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Interactive comment on "Modeling Sea Ice fracture at very high resolution with VP rheologies" *by* Damien Ringeisen et al.

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I am really pleased to see a paper that is making suggestions for idealised experiments we can use to differentiate between rheological models for sea ice. This in itself is worth publishing. The main result of the paper is that the elliptical rheology is inappropriate for representing observed cracking orientation in the ice pack, which is interesting and helps motivate changing sea ice rheological models used in climate and weather prediction.

I do have some concerns that the interpretation of observational data needs sharpening, and the results must not be overly interpreted given limitations of the use of RGPS data. Identifying intersection angles for lead pairs actually requires more work (and

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dedicated field data collection) than this paper warrants. You are motivated by the fact that a simulation shows larger intersection angles than RGPS, and I agree that this is something to address. I just do not think you can use RGPS to determine what the intersection angle should be, just that it needs to be smaller. Note, there are others in the community funded to do this work of identifying fracture patterns associated with particular modes of failure. For example I have an NSF and NASA project that is looking at identification of modes of failure from satellite imagery. There are also upcoming field experiments that could provide case studies to constrain the actual behaviour of sea ice, which should provide further guidance for use of your idealised cases to constrain rheological model design. I would be happy to talk to you in person about using this analysis and data to support future model validation efforts. I would also caution you to be more careful in your description of the differences between VP and granular models. Some clarification missing from the manuscript is provided in my comments. I also have suggestions for why the VP model creates LKFs, which I feel is important for understanding the validity of LKFs in the viscous plastic sea ice model representing nature.

Specific Comments

Check spelling throughout the manuscript. Also check for missing brackets throughout. Grammar can be improved in places, and sometimes words are repeated. Make sure you have someone very carefully proof read the manuscript. I did not correct all the typos I saw because I am short on time and wanted to focus my attention on the central messages in your paper.

In the introduction be specific that you are considering conjugate fault pairs that form under specific confining stresses, the orientation of which is controlled by the yield curve shape and flow rule. In particular reference Pritchard in the introduction. It is only when I got to the conclusion that I saw you were aware of this work and it was motivating your study. It is wise to point out that not all applied stress will result in intersecting fault pairs (for example tension and pure compression do not).

Page 2 line 4. The appropriate references for efficient solution is Hutchings et al. 2004 or Jean-Francois Lemieux et al. 2010, I would not call LSOR or Hibler's method, which I used in the 2005 paper, as efficient. This introduces the efficient solution method that correctly couples P and U, for a convergent plastic solution. This solution method was not used in Hutchings et al. 2005. Hutchings et al. 2005 is the correct reference for qualitatively reproducing LKFs in the viscous plastic model.

Page 2 line 15: MEB? typo? I think you need to introduce the acronym for the Maxwell-Elastic-Brittle model here.

Page 2, line 24: argues -> argued.

Page 2, line 31: Flato and Hibler 1992 is not a mohr colomb relationship. The cavitating fluid behaves very differently and the first SIMIP (see work by Kryesher and Harder) indicated this was not a suitable stress-strain relationship for sea ice. Also check that Ip et al. 1991 is not using a different flow rule to Tremblay's. I am wondering if you are missing text here, as these two references were left hanging.

Page 2, some important points that I do not think are clear in your introduction:

The Elastic-Plastic model developed during AIDJEX was based on assumptions of a material with embedded cracks in all directions that are sub-grid scale. This is closer to a ductile material than granular material

The Viscous-Plastic model is only considered valid on course resolution (Hibler 1977). It is possible to consider this model with the ice always being in a state of plastic failure, until you get to high resolutions that allow representation of ice areas between fractures, when the viscous creep, while numerically small, is unphysical. At small scales an elastic model is appropriate for low stress states. The viscous behaviour inside the yield curve is often treated as regulaisation required for numerical solution.

Personally I think it is still not clear that the failure mode of a single floe is the same as an aggregate of floes. This has not been shown observationally or with models, and statements of scale invariance based on observed qualitative correspondence between failure modes in the lab (cm scale) and ice pack (10-100km scale) do not extend to the floe scale.

Page 3 Discussion regarding orientation of intersecting LKFs from RGPS: I performed a similar analysis back in the early 2000's and never published the result, which was a wide spread in intersection angle. The reason I did not publish this is because I realised that the RGPS product could potentially be capturing fracture zones that form at different times, and therefore in the product appear to be a conjugate pair because they intersect, but they are not because they were not formed under the same confining stress. This is really obvious if you send some time on the ice pack in winter and observe leads forming and working. RGPS is not the right satellite product to use to identify conjugate fault pairs in sea ice. Hence I disagree that you can state "The wide range of intersection angles is presumably due to previous deformation history and associated hetrogeneity in the ice cover that dictates the strength locally".

page 3, line 32: Just want to clear up one very important point about my 2005 paper. It is steep stress gradients in the model sea ice stress field that allow LKFs to form. I suspect this opening is related to an instability in the model identified by Nico Gray (Gray and Kilworth 1995). We seeded stress gradients though a random number being added to P* (which defines compressive strength). At the time I wrote this paper I was obsessed with plastic convergence of the VP solution, so made sure there were no spurious stress values due to the numerical error. The pan-Arctic model of Heil's included in this paper, and other VP models, are able to show LFS because of the noise introduced by not converging fully to the yield curve. If you play around with a VP model you can create divergence related instabilities along gradients in forcing (e.g. non-smoothly interpolated wind fields), or even have the model blow up and crash due to one localised discontinuity in thickness (e.g. I have seen this when using a nudging method to assimilate data into the CICE model that created open water locally). The reason I bring this up is that I feel it is very important that people understand how the

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VP model can create LKFs. The mechanism is quite different from what might actually be happening in a granular or brittle material. I would be very happy to advise on experimental design, repeating and following up on my investigations 15 years ago.

page 3, line 34: The original study on shape of yield curve and ice arches is in Billy Ip's thesis, that was published later by Hibler in Hibler et al. (2006).

page 8, line 14. A reader unfamiliar with numerical solution of the VP model will need some guidance as to what non-linear and linear iterations are. I know you are talking about the sub-cycling to reach plastic equilibrium (or close to it) and converge the velocity solution at each time step. Perhaps use language that is more obvious to a casual reader. Incidentally, did you check convergence properties? Just curious. I think you point out somewhere that the modified coulombic rheology is slower to converge - I found that solutions for yield curves with corners never converged fully. A frustrating reality! If you follow my suggestions to delve into why the model creates LKFs you will need a full description of the interative process and convergence characteristics.

page 8, results section: Describe what the applied strains are in the numerical experiments (magnitude, not just direction).

page 8 line 26: What are the default parameters? I think you forgot to reference table 1.

page 9, line 4: Regarding your statement "Fracture occurs when the stress state intersects the yield curve". Plastic failure occurs then. The fact that a "fractures" form is because the ice deforms at a stress discontinuity where the stress accumulates and reaches yield. You are correct in pointing out that the strain-rate has characteristic directions along which divergence will occur, defined by the shape of the yield curve and flow rule. It is this divergence, relative to the confining stress, that defines the directions of the linear deformation features in the model runs.

Comment on differences between 3.1 and 3.2: The change in nature of cracks when you decrease the number of internal iterations (linear iterations) is probably related to the fact that the ice stress field is more heterogeneous (further from the converged solution) and the LSOR method tends to create noise in the stress field and smoothes with increasing number of iterations (unlike the SIMON method I proposed, Hutchings et al. 2004, that has a smoother convergence to the yield curve). Hence there are more points where LKFs can nucleate when you reduce the number of internal iterations. This is just a suggestion, with out looking at the stress fields in your experiments I can not tell you if this is what is actually happening. Incidentally, another unpublished result that I presented at AGU in 2003: The VP model can create intersecting deformation features across the entire Arctic Ocean is you do not converge to plastic equilibrium and are not careful in smoothing the solution between time steps (which can be done numerically through the choice of advection scheme or Bill's introduction of artificial diffusion in his 1979 paper). I never followed up this work. I suspect that this is a direct consequence of Gray's instability. This instability is damped by the addition of numerical diffusion (or artificial diffusion) in the solution proceedure. We might think the resultant strain-rate fields are more realistic, I just do not believe using the non-convergence and numerical instability is an appropriate way to model the process because we are not controlling the nature of the stress concentrators or stress propagation in the model appropriately. The key point is that the ice pack strength is highly heterogeneous and while we do not know the nature of the stress concentrators in the ice pack, they are likely to be more randomly distributed (which non-convergence to the yield curve might be approximating, but is not controlled for). There is a need to understand the nature and distribution of the stress concentrators in sea ice, so we can appropriately model this. And it would really help future sea ice modellers to point out this issue more clearly in papers that investigate LKFs in the viscous-plastic model.

Figure 7: Nice illustration of the role of boundary conditions on the stress solution. Section 3.5: Good illustration. I would suggest you critically look at the stress fields in your previous experiments to identify what the stress concentrators are there. Did you forget to reference figure 9 in this section. Finally, Bill Hibler has shown similar

results where embedded fractures of different orientations would join together to form larger scale fracture patterns. Not sure he published that, but I think he did. Look at the papers he wrote with Aksenov and his first anisotropic paper with embedded leads in grid cells. Unfortunately I am on an airplane right now and don't have access to his papers.

page 16 line 1: Here and in other places you confuse the simulation with reality. "This is in contrast with other granular materials". Remove "other", as this is in contrast with granular materials. The VP model is not modelling a granular material. While sea ice may be a granular material, the rheology is designed for different behaviour.

page 16 line 5: "larger that what" -> "larger than that"

page 16, line 9: I would like to see the strain-rate field for the longer simulation with e=0.7 where deformation is in convergence. Where in the field is the ridging occurring? What do the intersection angles look like?

page 17 line 2: "and individual floes form" could be clarified as "4 separate floes form".

page 17 line 15: Please clarify the statement "the fracture pattern is very sensitive to coefficient of internal friction. This makes measuring the fracture angle very difficult." Surely the sensitivity will help you differentiate fracture angles. Incidentally mu was not defined in section 2 or here.

What causes the spread in the stress state? Is this related to the opening/ridging and subsequent ice strength changes? So the spread in stress state is controlled by the strength parameterisation. I feel this is important to point out, because it is another control we have on the spread of intersection angles you might see under a particular confining stress.

page 17 line 25: Clumsy language: "the stress state touches the yield curve on both parts of the yield curve." Very unclear that you mean the stress state falls on the coulombic limb and the ellipse cap. rephrase.

page 19 top paragraph: This is a matter of opinion. I disagree that the ice pack is characterised by diamond shaped floes. Yes, diamond floes form under certain confining stresses, but is this the most prevalent mode of failure in winter? That needs to be proven. This point does not discount your use of your numerical experiments to differentiate between rheologies, but it does question if an anisotropic rheology based on diamond shaped floes is appropriate for all space and time.

Also, did you calculate characteristic directions for the VP model to confirm these are controlling the diamond structures in your simulations? I have a code somewhere (from 15 years ago) that does this. If I can find it I can give it to you.

page 19 line 12: This sentence is miss-representative: "Thus, the rheology is shown to be scale independent ... in line with observations". Your numerical experiment is set up to ensure the behaviour is scale independent from the scale of the grid size to the domain size. You would really hope your numerical experiment results do not depend on resolution (good practice to check this) and there is no reason scale should change intersection angles for the reasons you have stated previously. Rephrase this statement so someone does not quote it as evidence supporting Schulson's hypothesis.

page 19 line 20: Just to clarify, the reason the experiments with thin ice change the fracture angle is because the presence of the thin ice modifies the stress state across the domain. So with an ellipse this will change the intersection angle, with a coulombic rheology it would not. I feel this part of the paragraph needs more clarification.

page 19 line 22: Your interpretation of the RGPS data (if one believes the intersection angles are at conjugate pairs and not leads formed at separate times) would lead one to believe that there is not a constant fracture angle independent of confining stress.

page 20 line 2: Perhaps clarify that the Miller et al. (2005) experiments were using metrics of ice thickness, area and velocity to determine the optimal yield curve shape. It is my memory they did not consider the form of the ice strength parameterisation as an alternative to changing shear strength, or yield curve shape, just the eccentricity of the ellipse.

page 20 line 5: Can you show that your numerical experiments are consistent with Pritchard (1988).

page 20 line 8: questions -> questioned

After reading your discussion I wondered if the tear drop yield curve (originally proposed by Pritchard) might be more appropriate that the Hibler modified ellipse / mohr coulomb.

Also, Hibler and others recognise that you must have a closed cap on a coulombic rheology to allow ridging. Perhaps the ellipse is not the best choice for this. In engineering it is more common to have a flatter closure to the yield curve.

page 20 line 26: sensible -> sensitive

page 20 line 26: Another example where it is not so clear you are talking about the VP rheology problem: "The fracture angles is also sensitive to the surrounding sea ice cover, in contradiction to the granular nature of sea ice". Also, the stress field is going to depend on surrounding ice even when the ice is modelled as granular, which I am not sure is what you were meaning to imply is not true for granular materials. I think you need to clarify the language, and I think I disagree with you that this test is suggesting ice is granular - it would be something we could test in an ice tank to find out what the actual behaviour is though.

I feel you do not highlight a key result in the paper: That fracture angles below 30o are not possible with the elliptical rheology, and that this is in direct conflict with observational evidence for smaller fracture angles. Even in light of errors of interpretation of the RGPS intersection angles this result still holds.

page 21 line 3: Note that at cusps in a yield curve two possible solutions are possible. I feel you can clarify your point about not using non-differentiable yield curves. They are also numerically unstable. The unclear fracture pattern is not something I have issue with. Perhaps this exists in reality when the stress state can spread across opening and closing modes.

page 21 line 10: I feel you can be stronger here in stating that the ellipse with normal flow rule can be discounted as unphysical.

page 21 line 11: Scale is really unimportant in these experiments. You can perform them on any scale. The more important question is what scale do these types of fracture events actually occur on and can that be resolved in models?

I see you did not reference work by K. Wang (2007) who used lead intersection angles to try to estimate the shape of a yield curve. He also has a paper were he performed a similar study to you (Wang and Wang 2010), however for the pan-arctic and perhaps with convergence issues that make his findings hard to interprete. While this work suffered from problems of representativeness of the observational data (how can you be sure fractures formed at the same time), as you do, I feel you should consider Wang's papers in light of your findings.

Finally, I believe that the stress state between fractures in your numerical experiments is inside the yield curve (viscous), and the motion close to zero. Is this correct, it was what I found when I was working on this. Just a point to clarify that the accumulation of stress along fractures is due to the yield curve discontinuity, and the associated characteristic directions in the strain field that control the propagation of fracture direction. This accumulation of stress needs to be nucleated at a location with high stress gradient (such as a corner on the boundary or strength/stress difference between grid cells). Once the stress reaches the yield curve, the numerical instability is probably put into play during the inner iterations. You do not see LKFs in VP models that have smooth boundaries and strength fields. The formation of LKFs is grid resolution dependent (as the linear instability identified by Gray is). You have speculated on why LKFs form in the VP model only at higher resolutions in a previous paper and I would suggest the place to look is in the convergence of the solver, and the splitting of velocity solution

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from the ice strength (pressure). I do not think it is just the fact that divergence (and strength reduction) can be greater at higher resolution. Clarifying this mechanism will help readers understand why VP models show this behaviour. It will also hopefully get people thinking about how to represent stress accumulators in the model, because many people using the VP model and studying fractures are unaware of how the model produces these.

Jenny

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