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Discussions

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

Anonymous Referee #2

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This manuscript describes a new probabilistic method for representing the location of fractures in Antarctic ice shelves (although the title indicates "glaciers" it is really ice shelves that are the focus). Rather than focus on the physics of ice fracturing, the proposed approach lumps as many observational factors as possible into a probabilistic framework which then searches for a best-fit combination of factors that produces a probabilistic field (analogous to damage) that agrees with observations of fractures individually picked from satellite images. The method is compared to a selective use of continuum damage mechanics, and the authors claim that their new approach is much better.

The introduction and background is rather meandering, and paints a quite critical picture of the use of fracture mechanics and damage mechanics for representing fractures

in glaciers. Indeed, the naive reader might be left with the impression that these methods are completely arbitrary and without merit. It does not appear that the authors have a very thorough awareness and understanding of the damage mechanics literature as it has been applied in recent years to glacier and ice shelves. Inversions for damage of the type presented in the manuscript only produce damage in areas where fractures are actively forming or widening. It is important to distinguish between the "high-advection lifecycle" and "low-advection lifecycle" crevasse definitions of Colgan et al. (2016). If crevasses have advected far from where they formed, then the appearance of a crevasse is not a representation of local stresses/strain rates! In the presented statistical method, no difference is made between where fractures are initiating versus where they have been advected for long distances after initiation. This history-dependence is very important, and is a key shortcoming of the present approach. It also makes the comparison with the damage inversion a sort of apples-vs-oranges comparison. The two approaches shouldn't necessarily produce the same thing.

As for the statistical approach, there are many arbitrary and strange choices in its formulation. It seems as if every possible observational factor has been thrown into the mix just to see what comes out. Surprisingly, factors like the principal stress axes (components of unit normal vectors) and proximity to mountains end up being predictive, even if they have no physical relation to fracture mechanics, which should be the foundation of even a statistics-based fracture model. In the end, the most heavily influential factors in the statistical model are factors that would lead to higher stresses (and thus higher damage or predictions of crevasse depth) in a properly formulated and initialized model. This would seem to actually argue in favor of continuing with physical models such as the continuum damage mechanics models that are roundly criticized in the manuscript.

The manuscript would benefit from a careful rewrite to avoid the use of vague or unscientific language, and to better describe the background material and theory. The modeling methods and results need to be described and shown in much more detail in order for a competent peer to be able to attempt to reproduce the study and check its findings.

Modeling description

The description here of the use of damage mechanics is lacking in sufficient detail both to judge and to be able to reproduce the study. The authors are very vague in describing how they calculated damage for comparing to their statistical results. I am suspicious of the damage results, although there is simply not enough information for me to say one way or another whether their results are reasonable. I am not convinced that the new statistical model is actually better, in any quantifiable sense, than a properly formulated damage model.

The description of the modeling using ISSM is incomplete and vague. You mention that you use ISSM "to compute factors such as velocities, stresses, strains, backstresses, the dynamics of the ice sheet in time as well as friction coefficient and viscosity (calculated from inverse modelling)..." This language ("factors such as...") is too vague for such a methods description. Describe specifically what you calculated and how you calculated it. Calculating "the dynamics of the ice sheet in time" could mean anything from modeling one year of an ice shelf to modeling a paleo-reconstruction of an ice sheet. Did you really model transient ice sheet dynamics?

More information is needed on how the model geometry is initialized. How was the grounding line determined? Did you use a mask other than that provided by Bedmap2 for defining floating/grounded portions? This is important, as Bedmap2 is missing many ice rises and ice rumples that are very important for ice shelf dynamics and particularly for stress calculations (see for example Furst et al., 2015; Matsuoka et al., 2015).

In order to invert for damage, you must have a reasonable idea of the ice temperature in order to parameterize the flow rate factor ('B' in Glen's flow law). Uncertainty in the temperature directly translates to uncertainty in the inferred damage (Borstad et

al., 2013), as the inversion relies on determining a limiting value of strain rate that you can expect for ice at a given temperature. Therefore, you need to describe how you initialized the value of 'B' for the inversion (did you use a temperature field? Or a uniform initial 'B'?).

In fact, it is not clear which of two different methods for inverting for damage you have chosen. Borstad et al. (2012) inverted for D directly, whereas Borstad et al. (2013) calculate D as a post-processing routine after inverting for the rate factor 'B'. The latter is a more robust method of determining damage (and backstress, which the authors mention briefly without describing how this is calculated). I have no idea how you calculated damage, or how much confidence to have in your results since you didn't provide any details.

Inverting for basal friction and ice rheology involve a lot of assumptions and parameters, which are characteristics you used to criticize damage mechanics (all models involve assumptions and parameters). How did you initialize your inversion? Inversion results depend on reasonable initial guesses, so how did you initialize the basal friction? And the flow rate factor B? And D_o if you inverted for damage directly?

Did you use regularization to penalize sharp gradients in the inversions? If so, how did you determine the appropriate level of regularization? What metrics of goodness-of-fit did you look at to measure the goodness of fit between the modeled and observed velocities? Such metrics should be reported for comparing to other studies, otherwise the reader has no idea whether your initialized model, upon which your entire analysis is based on, is any good.

From lines 27-20 it seems that you have both a melting rate and a thickening rate on floating ice? How does this work? Where does this come in to your analysis or calculations?

Figures

Figures are hard to read, the color scheme is rather difficult to interpret (and I'm not color blind, but the figures are certainly not color-blind friendly; choose a perceptually uniform color scale). The scale is too large to be able to interpret much detail in the difference between the damage and probabilistic approaches.

The grounding line needs to be shown in the figures.

There are many fractures visible in your figures that are not represented with the ticks that you have placed for "identified" fractures. I'm not sure I have confidence in your "observations" against which you are comparing the different models.

Line-by-line comments:

P1, L9: 50% improvement in what specifically?

P1, L10-12: what kind of grouping? What insight is gleaned from this?

P1, L21: not sure the reference supports the assertion that ice shelf calving is the biggest source of uncertainty in sea level rise estimates

P2, L2: not sure this reference supports the claim about increased calving from 1998-2003 and concerns about ice sheet stability

P2, L3: increased with respect to what? some reference amount or time?

P2, L6: again, what do you mean by "increeased" calving?

P2, L17: the correct term is "Linear Elastic Fracture Mechanics"

P2, L22: you need to be more specific when criticizing previous work: approximation of what? how is it "only" first order? describe what this actually is.

Equation 1: no need to show this equation if you are just describing it among other methods of representing calving. Either show equations for all methods, or none.

P2, L28 (and L31, and elsewhere): avoid staring sentences with "Also, ..."

P2, L30: this is important background material, especially since you are critical of damage mechanics and use it to compare with your new method. The table is helpful as a reference, but you should describe the different approaches in the text here. Describe what "limitations" and "uncertainties" are associated with these methods, especially since you state that these are reasons to doubt them.

P3, L2-3: which is it? where they are located or where they initiated?

P3, L14: damage mechanics is not limited to "small" fractures

P3, L14-15: this is incorrect and misleading. Damage mechanics is much more general. You might be warranted in criticizing one particular study for its limitations here, but you cannot cast doubt on all of damage mechanics in this way.

P3, L18: "very high frequency of fracturing" needs a reference or better description here, this is quite vague.

P3, L18-19: this is the whole point of using inverse methods to infer the location of damaged ice, as you don't need to "see" the fractures from visible imagery, rather you infer them from their influence on velocities/strain rates

P3, L21-11: LEFM isn't about fracture propagation actually, people just assume that fractures will propagate until the stress intensity factor falls below some value, but this is not actual fracture modeling.

P3, L22-24: this is too vague of language for a scientific paper. You need to describe the crevasse depth criterion rather than just name it in quotes. You have to say what you mean by "interesting" results, and describe the "different mechanics" that preclude its use for ice shelves.

P3, L28: damage mechanics is about both the initiation and subsequent evolution of fractures.

P3, L31: Linear Elastic Fracture Mechanics

P3, L32: what do you mean that calving occurs “mainly” at the ice shelf front?

P4, L1-10: Borstad et al (2016) showed how to model damage evolution in an ice shelf based on stresses. Although this didn’t explicitly treat calving, it provides a foundation for calving using damage evolution.

P4, L12-13: unjustified statement. This method has not been applied to ice shelves, so you have to support your claim that this can help understand calving in Antarctica "much better"

P4, L13-15: not true! look at Borstad et al. (2016) for modeling ice shelf damage. Also, this is not really true in general, a properly parameterized damage model doesn’t need to "know" where fractured zones are in advance

P4, L18-20: unsubstantiated claim, damage mechanics has been successful in modeling very complex fracture processes in a wide range of engineering and natural materials... this is a disingenuous characterization of damage mechanics to suit your needs here

P4, L21-22: all methods have uncertainties, and this is not a reason to criticize them. You have to be a lot more specific.

P4, L25: see Borstad et al (2016)

P4, L26: this is value language and sounds unscientific. You need to be much more specific when criticizing other models/papers. All models have assumptions and uncertainties, and this is especially the case for some of the decisions you have made about setting up your statistical model.

P4 L29-30: this is only the case when inverting for damage, not in general damage evolution or when using the method described in Borstad et al. (2013) for calculating damage as a post-processing step after an inversion for the rate factor ‘B’. It’s not actually clear which you did, and the latter does not require selecting an initial damage.

P6 L20: it’s unclear how and why you used the components of the principal stress axes, which are components of a unit normal vector describing the direction of the principal stresses. As this is a 2-component vector, how did you use these components? Individually, or combined somehow? What does a unit vector have to do with fracturing?

P7 L15: this should be strain rate (with a dot)

P7 L16: where did the strains come from? Why did you use strains?

P7 L19-20: not true: the axis defines the unit normal in the direction of the stress. The sign of the stress determines whether it is compressive or tensile.

P8 L1: what is a “vertical bend”?

Equation 9: of course, this would be implicitly accounted for by increased stress/strain rates in a properly formulated physical model.

P8 L12: completely arbitrary, it seems. And you criticized damage mechanics for having uncertainties and arbitrary factors?!

P8 L13-16: you need to show this in the results. Has this been found before? What might be the physical reason for such a grouping of fractures? What time range of satellite data did you look at, e.g. is this a robust finding in space and time?

P9 L25-26: but this says nothing about the depth of the fracture, so an insignificant surface crevasse will be represented the same as a through-thickness rift?

P10, L4-5: this seems like a needlessly simplistic definition, and avoids using the knowledge we have about fracturing: fractures are a response to high stresses! You cannot say that some node has a 50-50 chance of being fractured if you have modeled ice stresses but have just chosen to ignore them.

P18 L23-25: not true, these are geometric factors that influence the stresses, which can be resolved in a properly-formulated physical model

P20 L13: this is not true, a lot of fracture mechanics pre-dates this work

References

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