

Response to referee 1: 'Ice shelf fracture parameterization in an ice sheet model' by Sainan Sun et al.

The referee is happy with this version of our manuscript. We would like to thank the referee for the review.

Response to referee 2: 'Ice shelf fracture parameterization in an ice sheet model' by Sainan Sun et al.

This manuscript presents an updated version of a prior manuscript that sought to include damage in the BISICLES model. The original manuscript was promising, but had some glitches and confusing portions. The authors, however, have done a commendable job of thoroughly addressing reviewer comments. With the exception of one point of minor confusion (which might be my own), I only have nitpicky and minor comments.

I would like to thank Dr. Bassis for the review and we will try to give a response.

The one semi-important point I have is with Equation 11. Here the role of mass balance on damage is still unclear to me. It looks like the authors are assuming that snow deposited to the surface will increase the ice thickness, but will not affect the basal crevasse penetration depth. Geometrically, this makes sense if we imagine a basal crevasse. Snow deposited onto the surface then increases the ice thickness, but leaves the crevasse depth constant. In this case, snow “heals” the crevasse—even if this is a purely geometric effect. This prescription fails, however, if there is a surface crevasse, but this manuscript is mostly concerned with basal crevasses so this is not a significant problem. In contrast, melt around basal crevasses appears to erode the ice around crevasses and also decreases damage. This would seem to assume that there is no melt within the crevasse and all melt instead occurs outside of the crevasse. (Not an unreasonable assumption.)

This is indeed our intention -- though we had thought that the healing term $(-\max(a, 0)) \frac{d_{tr}}{h}$ made sense for surface crevasses, too, e.g with the same depth of ice deposited inside and outside a surface crevasse. As the referee says, it is really basal crevasses that matter in any case.

This seems to be contracted, however, when the authors state that melt is assumed to be the same inside and outside of the crevasse. If the melt rate is the same inside and outside the crevasse, then it would appear as though the mass balance term should increase damage and by proportional to total melt rate (surface and bottom) and take the form $m \times \frac{d_{tr}}{h}$. See, for example, Equation 26 in Bassis and Ma (2015, but note the annoying typo where the r is missing from the second term). The reason the term appears this way is that if the melt rate is the same inside and outside the crevasses, the melt rate makes no

difference to the crevasse depth. The melt does, however, decrease the ice thickness. Hence, any (positive) melt rate will increase damage. It is possible that I have simply misread the authors statement, but I encourage the authors to check to make sure this statement isn't a typo and to make sure the equation is consistent with their description.

The referee is correct here, and our sentence was careless - correct for accumulation at the surface but not melting at the base. We changed it to 'Note that we do not attempt to include any additional accretion or ablation physics particular to the inside of crevasses'

Some really nitpicky comments:

Near page 2 line 10: "Macro-scale fractures originate from micro-scale cracks, which appear when viscous strain is too high" Is this really what Rist says? Normally we think of micro-cracks forming when **stress** is high. Of course, stress and viscous strain rates are related through the rheology. The technical problem with the statement above is that at first glance, this implies that for a given stress, micro cracks are more likely to form in warm ice than cold ice because the viscous strain rates are larger. This is not well supported by the literature and, in fact, we often see that crevasses form when the stress (after temperature has been accounted for) exceeds a few hundred kPa. More pernicious, large portions of ice sheets have very large viscous ***strains*** because small strain rates have accumulated over millennia. This does not necessarily lead to micro-fractures because of other processes (like recrystallization). All in all, this is a minor point. If the authors want to be particular about it, I would probably say something along the lines of "Macro-scale fractures originate from micro-scale cracks, which are more likely to form when stresses within the ice become large" or if you want to be more specific "Macro-scale fractures originate from micro-scale cracks, which are more likely to form when stresses within the ice exceed a few hundred kPa"

We take the suggestion of the referee and modify the sentence to be "Macro-scale fractures originate from micro-scale cracks, which are more likely to form when stresses within the ice exceed a few hundred kPa."

Near page 2 line 10: "microscopic scales fracturing is a discrete process which operates on time scales determined by the speed of sound in ice" This is a fine statement. The authors might be interested to know that new research is, however, pointing out that slow rupture at speeds that are significantly less than body wave speeds is also possible. This can happen through a variety of processes including void growth and ductile failure. It is, however, unclear that any of these mechanisms are at work in ice.

The sentence is modified to be "On microscopic scales fracturing is a discrete process which operates on time scales determined by the speed of sound in ice, and the rupture speed could be influenced by the local variations of stress state and material properties (Ye et al., 2016)."

Results: breaking the results section down into subsections (perhaps identified by the different experiments) would help readers follow the different model results.

We split this into sections now.

Page 11, near line 5. I think there should be a tilde on the D?

Yes, corrected.

Also, it is interesting that $D \sim 0.5$ appears to be an appropriate rule of thumb. This is, of course, the prediction for surface+basal crevasse depth that we would infer based on the Nye zero stress model in an unconfined ice tongue.

Right, but we don't want to mislead here, so we have changed ' $\bar{D} \sim 1/2$ at all times in the ice shelf' to ' $\bar{D} \sim 1/2$ at all times in the ice shelf, with lower values ($\bar{D} \approx 1/3$) close to the grounding line and in confined regions of the shelf'