

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

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AC: We thank Jeremy Bassis for his careful and insightful review of our paper. His comments significantly improved the clarity of the manuscript.

RC: 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 author's 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. Peo-

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ple 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.

AC: We revised significantly the sections "Ice Sheet System Model (ISSM) Model setup" and "Methods" following the reviewer's advice.

RC: . 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.

AC: This is not entirely correct. We do not use damage as a proxy for locations where the ice is fractured. Damage is used only for qualitative comparison against our model and observed fractures.

RC: However, section 3 states that the authors use fractures when they are visible in satellite imagery.

AC: Correct. We do use observed fractures to construct our statistical model as well as a ground-truth for our modelled results. We do not use damage to construct our statistical model. We moved the whole section discussing damage-based method to the end of the methods section to make it clearer that the damage modelling does not form a part of our model.

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RC: This is then confirmed in section 3.2 where it is stated that satellite images are used to determine when ice is fractured.

AC: Correct.

RC: 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.

AC: This is essentially correct. However, we do not compare damage with probabilities. We state in section 3.6 of the paper: "We do not compare our probability-based model with the damage model directly; rather, we evaluate their respective ability to predict the formation of fractures in ice." We added the following sentence to clarify our methodology: "We utilise the damage-based model as an independent method in order to compare it with the observations of fractures and to identify areas where it can and cannot accurately predict the presence of fractures." (we clarified the text in other places as well, see below)

RC: I really like the idea of using damage as an independent method to compare observed fractures with, but this should be emphasized early on.

AC: We have added a sentence in the end of the introduction section to make this clear.

RC: 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.

AC: We chose the latter option. We moved the description of the damage method to the end of the methods section.

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RC: 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.

AC: A previous reviewer suggested that we should include the equations, therefore we would like to keep them.

RC: 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.

AC: Some fracture observations might be left out of the set we used due to the fact that we identified the location of all the fractures manually, however missing a few fractures would not significantly affect the statistical model as a whole. We already explained this in the paper (section 3): "We did not need to select all the fractures on the ice sheet surface to build the statistical model but, in order to compare the results of our model with observations, we constructed extra data sets where we made a concerted effort to select all the visible fractures on the ice surface. It is possible that some fractures were missed due to the large spatial extent of the experiments."

RC: 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

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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.

AC: This is an important point that we probably did not explain well. The definition of rifts and crevasses is different, and our paper attempts to model fracture processes, not rifting. We have now made this clear in the beginning of section 3.

RC: 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?

AC: We agree. The focus of our paper is on the ability of our model to match the observed fractures, not on explaining why the damage approach can not always match the observations. RC: 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.

AC: Because we do not rely on the damage predictions to construct our model, the limitations of the damage approach do not affect our results. We stated this indirectly in section 3.6 where we indicate that our model construction depends only upon the probabilistic approach.

RC: 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.

AC: The focus of our paper is on the performance of our own model, not of the damage modelling that has been published previously. The aim of this paper to develop an al-

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ternative method that can predict fractures well. We do not want to change the focus of our paper. To reduce the apparent importance of the discussion of damage modelling, we have moved it to the end of section 3.

RC: 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.

AC: The statistical approach does not work well if the data are taken from just a few glaciers, therefore working with many regions is absolutely necessary. The wider the range of data, the more robust the statistical model. Added to section 3: "This large number of ice shelf regions was chosen due to the fact that the statistical approach does not work well if we train the model on too few glaciers. Taking a wider range of input data (using different regions in Antarctica) is more important than having a larger number of similar data points. Variety of observations of different regimes improves the reliability of the statistical model, as the diversity in sampling provides a better estimation of correlation coefficients for the statistical model (called β coefficients in LRA). Thus, by choosing multiple glaciers we can more accurately construct an approximate surface that separates fractured from non-fractured nodes (the plane is determined by β coefficients)."

RC: 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.

AC: We added an enumeration in the text to make this part clearer. We also revised the text to improve clarity.

RC: 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

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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.

AC: We indeed use strain rates, thank you for pointing this out. We modified the paper accordingly.

RC: 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.

AC: this may be true, but is not entirely relevant. Our statistical approach makes use of whatever indicators are available and relevant. If a particular parameter was superfluous then the LRA would, by construction, ignore it and that parameter would not have been included in our optimised set of predictors. This circumvents the issue raised here, however, we added an explanation of this in section 3.3.1.

RC: 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. we meant strain rate. This has been corrected.

RC: But the gradient of strain rate, presumably converted 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

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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 direction.

AC: We agree with the reviewer that the change in strain rate tells us that there are fractures present. This is precisely why our new approach works. We use this as a predictor precisely because it tells us where we can expect fractures. Our method does not attempt to understand the process by which the fractures formed. Rather, our aim is to identify where they form. We would still discover new regions where crevasses form even if they were not observed there in the first place for two reasons. First, if there are no observed fractures but the strain rate is high it means that fractures are not visible but should be still there (maybe covered in snow or bad resolution images). Second, if strain rate is small we have other predictors in the model to tell us if there are fractures or not. Added to section 3.3 (iii): "It is important to note that the change in strain rate is not the cause but the result of presence of fractures. However, the aim of our study is to identify where fractures are present without attempting to fully describe the process by which they are formed. Using the strain rates as a predictor we would still discover new regions where crevasses form even if they were not observed there in the first place for two reasons. First, having no observed fractures but high strain rate means that fractures are not visible but should be still present (they can be covered in snow or not visible due to bad resolution of the satellite images). Second, if strain rate is small we have other predictors in the model to tell us if there are fractures or not."

RC: I will also note that because ice is incompressible and Equation 6 is not obviously correct unless one somehow sets $\epsilon_{zz} = 0$. 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)?

AC: We corrected the equations

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RC: 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

AC: Following the reviewer's recommendation we ran additional experiments with only stress measures.

RC: 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.

AC: We added to the Methods a new subsection "Test runs with a small set of parameters" and included plots showing a number of test results obtained when only using the effective stress, when using principal deviatoric stresses and the last when using von Mises stress expression. We now show in this section that including only stress parameters does not yield a sufficiently accurate model.

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RC: 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.

AC: The direct modeling of advection of the fractures is beyond the scope of this research. We do not know just by looking at the images if the small crevasses we select are initiated or advected in that particular location, therefore we model surface crevasses without distinguishing if they are advected or initiated ones. However, the fact that what may be advected fractures are still visible provides information about the particular conditions at a location.

To clarify this, we have added: "Our main goal is to determine the most likely location of fractures without focusing on their initial source, since we can not claim if the observations consist only of initiated crevasses. Although we do not directly model advection, the statistical model predicts the presence of fractures (both initiated and advected fractures without distinguishing one from another). The question that arises then is how do we know that the flow regime conditions that caused opening of the fracture are the conditions in the observed point and not the conditions upstream from the observed fracture (in case of advection)? However, even if an observed fracture was not formed at a particular location, but was advected with the ice flow it is still visible on the satellite image. The fact that fractures can be seen indicates that there are factors that act to permit the fractures to exist, whether they formed in that particular location or remained open after being advected from upstream (since another combination of factors could close the fracture).

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RC: 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.

AC: This was an accidental rotation of the figure, which we have now corrected.

RC: - 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? a previous reviewer specifically asked for the information to be provided in a different colour. We have chosen to leave it as it is.

RC: 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 the 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.

AC: The manuscript has now been read carefully by all authors.

RC: 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 is 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.

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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:

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: $BT(z)(1 - D)dz = (1 - D)BT$. (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.

AC: This is a good point, but again we iterate that the focus of our paper is not the damage results but the agreement of our new statistical approach with the observed fractures. Nonetheless, we added: "We use the method suggested by Borstad, 2016 where it is assumed that damage is independent of depth." to provide clear reference to the source of the damage model used.

RC: 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.

AC: This is true, and we mentioned it the paper already: " Estimation of B_T is the source of the main uncertainty in damage calculations due to the lack of ice temper-

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ature data, which can be crucial in affecting the accuracy of the viscosity parameter". To make it clearer we added: "Thus, the errors in assumed temperature may affect the inferred value for damage."

RC: 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?

AC: This is interesting, but is not the focus of our paper. We have chosen not to add such information to our manuscript.

4 Minutia

RC: 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.

AC: In all cases, each group includes either strain rates or friction, not both at the same time. We run an experiment replacing friction by strain rates and found that the prediction success for some glacier decreases by about 5%. We clarified this by adding information on additional experiments that we conducted: "Moreover, including both friction and strain rate is ambiguous since lower friction can lead to a larger strain

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rate. However, by looking at the predictor data sets, we found that the optimal choice of parameters for each group includes either friction or strain rate, never both at the same time. We ran an experiment replacing friction by strain rates and found that the prediction success for some glacier decreases by about 5%. We therefore kept only friction as a predictor parameter.

RC: 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.

AC: The reviewer is probably correct here. We don't include ice temperature in our model because the resolution of the values is not good. We found that using ice stiffness is a better indicator. We added: "In addition, we include the stiffness of ice as well as thickness due to their physical relation to fracture mechanics. When ice stiffness is high and ice crystals cannot creep fast enough, fracture might occur. Therefore, this parameter (obtained from the inversion of velocities implemented in ISSM) is added as a predictor. Adding temperature directly into the analysis did not improve the prediction results, which might be due to the uncertainties in the temperature estimation "

RC: Page 6, line 10: What units are the friction coefficient and what sliding law was used?

AC: The units is $\sqrt{s/m}$, however this section has been removed in our editing of the manuscript. This has been added: Budd sliding law (Budd et al. 1979)

RC: 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?

AC: We have modified the entire manuscript to refer to only strain rates, not strains. Also, we removed "velocity gradients", since these are not used.

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RC: Page 7 line 26: missing space after Each

AC: Corrected

RC: 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.

AC: It is strain rates. This has been modified everywhere in the paper.

RC: 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.

AC: Corrected to strain rates

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