield curve (point p on Fig. 3a) defines the position and size the Mohr's circle at fracture (blue circle on Fig. 3b), (2) the slope of the plastic potential determines the point on the Mohr's circle where deformation takes place, that is, the slope directly predicts the fracture angle θ as a function of the dilatancy angle δ (per Roscoe theory, Fig. 3b). For the special case of uni-axial compression, we (A) determine the stress state on the yield curve for uni-axial compression as a function of the yield curve ellipse ratio e_F, and (B) compute the slope of the plastic potential at that stress state as a function of the plastic potential ellipse ratio e_G. Finally, we combine (2) and (B) to compute the theoretical prediction for the fracture angle as a function of ellipse ratios e_G and e_F.*".

R2#21, Can you be clear what the theory of Roscoe is describing. Is the angle you are obtaining the expected angle of fracture due to minimizing some sort of energy potential? Or does it relate to an analytical solution of fracture? The mathematical expansion here is clear to follow, but the reasoning behind why you have shown it is less so.

The Roscoe angle is base on observations of deformation of granular materials in the lab, and is explained as the direction of "*zero-extension lines*"

We add the following sentence on L215 "*Based on laboratory experiments, Roscoe (1970) states that the* velocity characteristics *(the post-failure deformation) seem to be a better predictor than the* stress characteristics *(the stress at failure) for the orientation of shear bands in granular materials.*".

R2#22, In figure 4 you describe how the ratio of divergence to shear changes with changing plastic potential. Is this the key effect of the non-normal flow rule? In that by separating the yield curve and plastic potential it is possible to change the ratio of divergent to shear stresses whilst under deformation? But without also changing the point of deformation (as in the yield curve) If so please emphasize this point throughout the paper! It makes the non-normal flow rule much clearer for me!

Yes it is exactly the point.

We make two modifications to add this explanation of the non-normal flow rule:

- **"*By doing this, we will change the orientation of the flow rule, without changing the yield stress state (see Fig. 2 and Fig. 4 for some examples).*" on L195.**
- **"*. . . with an elliptical yield curve, therefore modifying the flow rule without changing the yielding stress state.*" on L472.**

R2#23, Figure 3 caption - the arrows are described as orange, but appear red to me.

The mistake was in the caption, the arrow is red. The caption is modified accordingly.

The caption now reads: "*The red arrow is...*"

R2#24, Figure 4 I see red and orange arrows here, and they are correctly described. Can you check

We checked – the colors are correct here.

R2#25, figure 3 Do the colours relate between the two figures?

Yes, red represent the case with non-normal flow rule, the blue one represent the case with a normal flow rule. We add a precision in the caption of figure 3.

The caption was modified to read " *(b) Mohr's circle for the fracture state p in a) (for normal in blue and for non-normal flow rule in red) in the fracture plane of reference (σ, τ)*"

R2#26, L222 is the initial ice state entirely uniform? Or did you seed some noise into the initial state?

Yes the initial state is with uniform ice (thickness and concentration).

The sentence is now "*...Following Ringeisen et al. (2019), we load a rectangular ice floe of 8 km by 25 km with a uniform thickness of* $h = 1\,\mathrm{m}$ *and a uniform sea ice concentration of* $A = 100\%$ *(see Fig. 5).*" on L267 of the revised manuscript.

R2#27, L231 did you test at other time and spatial resolutions? Later you comment that fracture angles were shown in a previous study to be independent of model resolution (we found this too). Did you test this for this study too?

No, we did not test the dependence of scale and resolution here, only in Ringeisen et al. (2019). There is no reason to think that this would change, no scale nor resolution are included in the formulation of the rheology.

R2#28, L232 is this equation 4 that is solved for?

Yes, we add a reference

We add "*...Eq. (4) ...*".

R2#29, L233 What are the non-linear and linear problems ? Can you relate these back to the model equations?

The non-linear problem is linearized and solved iteratively. The non-linear problem is then updated with the intermediate solution from the linear iterations, and then the cycle continues. We do not describe here further the formulation of the solver, it would be outside of the scope of this work, and relatively long. However, it is described in several studies (Zhang and Hibler, 1997).

We add *"For the linearized problem within each "non-linear" iteration ..."*.

R2#30, L246 So are the simulations only run for 5 seconds of model time? Have you tested how long the model can run for and its overall stability? I read above that you have used excessive computation to ensure the extra complexity of the non-normal flow rule is accounted for. How successful is this approach? Did you find that certain computational setups did not perform well when attempting to solve the equations? Any insight you can share into how to solve these equations will greatly help the sea ice modeling community

We tried this rheology in a pan-arctic setup (2 km) with an integration time of the order of one year and did not experience any problem (not shown). As we show, a lot of iterations are needed for this experiment, but much less are needed in realistic setups. The actual computational details are not relevant to this paper, but will be discussed in a different manuscript describing realistic applications, where they are relevant.

R2#31, L263 what is average residual norm R? is this a measure of the solution accuracy?

We mean the L_2-norm of the residual of the non-linear equations.

We modify the sentence on L312 of the revised manuscript to start by "*For instance, the L_2 norm of the residual of the non-linear equations*".

R2#32, L282 is the shear strain rate shown anywhere? Are you relating back to figure 6? If so can you say so? Are you saying the relationship in figure 6 for e_F and shear strain rate is also true for the various values of eF in Figure 7? Or is this a theoretical postulation?

We were referring to Fig. 6. This is something we observe in the simulations, Figure 6 gives an example.

The sentence starts now with "*For $e_G > e_F$, the shear strain rate increases along the LKFs (see Fig. 6c) and...*". The same way we change the sentence on L283-284 as "*...less shear along the LKFs (see Fig. 6a), and the fracture ...*"

R2#33, L282 fracture angle or angles plural? Do you you take multiple angles or just one per simulation?

We measure multiple angles, then compute the average and standard-deviation. In this case, "*angles*" would be the correct conjugation

We correct "*...and the fracture angles tend toward ...*"

R2#34, Figure 7 Is it possible to add the red orange and teal numerical simulations to figure 7b? If you have added the blue dots then the omission of the others makes me wonder how they will fit? I see that you only have multiple values for $e_G = 4.0$. Though there are 2 points for 0.7 and a single point for 2.0 and 1.0. I also see that the full range of e_F was not investigated for each e_G. What is the reason for this? Is it the limitations of the model? Or did you choose not to in order to keep the simulations physically relevant?

This would need new simulations, data points on figure 7a cannot be reported on figure 7b (excepted the three points for $e_G = 4.0$. The figure 7b was intended for illustration only, the blue points for $e_G = 4.0$ were added to show that the theoretical prediction still fit. Because it shows the same formula, the predictions should be as accurate in 7a than in 7b. Changing e_F at constant e_G is not as interesting as the inverse, because both the flow rule and the fracture stress state change (see Fig. 4). We add an sentence in the figure caption to clarify this.

The caption of Figure 7b now reads: "*Roscoe fracture angle computed from Eq. (30) as a function of e_F with a constant e_G, for illustration. As e_F changes, both the stress state and the flow rule change (see Fig. 4), resulting in a more complex behavior. The black dotted line for the normal flow rule ($e_F = e_G$) is drawn for reference.*"

R2#35, L305 this line is very informative to what the non-normal flow rule can achieve. Can you put this information into the introduction and abstract please?

We have added a similar sentence to the abstract, please see R1#7.

R2#36, L309 while you have displayed the agreement to Roscoe for the cases of constant e_F the case of constant e_G (fig 7b) is inconclusive to the reader due to the lack of numerical simulation data points. Is it possible to fill out figure 7b and thus strengthen this statement?

Filling it is totally possible, but would mean a non-negligible amount of new simulations. We think this is unnecessary, it would just mean reversing the Eq. (27), we showed the case of $e_F = 4$ to show that it does correspond to the Roscoe Angle as well. Please see also our answer to comment R2#1.

R2#37, L313 Can you sort out the parenthesis on the Ringeisen 2019 citation. It currently doesn't read very well.

Corrected as suggested.

R2#38, L317 is this lack of convergence the reason for the lack of results on figure 7b?

No, figure 7b is shown to illustrate of the evolution of the fracture angles when e_G is kept constant and e_F changes. See our answer to the question R2#35

R2#39, L319 Can you give a citation for a description of how this result with the changing fracture angle with changing stress confinement was obtained? I assume it is not from this study as you have not altered the confinement ratio for any of your simulations. Or are you referring to that the fracture angles change as the loading increases with time?

We are referring to the experiments in Ringeisen et al. (2019, Sec. 3.2.2, Fig. 8), where the elliptical yield curve with normal flow rule is used. We add this citation.

We add the citation on L368 as such: "*Because of the elliptical shape of the yield curve, the angle of fracture in the standard VP model changes with confining pressure (Ringeisen et al., 2019, Sec. 3.2.2, Fig. 8) 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). 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 as one passes the maximum shear stress at $P/2$ (Ringeisen et al., 2019).*" Note also our answer to Comment R1#3 of Reviewer #1.

R2#40, L321 How do think this result relates to laboratory experiments on sea ice where two clear fracture angles were found above a critical confinement ratio? (Golding et al., 2010; Schulson, 2002)

Thanks for the reference, we now add a short discussion in the text.

We add the sentence "*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).*" on L377 of the revised manuscript

R2#41, L341 Is this result about pure shear and angle of 45°. From the Ip et al. (1991) citation? How was it obtained?

This is not a result, but the prediction from the Roscoe theory when a pure shear flow rule is used. When the flow rule tends to be only oriented in shear, the fracture tends to 45°. We add a few words to reflect this.

The sentence on L399 now reads: "*For a Mohr—Coulomb yield curve with a double sliding law (i.e., pure shear deformation, Ip et al., 1991), the Roscoe theory predicts a fracture angle of approximately 45° that is independent of the slope of the yield curve. This behavior can be mimicked using an elliptical yield curve and plastic potential by setting $e_G \gg e_F$, hence $\delta \simeq 0$ and $\theta = 45°$ (Fig. 7a).*"

R2#42, L345 angle - angles

Corrected as suggested

"*...would allow for a different angle of fracture with shear ...*"

R2#43, L363 Is it possible to include a diagram of the various yield curves discussed in this section? This would greatly ease the understanding of your arguments. I'm sure others have included such a diagram in previous work so you may be able to cite such a diagram.

We add a figure to show the alternative yield curve and their flow rule.

A new figure Fig. 8, showing the different yield curves and their flow rule, is added to the revised manuscript. It is shown below as Figure 2.

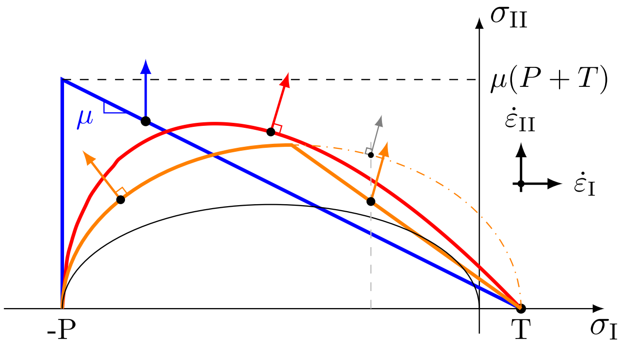

Figure 2: New Fig. 8. *Caption:* Alternative yield curves and flow rules: The Mohr-Coulomb yield curve with shear, non-normal, flow rule (blue Ip et al., 1991), the modified coulombic yield curve with normal (elliptic part) and non-normal (linear part) flow rule (orange, Hibler and Schulson, 2000), and the teardrop yield curve with a normal flow rule (red, Zhang and Rothrock, 2005). The elliptical yield curve with $e_F = 2.0$ is shown for reference (black thin line). P is the compressive ice strength, and T the tensile ice strength.

R2#44, L369 Can you explain why non symmetrical deformation features are unrealistic or present an incorrect solution? Do they also correspond to poor numerical solutions? With a non-linear system of equations such as in all sea ice rheologies, asymmetry is often expected. This relates back to most laboratory experiments on ice deformation and even the ill-posedness of divergent weakening (Gray, 1999). Also if you use a non-zero Coriolis acceleration then asymmetry will be expected as the run progresses. What value did you use?

- Because our experiment is fully symmetrical (forcing, ice initial state, ...), the fracture pattern should not show asymmetries. This expectation is met by most of the simulations with the elliptical yield curve with normal flow rule (See Figure 6b or in Ringeisen et al. (2019))
- There is no Coriolis force in these experiments that would break the symmetry. We add this detail to the experiment description, see our answer to comment R2#16.

We add the following sentence: "***This asymmetry is not expected, and is not found with normal flow rules, therefore we assume is stems from the non-normal flow.***"

R2#45, L371 I'm not sure I understand your argument here. Are you saying; poor non-normal flow model convergence won't be an issue in realistic simulations as the numerical solver can't solve the VP rheology anyway? Surely this argument says that there isn't a hope of using non-normal flow VP rheology in realistic simulations?

Our argument is:

- The forcing in high-resolution simulations of sea ice changes on long timescales compared to the timestep of the ice dynamics
- Therefore the solver starts from an already good solution from the previous timestep. This way, the solution of the momentum equation is accurate enough even at high-resolution.
- In the experiment here, the forcing increases fast, at every timestep, so the solver does not benefit from the previous timesteps.

The sentence now reads "***In practice, the numerical convergence issue will go unnoticed in simulations using realistic geometries and time varying wind forcing. In these simulations, while the number of iterations typically used (O(10)) is much smaller than that required for full convergence, at each time-step, a new iteration typically uses the solution of the previous timestep as the initial estimate.*** " on L438 of the revised manuscript.

R2#46, L396 These issues are not exclusive to high resolution climate modeling. It can be argued they are even more important for current coarse resolution models which are currently used for long climate simulations and typically perform poorly for reproducing ice drift patterns. LKF intersection angles are also observed over basin length scales (Weiss and Schulson, 2009) and your discussion in this paper is relevant for modeling sea ice deformation at these length scales.

We agree with your analysis. We modify our statement as follows.

"***Designing more appropriate rheologies for improved high-resolution climate models and more accurate sea ice prediction systems requires consolidated observations of these still unclear physical processes.*** " on L466 of the revised manuscript.

R2#47, L406 I am confused by your conclusion here. Where have you shown that the fracture angles depend on the confinement pressure? Where did you change the confinement pressure? Do not Figure 6 and 7 show clear changes in intersection angle with changing plastic potential in accordance with predictions from the theory of Roscoe?

We are referring to the results presented in Ringeisen et al. (2019), where we showed that the fracture angles depend on confinement because of the elliptical shape of the yield curve. We state this more precisely in the revised manuscript.

"***Because of the elliptical yield curve, the fracture angles still depend on the confinement pressure (Ringeisen et al., 2019), and the elliptical plastic potential only modifies the ratio of divergence relative to shear, but not the direction of deformation at the fracture lines (convergence or divergence).***" in L478 of the revised manscript.

R2#48, L409 again I'm not convinced that symmetric solutions are mandatory for a symmetric experiment? Again can you say whether you used a zero or non-zero value for the Coriolis acceleration? If it is non-zero then asymmetry will be expected.

The Coriolis force is zero in our experimental setup (see R2#16)

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