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Interactive comment on "Non-normal flow rules affect fracture angles in sea ice viscous-plastic rheologies" *by* Damien Ringeisen et al.

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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, very clear. Here are my major comments/concerns :

âĂć It does not appear clear in the paper what physical process(es) the authors really want to model. In the introduction, it is mentionned 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 thoughout the paper as to what the authors consider is the mechanical behavior of sea ice : is it caracterized 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.

âĂć 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 independant of confining pressure. It appears that they aim at developping 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 circonstances other than an individual ice floe crashing into a coast? References are lacking here to support this assumption of independance on confinement and should crucially be added.

âĂć 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 strenghten the paper.

âĂć 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, Heil and Hibler, 2005, if deviations between the simulated angles and the predicted values occured, 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.

âĂć 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, Heil and Hibler, 2005 who have looked at intersection angles. They have compared simulated angles between the modified Coulombic and the elliptical yield curve. Mentionning these previous results

and comparing them with the current study would be interesting and would strengthen the litterature review and Discussion part of the paper. I therefore recommand 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.

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, se my main comment about the agreement of the theory with your modeled angles.

Page 1, line 11: I suggest dropping "In conclusion" from your abstract.

Page 1, lines 14-15: "to make the fracture angle independant 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 independant of the confining pressure? Please give supporting references. Is it even possible to distinguish between fracturing processes ocurring in confined and unconfined conditions in the sea ice cover at the geophysical scale?

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.

Page 2, line 23 : "The ice strenght locally depends on the ice thickness". This is only partially true: local ice strenght does not depend only on local ice thickness. This sentence perhaps needs some rephrasing.

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.

Page 2, line 28: “This anisotropy”. This is unclear. Please define this anisotropy and better explain how it emerges.

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.

Page 2, line 39: I believe a simpler and scientifically more objective formulation would be “most widely used”, instead of de facto standard.

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?

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 invoque 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.

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

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.

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

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?

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 derived this directly from their estimated internal friction angle, which is fitted 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.

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?

Page 3, line 75: the ‘’gradient” of shear to compressive strenght. Did you mean the ratio?

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 assesment of your assumptions and methods by the reader much easier.

Page 3, line 82: "The ratio of shear and divergence along the LKFs allows to infer the dilatancy angle." Again, if one assumes sea ice in any state behaves as a granular material.

Page 3, lines 84-85: "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 (...)".

Page 4, line 90: ''In these different classses 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?

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, Heil and Hibler, 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 litterature 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 independant of confinement, which would be an important point to discuss further.

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

Page 4, line 100: "from the yield curve".

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.

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.

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.

Page 4, lines 17-19: 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).

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.

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.

Page 9, line 204: “The slope of the yield curve”. And many other missing “the” throughout the text.

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 contraint on the appearance of conjugate faults and on their orientation. I think this would be an interesting and not time-consuming test.

Page 11, line 240: I suggest “more numerically challenging”.

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 et al., 1988 and Wilchinsky et al., 2010?

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?

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.

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.

Page 15, line 300 : ‘’the shape of the plastic potential”.

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?

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.

Page 15, point 2: About confinement, shear bands and fractures, see my major comment above.

Page 17, line 382: ‘’sea ice mechanical strenght properties (yield curve) and deformation (flow rule)”. Again, you write this with the perspective of a VP model, but mechanical strenght 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 undestand why Dansereau et al., 2016 is cited in this context.

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.

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2020-153/tc-2020-153-RC1-supplement.pdf

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