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 idealised 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 rhelogies. 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 modelling of sea ice deformation. It is a very useful introduction to use of non-normal flow rules for sea ice modelling for future work in this area. I recommend this paper for publication after a few questions I have.

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.

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?

General editing points:

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.

Can you describe what is documented in this study that is novel and new?

L 20 they are also, more importantly, observed

L 21 Here you LKF’s influence in many ways but what follows is not a list. Consider re-writing

L 22 Please define what a lead is. Consider starting with a definition of LKF’s that are typically leads or ridges

L 70 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?

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.

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.

L80 this paper require a definition for a normal flow rule. This sentence and the following paragraph make little sense without it.

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

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

L89 it will be nice to have the Anisotropic Plastic (Tsamados 2013, 10.1029/2012JC007990) rheology listed here too

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

L 115 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)

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

L 138 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 difficult concept that is explained well here. An additional physical description will make it even better.

L 180 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.

L 180 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 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?
Can you be clear what the theory of Roscoe is describing. Is the angle you are obtaining the expected angle of fracture due to minimising 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.
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 change the point of deformation (as in the yield curve) If so please emphasise this point throughout the paper! It makes the non-normal flow rule much clearer for me!

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

Figure 4. I see red and orange arrows here, and they are correctly described. Can you check figure 3. Do the colours relate between the two figures?

L 222 is the initial ice state entirely uniform? Or did you seed some noise into the initial state?
L 231 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?
L232 is this equation 4 that is solved for?

L233. What are the non-linear and linear problems ? Can you relate these back to the model equations?
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 modelling community

L 263 what is average residual norm R? is this a measure of the solution accuracy?

L 282 is the shear strain rate shown anywhere? Are you relating back to figure 6? If so can you say so? Are you saying the relation ship in figure 6 for eF and shear strain rate is also true for the various values of eF in Figure 7? Or is this a theoretical postulation?

L 282 fracture angle or angles plural? Do you you take multiple angles or just one per simulation?

Figure 7 Is it possible to add the red orange and teal umerical simulations to figure 7 b? 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 eG = 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 eF was not investigated for each eG. 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?

L 305 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?

L 309 while you have displayed the agreement to Roscoe for the cases of constant eF the case of constant eG (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?

L 313 Can you sort out the parenthesis on the Ringeisen 2019 citation. It currently doesn't read very well.

L 317 is this lack of convergence the reason for the lack of results on figure 7b?

L 319 Can you give a citation 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?

L 321 How do think this result relates to to laboratory experiments on sea ice where two clear fracture angles were found about a critical confinement ratio? (Golding et al. 2010 1359-6454/$36.00, Schulson 2001 10.2138/gsrmg.51.1.201)

L 341 Is this result about pure shear and angle of 45deg. from the Ip et al. 1191 citation? How was it obtained?

L345 angle - angles

L 363 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.

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 10.1175/1520-0485(1999)029<2920:LOHAIP>2.0.CO;2). Also if you use a non-zero Coriolis acceleration then asymmetry will be expected as the run progresses. What value did you use?

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?

L396 These issues are not exclusive to high resolution climate modelling. 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 modelling sea ice deformation at these length scales.

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?

L 409 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.

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.
Here are my major comments/concerns :

- 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.

- 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.

- 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.

- 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.

- 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 material*s*" or "a viscous-plastic material", "with any flow rule~~s~~".

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.