The Cryosphere - TC2016-88 *"Semi-brittle rheology and ice dynamics in DynEarthSol3D"* by Logan and others.

This paper present a new modeling framework to account for the semi-brittle (brittle and ductile together) behaviour of ice using the DynEarthSol3D model. Two applications are proposed, the first is a comparison of the brittle and ductile behaviours and the second an experiment with the semi-brittle behaviour. These two experiments are performed using very simplified setup (geometry and boundary conditions). I have three main concerns with this paper. I have also listed some minor concerns/typos that should be accounted for in a revised version.

Boundary conditions I am wondering how much the conclusions from the first experiment are related to the imposed kinematic boundary condition, especially the two orders of magnitude difference between brittle and ductile effective stress. Imposing a velocity field on three of the four boundaries of the domain conduct to stress that are not realistic at all. The flow of ice is gravity driven in reality so that I am not sure of what can be really learned from this first experiment. In other words, I am not sure that under realistic conditions (realistic geometry and boundary conditions) the two approaches would give so different stress field (because the global static equilibrium would be similar if not the same).

Description of the implemented rheology. The paper should be really improved regarding the presentation of the implemented rheology and failure criteria in the model. All this material should be consistently presented in section 2.1. Some of these aspects are described all along the manuscript whereas they should really be presented consistently in the model description section (e.g., the Mohr-Coulomb failure envelope, the fact that there is no failure criteria for the ductile behaviour or the expression for the ice effective viscosity are presented in the application section). Some part of the model are not described at all. For example, it is not clear what becomes the rheology when the Morh-Coulomb criteria is reached in the brittle approach? From Fig. 4, I understand that in fact there is no real failure of the material property of ice for the brittle rheology and that failure is estimated when a plastic strain is larger than 0.03? This should be really explained in much more detail.

Sensitivity to mesh quality The free surface for both experiments looks very jagged. It is mentioned page 9 line 16 that it is an artifact of the low resolution and that these features disappear with higher resolution. I don't understand then why the results with a higher resolution are not presented, especially when it is mentioned in the conclusion (page 13, line 23) that all the simulations presented here are computationally cheap! How much the results presented in this paper are mesh dependent? The geometries obtained after only 6 months of simulation and presented in Fig. 5 look so bad that I have some doubts that the simulation can be performed for a longer time before exploding? It looks like you have positive slope which would induce reverse velocity for a 2D flow line problem. How much the spacing of the "crevasses" presented in Fig. 4 is mesh dependent? All these feature (distribution of the plastic strain larger than 0.03, upper and lower surface ondulations) seem to be of the element size. Information regarding the mesh are really needed, as well as a clear study of the sensitivity of the results to mesh refinement.

Other remarks

page 2, line 8: add "e.g." in front of these references as they are not exhaustive on that subject. The same remarks apply at other places in the manuscript.

page 2, lines 10-14: the tone of this introduction is a bit naive? You are writing in TC, people have heard about calving?

page 2, line 17: there is nothing about LEF mechanics in Larour et al. (2012) paper.

page 2, line 19: or a mixture of both like in Krug et al. (2014).

page, line 23: over *very* short time scales?

Equation (1): define σ_e as well. The definition of the effective pressure should be presented here.

page 3, line 11: Most *ice-flow* numerical models

page 3, line 18: I don't agree that viscous model are not capable to represent ice failure and ice retreat. As far as I know (and some of these paper are cited in the present manuscript), there have been some work to include these processes.

page 3, line 26: the need of elastic stress to be accounted for is a bit affirmative and, as it is said in this paper, would need some modeling effort to really understand how important it is to account for them. Moreover, I think it really depends at which scale (time and space) you are interested, which should be mentioned.

page 4, line 14: some words in the introduction about particle models or discrete element models would be interesting (e.g. Bassis and Jacobs, 2013; Åström and others, 2014), and how they compare to the present approach.

page 4, line 17: the main issue of using a Lagrangian approach in glaciology relies in accounting for the in/outcoming flux of ice on the domain boundaries (accumulation and/or ablation on the surface, melting/accretion at the base). You should mention in the manuscript how this problem is (or will) be overcome for realistic applications.

page 4, line 19: FS models neglect acceleration because it is completely negligible for the time step of interest of many applications. In the proposed applications, it would be interesting to document the relative contribution of acceleration in the total momentum. Their importance, as stated here, has still to be proven?

page 5, line 5: avoid repeating "*of ice*".

page 5, line 7: from my experience, a Dirichlet BC is only required where you have an output flow and not on all the boundaries, as it seems the case here. Does it come from the Lagrangian formulation?

page 5, line 13: This sentence is not clear and looks technical more than related to the physics in the model? Which equation is solved for incompressibility should be given here, whereas how it is solved should be given in the following section.

page 5, line 23: it is not clear if the floatation is fulfilled for the floating part?

page 5, line 25: you mean an explicit time-stepping scheme?

page 6, line 6: are given in Choi et al. [2013].

page 6, line 14: no need to define again the minimum element facet length.

page 7, line 25: I don't really think this list of capabilities is relevant for the present paper

page 8, line 4: we divide this section (delete *the*)

page 9, line 4: the two order of magnitude differences in term of stress certainly is the result of the very particular boundary conditions applied here and therefore no real conclusion can be drawn from this setup regarding a realistic case (see major remarks).

page 9, line 5: (Figs. 3a and b)

page 9, line 9: (Figs. 3e and f)

page 9, line 16: So why not showing these better results obtained with an higher resolution? In any case, a sensitivity study of our results to the mesh resolution would clearly improve the strengh of the paper.

page 9, line 25: How much the spacing shown in Fig. 4 is dependent of the mesh. In other words, do you get the same spacing with a mesh with halved elements?

page 10, line 23: The most appropriate variable to write a criteria for damage would be the Cauchy stress, not the strain or strain-rate.

page 12, line 9: How would you account for basal melting in a Lagrangian model?

The basal geometry from Fig. 3 does not correspond to the setup presented in Fig. 2a. In Fig 2a it is a straight line over 10 km whereas in Fig 3 there is two lines that define the base (over the same 10 km)? I am wondering if the 10 km scale indicated in Fig. 3 is therefore correct? An horizontal scale in Fig. 4 would be helpful for the same reason.

The geometry in Figs. 5 and 6 look very mesh dependent and it would require some convincing arguments (i.e., a mesh sensitivity study) before moving to physical explanations about these modeled features as done in Fig. 7.

References

Åström, J. A., D. Vallot, M. Schäfer, E. Z. Welty, S. O'Neel, T. Bartholomaus, Y. Liu, T. Riikilä, T. Zwinger, J. Timonen and others. 2014, Termini of calving glaciers as self-organized critical systems. *Nature Geoscience*, **7**(12), 874–878.

Bassis, J. and S. Jacobs. 2013, Diverse calving patterns linked to glacier geometry. *Nature Geoscience*, **6**(10), 833–836.