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The Cryosphere Discussions

Interactive comment on "Semi-brittle rheology and ice dynamics in DynEarthSol3D" *by* L. C. Logan et al.

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General appreciation: This study seeks to apply a viscoelastic rheology combined with a semi-brittle yield strength to simulate the flow and failure of glacier ice. The authors apply a pre-existing model designed to simulate deformation of the solid Earth and apply it to simulate failure that originates near the grounding line of glaciers that transition from grounding to floating. The authors perform experiments using idealized geometries to demonstrate the method is promising and predicts basal crevasses that originate downstream from the grounding line.

Overall, I think that the approach proposed is innovative and has potential to provide significant insight into calving and fracture processes of glaciers and ice shelves. I do, however, have some comments and lingering questions. Some of the comments relate

to pleas for additional clarification of aspects of the model. Others are suggestions for additional model experiments. As is always the case, reviewers want a different subset of experiments than those the authors provide. Given limited computational resources, I don't think it is necessary the authors perform all of the additional simulations suggested, but the authors should consider a subset of these experiments, if not for this study than for future studies. Many of the comments are geared towards clarifying concepts and approaches to make the manuscript more accessible for a wider readership of glaciologists and the authors should at least consider some of the modifications. These suggestions are described in more detail below

1. Rheology and yield relations. I would like to see a much more detailed description of the spectrum of rheologies and yield relations used. The authors provide a description of the usual power-law viscous creep deformation glaciologists are used to, but few equations describing the rheology beyond this. I recognize that the model used is fully documented in prior publications. However, the authors are introducing concepts that are new (or at least less familiar) to glaciologists and some hand holding is appropriate. There are also some details that are missing. For example, the 2D viscoelastic simulations are presumably done under plane stress or plane strain conditions, but I could not find which in the manuscript. (I apologize to the authors if I missed this in the manuscript.) More importantly, I would like to see equations describing the yield relations and some description of the assumptions. For example, the authors state that they use a Mohr-Coulomb yield strength. The typical interpretation of the Mohr-Coulomb yield strength is that materials fail when the maximum shear stress exceeds a threshold that depends on the normal stress and a cohesion parameter. This is occasionally interpreted as the initiation of new faults or the re-activiation of previously existing faults. Which interpretation are the authors assuming? Or does it not matter? Also, what happens above the yield strength? Does the yield strength denote a boundary between flow laws, as in a Bingham plastic? What happens once ice has failed? Does it return to behaving like intact ice if the stress decreases beneath the yield strength (as is true in a granular material) or does it continue to behave as dam-

aged ice once yielded, irrespective of the current state of stress?

Another question I have relates to tensile versus shear failure. For example, typically, we think of crevasses as tensile failure features, but the Mohr Coulomb failure envelope is usually applied to shear failure. (In the absence of a cohesive strength, a Mohr-Coulomb failure law implies no tensile strength.) How do the authors simulate tensile failure as opposed to shear failure? Are there different yield strengths used? Typically, faulting is more important in the Earth, but in ice people often focus on tensile failure. (We partially dispute this. See for example, Bassis and Walker, 2012, Proceedings of the Royal Society.). Moreover, failure envelopes in compression and tension are usually very different with compressive strengths much larger than tensile strengths. Is this accommodated in the model? Is compressive failure considered negligible?

There are also some technical questions associated with simulating yielded ice. We (and others) have found that the maximum shear stress criterion associated with Coulomb-like failure can be difficult to implement numerically. Instead, we (and many others) often prefer to use the effective stress (2nd deviatoric stress invariant). This is qualitatively similar, but corresponds to a Drucker-Prager granular material and not a Coulomb-granular material. I assume the authors are using the Coulomb criterion, but do the authors need to stabilize the method to avoid the numerical errors associated with the non-robustness of finding a maximum?

All of these questions leave me with an imperfect understanding of the physics assumed by the authors and this clouds my understanding of the results that follow from these assumptions. I suspect most readers will have similar questions and it will help tremendously if the authors step us through the assumptions and assumed physics instead of rushing us through to the results. In many ways, I think the physical model has much greater value than the preliminary results so I urge the authors to take the time to explain the model thoroughly to the audience.

2. Boundary conditions. The authors specify velocity boundary conditions at the left,

bottom and right edges of the domain. Specifying a velocity boundary condition at the bottom is a bit odd. Typically, we would specify a sliding law or, alternatively no-slip or free-slip boundary conditions. I'm a little bit worried that the velocity boundary condition contaminates the results. I would recommend either re-running simulations using a sliding law. We often like to do both the free-slip and no-slip conditions to bracket behavior when doing idealized experiments where we don't want to specify parameters in a sliding law. If this is unfeasible, then I think some additional justification for the boundary conditions is appropriate. If the authors maintain the velocity boundary condition the authors should plot basal shear stress. Basal velocities are specified to be reasonable, but does this produce realistic basal shear stresses? The authors also might want to consider using a free-slip boundary condition for the vertical displacement in the left side of the domain. This will avoid the weird abrupt decrease in ice thickness near the left wall.

3. Model numerics and comparison with existing solutions. The model that the authors are using is a complex viscoelastic model used to study solid Earth deformation. The model appears to have been well benchmarked agains standard solutions and so hopefully the model numerics is well understood. However, there are aspects of the numerics associated with the flow of ice that are not as well represented in the previous set of benchmark experiments. In particular, the mass weighting and damping to obtain stable solutions in the explicit integration of the Navier-Stokes equations (with inertia) does not appear to have been calibrated with ice is mind. This raises some questions about the appropriateness of the numerical parameters. The mass weighting method that the authors use to time step the Navier-Stokes equations is one of the those methods that gets periodically rediscovered. I would personally prefer if the authors made it clear that the mass weighted explicit integration is used as a means of avoiding the cumbersome and expensive task of solving of large-non-linear sets of equations and that individual time steps do not provide accurate solution to the equations of motions. The hope is that over long time scales the solution is approximately steady-state, which corresponds to the Stokes equations that the authors rely want to solve. Presumably,

one could use, say, a multi grid or other fancy numerical solver instead to find the solution to the elliptical set of equations. Having said this, it would be nice if the authors could show that the model that they use is able to reproduce existing analytic or benchmark solutions for glacier flow. There have been a number of model inter comparisons that the authors could consider. I'm agnostic to the choice, but it would be reassuring to show that under viscous conditions, the authors can reproduce standard solutions for velocities and ice thickness. The authors have probably already done this and so a few sentences or a section in an Appendix would be all that is required. If possible, it would be great to see some convergence studies to show that the results shown in the paper are not numerical artifacts or signs of instabilities. The figures in the paper show jagged ice shelves. I suspect that failure will look more realistic if the authors conduct higher resolution model runs.

4. Interpretation of model results: One of the most intriguing results that the authors obtain is that they produce basal crevasses under ice shelves. We tried to explain these features in a recent paper using a perturbation approach (Bassis and Yue, 2015, EPSL). We focused on viscous instead of brittle ice and found a long wavelength instability that could result in wide basal crevasses so long as the stress was sufficiently large compared to the confining pressure. In our formalism, we can also examine brittle failure by taking the limit that the flow law exponent (n) tends to infinity. When we do this we find that the dominant wavelength is of the order of the ice thickness. The growth rate of perturbations, however, becomes extremely large. This is a consequence of the fact that in our model, we assume the ice is isothermal. This implies that over long wavelengths, the strain rate and deviator stress are both constant with depth and the entire ice shelf reaches the yield strength at the same time. This raises the question of whether the results here are consistent or inconsistent with our (admittedly limited) analytic result? If not, what controls the rate at which brittle failure propagates. What control the spacing between basal crevasses? Incidentally, the perturbation analysis that we conduct is analogous to some of the original perturbation calculations to explain boudinage in rock by Smith and others.

5. Clarification of the role of elastic stresses: The authors make a really interesting point that despite the fact that elastic stress decay over long time scales, the fractures that result from elastic stresses remain important. Based on this, the authors argue that we need viscoelastic rheologies to accomodate failure. I don't disagree with the authors. However, if elastic stresses are important (through their role in promoting failure) then, unlike purely viscous flow, simulations become an initial value problem. What I mean by this is that in purely viscous flow we can initialize a model with an unrealistic initial condition. The unrealistic initial condition will generates shocks in the model that will relax over time and we typically either initialize a model in such a way as to not generate shocks or allow the model to spin up until those shocks have sufficiently dissipated that the model is no longer contaminated by these shocks. In a viscoelastic model with failure, it seems possible that the template for failure will be strongly controlled by the initial conditionâĂŤespecially if the initial condition is unrealistic and generates shocks. The authors are starting with simple wedges and allowing them to evolve. Do the authors obtain similar results if the model is first spun up to a quasi-steady state consistent with purely viscous flow and only then is failure allowed to occur? Do elastic stresses remain important if the model is started from a configuration in which elastic stresses have already decayed? What is an appropriate starting condition for models or is the initial condition not that important?

Incidental comments:

Page 3, near line 5 "Ductile fracture is initiated by the formation of distributed voids that eventually coalesce to form a macroscopic fracture". Laboratory experiments indicate that ductile failure growth through the nucleation and growth of voids does not occur in ice. Fractures instead usually propagate through the formation and propagation of micro-cracks. I think that is what we proposed occurs ahead of the rift in the Amery Ice Shelf. The void growth mode of failure occurs in metals (perhaps rocks as well?), but to my knowledge is inapplicable to ice under terrestrial conditions. There is, of course, the separate question of whether the macroscopic behavior of ice in glaciers can be simulated using a framework appropriate for ductile failure of metals. However, I would like the distinction to be made more clearly in the manuscript.

Page 8, left, right and bottom velocities are set to 300 m/a. First, I recommend using more physical notation, like inflow, outflow and basal boundary conditions, including left, right, bottom as the authors see fit. Second, the fact that the velocity is constant implies no bulk extensional stresses, which seems odd for a glacier. I would appreciate more description for the motivation for this set of experiments.

I'm less confident for the evidence of a sharp brittle-ductile transition at a critical strain rate. We clearly see tensile fractures at a range of strain rates, with the controlling variable usually stress. Of course, stress and strain rate are interchangeable if the ice is isothermal, but that is not often the case.

Page 1 Line 15: "We find that the use of a semi-brittle constitutive law is a necessary material condition to form the . . ." I believe necessary should be replaced with sufficient. I don't think the authors have proven that no other conditions are able to reproduce fields of basal crevasses. What they have demonstrated is that a brittle rheology is sufficient to produce this feature.

Page 3 Line 5: Usually brittle failure of ice is thought to be a consequence of high stresses rather than strain rates. See, e.g., Vaughan, Journal of Glaciology, 1993 "Relating the occurrence of crevasses to strain rates".

Page 3 Line 5: The point about ductile failure versus brittle failure is subtle. The coalescence of voids to form macroscopic fractures might actually be brittle. At the very least, the formation of these voids appears to be seismic. But the brittle failure that occurs may act like plastic or ductile failure over macroscopic length scales.

Page 3, Line 20 It seems odd to claim that models based on Linear Elastic Fracture Mechanics do not predict the correct stresses if their rheology is assumed to be purely viscous. By definition the "E" in LEFM corresponds to elastic so how can the rheology

be assumed to be purely viscous?

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