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Interactive comment on "A statistical fracture model for Antarctic ice shelves and glaciers" *by* Veronika Emetc et al.

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1 General Appreciation

This manuscript describes a statistical method to predict the location of fractures in Antarctic ice shelves and glaciers. Overall, there is a welcomed novelty to the authors approach, in which they take observations of fracture and attempt to link these observations with dynamic variables predicted by ice sheet models. This type of analysis can be used to formulate an empirical model of fracture initiation and propagation or as part of a hypotheses testing program. This manuscript seeks to do a little bit of both of these, although the emphasis is on the former. People often have strong prejudices against the former, but this is largely a question of philosophy. I should acknowledge

that my sympathies lie towards the latter and some elements of my review may push the authors in this direction.

Overall, I like the idea of the manuscript, but I have a lot of comments. In particular, I had a very hard time following the methods and discussion and my review is going to focus on many of these elements. There were also quite a few typos or errors in the manuscript (sometimes it was hard to tell which) and I suspect that the manuscript will require a significant rewrite with the full attention of the senior authors to make the manuscript accessible to a wide glaciological audience.

2 Major comments

2.1 Observed fractures vs inferred damage

One of places where I'm confused is in the data sources that are ingested to compute where ice is fractured. The presentation in Section 2.2 led me to think that surface velocities are used to invert for damage and then damage is used as a proxy for locations where the ice is fractured. However, section 3 states that the authors use fractures when they are visible in satellite imagery. This is then confirmed in section 3.2 where it is stated that satellite images are used to determine when ice is fractured. As far as I can tell from reading the manuscript and figure captions, the authors used satellite imagery to identify surface fractures and these are shown as green dots in Figures 3 onward. The satellite imagery derived fractures were then ingested into the statistical framework and this is used to infer the probability that ice is fractured. My understanding is that damage is only used qualitatively to compare with the probabilities inferred. I really like the idea of using damage as an independent method to compare observed fractures with, but this should be emphasized early on. To make the method clear to readers, I suggest rewriting the methods and background section to introduce both the damage based method and the satellite based method simultaneously. Or postpone the damage section entirely until the discussion section since it is only used qualitatively and does not factor into the analysis. Given the qualitative nature of the comparison, I also wonder if the details of the inversion can be omitted and replaced with a suitable reference.

2.2 Location of surface fractures vs location of rifts and other fractures

I also had a hard time interpreting the location of both damage and fractures identified in satellite imagery. I'm going to focus my comments here on the Amery Ice Shelf and Mertz glacier tongue because I know both regions well. Looking at the observed fractures (green circles in Figure 5), I see quite a few observed fractures on the grounded ice, but very few on the ice shelf. However, I know there are significant crevasses/rifts that originate near Gillock Island on the Amery that form a long crevasse/rift train. There are also several rifts near the front of the ice shelf, including the Loose Tooth, that don't have corresponding observations identified. As far as I can tell, the rifts that are most likely to become detachment boundaries are not clearly represented in the dataset used to infer the locations of fracture! There seems to be some objective criterion used that, unless I misunderstand, doesn't include what I would typically think of a crevasse. Similarly, you can see from Figure 6 that the entire Mertz Ice Tongue is heavily fractured and yet these fractures are not represented in the dataset used. The probability inference is only going to be as good as the data ingested so it is important to explain why most fractures on the ice shelf appear to be ignored.

2.3 Location of damage vs location of rifts and other fractures

Similarly, the inference for damage for most regions does not fill me with confidence given the fact that inferred damage occurs in regions where there is little evidence of

fracturing and misses regions of actively propagating rifts. To make things more perplexing, there is very little agreement between the observed fractures and the damage that was inferred. What exactly is the damage supposed to tell us if it doesn't correspond to locations where there are fractures?In theory these two methods should provide independent confirmation of areas that are damaged. The limited overlap between these regions makes me question if the ingested data is limiting the applicability of the results. Here I'm not sure what to suggest, but I do think the authors need to address the discrepancy between observed fractures, rifts visible in MODIS/MISR imagery and inferred damage and what it does to the results presented. I do wonder if focusing first on a single region that could be studied in detail would be beneficial before attempting to merge many different regions.

2.4 Choice of variables used as predictors: Part 1 strain or strain rate?

I'm not sure that I understand the motivation for (or need) for many of the predictors ingested into the probabilistic framework . I should say that I like Tables 2, 4 and 5, which quickly summarizes the different variables considered and the dominant variables. These are great. The text describing the motivation of many of the variables is, however, hard to follow. To start, the authors appear to be confused about the difference between strain and strain rate. Strain is related to the gradient of the displacement. Strain rate is related to the gradient of the velocity. These are not the same thing. The authors note multiple times that they are looking at strains and principle strains (e.g, page 7-8). It is, however, unclear how they can get strain: do you accumulate strain rate over some interval of time? If so, what is the time interval? I think this could be a really interesting calculation, but after multiple readings I think the authors **might** really mean strain rate. This absolutely needs to be clarified.

2.5 Choice of variables used as predictors: Part 2 what is physical and what is not?

Some of the variables used as predictors are intuitive and have a long history of usage (often irrespective of whether they are supported by observations or not). Physical variables (in my opinion) include measures of the strain rate tensor and stress/deviatoric stress tensors. Most of the other variables included, especially the geometric variables should correlate with various measures of the stress and/or strain rate. This makes me wonder if these additional variables are needed to make up for deficiencies in the ISSM inferred values for things like stress. The authors also use some measure related to the gradient of the strain rate called strain change. Again, I'm not sure if they really mean strain or strain rate. But the gradient of strain rate, presumably converted to to some scalar measure, might be diagnostic of the presence of fractures rather than predictive. For example, fractures/rifts/crevasses lead to large gradients in the strain rate field across individual fractures. The fractures do not originate because of the change in strain rate. The change in strain rate is telling you that there are fractures present. I can understand why including this as a predictor would improve results, but the causality in this case is almost certainly in the wrong directly. I will also note that because ice is incompressible $\dot{\epsilon}_{zz} = -(\dot{\epsilon}_{xx} + \dot{\epsilon}_{yy})$ and Equation 6 is not obviously correct unless one somehow sets $\dot{\epsilon}_{zz} = 0$. Finally, given that the authors include the effective strain rate, why not also include the effective deviatoric stress invariant as a variable (or the Von Mises stress)?

2.6 Choice of variables used as predictors: Part 3 a recommendation

My suspicion is that all of the variables included in the statistical analysis were included because the authors found that they were needed to explain observations (but see my earlier question about the reliability of the observations). I wonder if it would be more physically useful to start with a simpler model that **only** considers one or two

variables. For example, can the authors prove that various measures of strain rate or stress by themselves are not sufficient to explain the observations? Two of the co-authors have made proposals (Borstad and Morlighem) that could be tested given an appropriate dataset. This alone would be a big step forward. Once, the authors demonstrate that stress/strain rate measures alone are not sufficient, then I think the authors could more easily motivate a more elaborate set of tests. But these could be motivated by regions where the statistical model fails. This would have the advantage of providing physical insight in addition to empirical predictions. (Again, note my bias here towards hypothesis testing.) For example, flexural stresses near the grounding line/pinning points are key features that could result in fracture formation and these processes are not included in ISSM. If this is the case I would expect that fractures in these locations would not be resolved. One could then include additional variables that could diagnose flexural stresses. The advantage of this approach to someone like myself, that is mechanically inclined, is that it tells me about the processes that are important and need to be included in models.

2.7 What about fracture advection?

There is also an issue that the authors hint at, but don't quite address which is that fractures advect after they form. A consequence is that places you observe fractures may be far from the places they are observed. Because stresses and strain rates have not been constant, this means that the state of stress when a fracture formed could have been very different than it is now. Moreover, the fracture could evolve based on the integral of strain rate/stress tensor invariants over the life time of fractures. Some of the fractures observed may be diagnostic of stress regimes hundreds or thousands of years ago and hence not that useful to the analysis.

2.8 What about a yield strength?

Most theories suggest that fractures form when some measure of the stress (or in some cases, strain rate tensor) exceed some material dependent parameter. It is not clear to me how this type of threshold behavior is incorporated into the statistical model. What happens when a larger (smaller) strain rate doesn't lead to more (less) fractures, but that there is an abrupt (or rapid) transition centered around some yield envelope?

2.9 Tables and Figures need a bunch of work

As I said, I like some of the Tables, but I don't see the point of Table 1.

Figure 1: I don't understand the colors or the content. For one, the legends have colors that don't correspond to the colors of the figure (e.g., Fig 1a has pink and green legend but bars are pink and brown).

Figures 3-10 need better captions and some attention. - What do the cyan boxes represent? - Can you include a small location box for each region? - What does it mean to show Group X for a particular region? - Figure 5 (a) appears to denote a place called Pain Isalnd. I am going to guess that is supposed to be Pine Island. - Figure 5b and c know show two different views of the Amery Ice Shelf with different color scales, but the caption tells us everything is identical to previous figures. I'm guessing that Figure 5c shows inferred damage. This needs to be in the caption. Figure 5c also seems to have some red dots that aren't described in the caption. - Why is damage only introduced in Figure 5 onwards? Why not earlier? What am I supposed to see in these figures? - In Figure 5b-c, the entire area around Gillock Island appears to have no observations or model results. This makes me wonder what was used as boundary conditions and how reliable the results can be given that many crevasses originate around Gillock Island. - Figure 6, now you tell us that red dots denote fractures that were filtered out due to damage upstream. What does this mean? Is this the same in Figure 5? This figure is

extremely frustrating because Figures 6a and 6b appear to show the same ice tongue, but the size and orientation are completely different making it impossible to compare. - Figure 8, again with the cyan boxes? What do those represent and why aren't they included in the captions? - Why not use the same color scale for probability as damage to make it easier to compare?

2.10 Writing and style

There were quite a few typos in the manuscript and these need to be fixed to ease the exposition. Given the number of typos I wasn't sure if some of issues I found were typos or errors (see strain vs strain rate). The manuscript needs a very careful scrubbing and editing to tighten the prose. This should be supervised by the senior authors of the study. The context around different approaches is not entirely correct in the introduction and background section. To my knowledge **no** method has been able to simulate the diversity of calving regimes observed. Damage mechanics is, in theory, able to simulate failure of grounded and floating ice. However, the approaches cited rely on small scale laboratory data, which may not apply to large scale glaciers. Moreover, viscoelastic damage mechanics is an approach that if often used to simulate the propagation of individual fractures. This can be prohibitive in large-scale models. Hence, the approach by Borstad et al. As far as I can tell, the approach by Borstad works really well for the Larsen ice shelves and is quite promising for ice shelves in general. I don't think this approach has been applied to grounded ice before. Similarly, the efforts by Levermann (eigen calving) seem like they work OK for floating ice. The Von Mises criterion (Morlighem) seems promising for grounded ice.

3 Technical comments

3.1 How damage is defined and calculated?

Damage is implicitly defined in equation 1, but the assumptions are a bit unclear. The stiffness parameter is a strong function of temperature. However, for ice shelves, we can often approximate the flow as plug flow and thus the stiffness parameter that is relevant is the depth averaged quantity:

$$\bar{B}_i = \frac{1}{H}\int_b^s B_T(z)(1-D(z))\mathrm{d}z. \tag{1}$$

where H is the ice thickness, s is the surface elevation and b is the bottom of the ice. Both temperature and damage will depend on the vertical coordinate z and the integral cannot be done analytically. In the special case that damage or temperature is constant with depth, the integral can be done analytically. As far as I can tell, the authors are assuming that damage is independent of depth and thus they write:

$$\bar{B}_i = \frac{1}{H}\int_b^s B_T(z)(1-D)\mathrm{d}z = (1-D)\bar{B}_T. \tag{2}$$

and this leads to Equation 1. Given how little we know about damage in general and its depth dependence in specific this is perhaps a plausible assumption. However, the interpretation of constant damage with depth differs significantly from the observation of surface fractures in satellite images where it is unlikely that all fractures penetrate the entire ice thickness. This might explain why damage has little relation to observed crevasses and merits some comment.

3.2 Damage inference

There is also an issue with inferring damage based on the viscosity. The inferred value will depend sensitivity on ice temperature. Errors in assumed ice temperature will contaminate the damage calculation. That is inevitable, but should be acknowledged. What is exciting here is that the authors appear to have independent estimates of damage from satellite observations and this suggests that damage can be compared independently (subject to the many above caveats). I would personally like to see more of this, but that might be a different manuscript. Given my previous comments about the weird places damage is inferred, I do wonder if the damage calculated for some of the locations is fiercely contaminated by bad temperature estimates. Damage on the Amery Ice Shelf seems to be especially suspicious. However, because damage is always less than unity and, I assume errors in ice temperature are more gaussian distributed, one might be able to examine the frequency with which the model would prefer an ice viscosity that is stiffer than inferred from the temperature field alone (negative damage). If this is vanishingly rare than one would have significant confidence in the damage estimate. Perhaps that is what is going on in some places, like the Amery, where damage is inferred in unphysical locations?

4 Minutia

- Low friction can lead to larger tensile stresses, but won't this also lead to larger tensile strain rates? If strain rates and stresses are included in the model this seems redundant.
- Why does ice stiffness factor into the calculation? The fracture properties of ice have little sensitivity to ice temperature? I wonder here if this is getting at problems with estimating the temperature of ice in ISSM.

- Page 2, line 11: “There have been a number of approaches that successfully modelled rift formation on particular ice shelves,” Really? I’m not sure that anyone has successfully modeled rift initiation and propagation.
- Page 2, line 21: this observations-> these observations
- Page 2, line 15 and down. This seems like discussion/abstract/results and without knowing more about the method is a bit confusing. I suggest moving this to later in the paper.
- Page 3: Discrete Element Models are also used to predict short-term calving events
- Page 3: “There are a number of other studies that proposed other calving laws (Pralong and Funk, 2005; Duddu and Waisman, 2012), but they might be not applicable in a generalised large-scale case.” This is probably true, but is as much true as any of the other methods. These continuum damage mechanics methods can model the propagation of crevasses in the vertical and horizontal directions, but rely on calibrating to old and-perhaps-unreliable laboratory data. These methods can include hydrofracture and other modes of failure, but have largely been applied to grounded calving margins. The methods by Borstad in contrast, are calibrated to field data and have been applied to ice shelves, but not grounded calving margins. It isn’t obvious to me how to include hydrofracture in the Borstad method.
- Page 4, paragraph near line 30: I don’t think Duddu and Waisman applied their model to any specific model in Greenland. This was largely a prototype model that was applied using idealized geometries. The model, however, was calibrated to laboratory data and in theory this data should remain valid for any loading situation.

- Page 6, line 10: What units are the friction coefficient and what sliding law was used?
- Page 7, line 13: What do you mean velocity gradients? Strain rates are related to the gradient of the velocity. Do you mean that you also include vorticity as a predictor or do you mean the gradient of the strain rates? Also, how do you measure strains as opposed to strain rates?
- Page 7 line 26: missing space after Each
- Page 8, line 10: again how can you calculate strains from a viscous model? I can see how to get strain rates from ISSM, but strains seem to require an elastic component that is missing.
- Page 8, line “to model a gradual viscous process strains have to be taken into account” I don’t understand this statement. What is a gradual viscous process and what does it have to do with strains. Viscous processes are usually a function of strain rates.

And this is where I stopped noting small quibbles with wording.

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