Review of "Inland migration of near-surface crevasses in the Amundsen Sea Sector, West Antarctica"

In this study the authors develop a method of mapping crevasses on the grounded ice of the Amundsen Sea Sector of West Antarctica and use airborne radar data to assess the depth of the crevasses they identify. The crevasses are shown to be in the upper part of the ice - largely confined to the firn. They also report changes in crevassing on the grounded ice across the Amundsen Sea Sector and suggest this is linked to changes in the dynamic behaviour of the region's ice streams since 2015. They go on to investigate the tensile strength of the ice surface (using a von Mises stress-criterion model) using their crevasse data and satellite-derived ice velocity data, and find strengths compatible with porous firn - estimated using a simple model fit to data. Overall, I think there are many good things about this paper: what we can see of the crevasse maps look good, the methodology for generating them is explained well and the data is validated; the conclusion that the crevasses on Thwaites rarely penetrate deeper than the firn is convincing and timely, given the current discussion regarding the impact such crevasses could have on ice flow. Similarly, the conclusion that surface fracture occurs according to a von Mises-like criterion is promising.

However, some of the ideas in the article are not developed fully or demonstrated as convincingly as they could be. It is possible that some of this is to do with the presentation of results, which should be improved throughout. I think the paper on the whole might try and do a little too much. For example, it doesn't benefit from the inclusion of the firn model. Hence, I believe it should be published, and will be useful to the community, but requires a number of revisions before publishing.

Changes in crevassing:

In general, some more work is required to demonstrate that the result in the title is widespread. This could be improved by showing a greater number of crevasse maps, rather than focusing on a small portion of Thwaites Glacier (as in Figure 6) and some large scale statistics like crevassed surface area (as in figure 7b-c).

The inland propagation of crevasses is shown in the main paper only in figure 6, where we can see some evolution on the south-eastern side of the crevasse field between March and June 2019. However, the larger region in Figure 1b (where the colours are labelled the other way round, or perhaps incorrectly?) shows a more varied picture with some locations showing that crevasses have been advected downstream and not replaced, and others where crevasses have developed further upstream.

Figure 7c shows that the crevassed area on Pine Island Glacier (imaged by path:53, frame:857) increased by 25% between 2016 and 2023. This is a big change so it would be great to see some maps showing the difference in crevasse area at the start and end of the timeseries, especially as there is no indication of uncertainty in the plots.

There is quite a lot of talk about correlation of increased crevasse area with changes in speed or increased surface strain rates. However, we are not presented with any data regarding changes in speed or surface strain rates in the ASE and no analysis linking this to changes in crevassing has been shown.

Firn model:

I found section 2.3 a bit difficult to read, and the diagram in Fig. 2 is not particularly illuminating. However, the idea is pretty simple and I think can be reduced to something like the following argument:
The porosity P of the firn is defined as the ratio of the volume of air contained in the pores to the total volume. The fractional area of air intersected by a plane is then $\sim P^{2/3}$. Hence, the tensile strength of the firn, which is proportional to the area of ice intersected by a plane normal to the background tensile stress, is reduced by a factor $\sim (1 - P^{2/3})$ compared to firn of zero porosity. At high porosities, some of the filaments of ice separating pores from each other break and the pores can merge. To account for this, we follow Jelitto and Schnewider (2018), and introduce an additional factor $(1-P)^n$ where n is a parameter we fit to data from laboratory experiments. Hence, we find:

$$\frac{\sigma_f}{\sigma_I} = (1-P)^n \times (1 - P^{2/3}). \qquad (1)$$

This model does not constitute a major novelty of the article (as it is described in Jelitto and Schnewider (2018) and is not validated by tensile strength estimates later) so this argument could be moved into the discussion where it could sit alongside figure 9.

However, I am not sure the description of the firn model and figure 9 add much to the article, so would just suggest removing them. The porosity of the firn is not known (at least, it is not shown in the article) so what figure 9 serves to do is show that the tensile strengths found from the estimates of surface stress are not inconsistent with firn described by such a model. This is fine except that these data are compatible with pretty much any model where tensile strength decreases monotonically with porosity (especially models with two fitting parameters). The fact that we do not know the porosity of the firn means that the strengths calculated from the satellite data do not provide evidence for the model. Furthermore, the model itself does not provide any further evidence for the crevasses initiating in firn than figure 8c plus the statement that firn tensile strength should decrease with porosity. Overall, in my opinion, there is not really a need for this in the article so I would recommend taking these sections out. I think figure 8c is useful and interesting, and does the job at demonstrating that the crevasses are likely to initiate in firn. This might have the added benefit of improving the overall flow of the article.

Von Mises stress criterion:

The ellipses shown in figures 7 and 8 look great. It is evident that the vast majority of the crevassed locations on Thwaites sit inside the bounds of the yield region shown, and the vast majority of the un-crevassed locations sit at effective stresses lower than the inner yield curve. There are a couple of things I hope the authors can elaborate on.

Firstly, the ellipses show that reaching a stress threshold between 70 and 200 kPa is a necessary condition for fracturing. However, I wonder whether the authors could comment on whether it is a sufficient condition? The panels 8a and 8b have different scales on the colourbars (necessary because of the huge class imbalance between crevassed and un-crevassed locations), so it is not clear whether a measured surface stress between 70 and 200 kPa is a good indicator of whether there will be a crevasse. Perhaps the authors could show a map of where stresses in the Amundsen Sea Sector exceed 200 kPa. The figures at the moment do not rule out the possibility that the von Mises stress criterion is a good way of determining where crevasses are not likely to be, but might not be particularly skillful at determining where crevasses are likely to be (potentially because the strength of the ice varies too much due to differences in firn porosity?). If this turns out to be true, it is an interesting point of discussion.

It is also not clear exactly which area of Thwaites Glacier is being used to test the stress envelopes. Is it the entire crevassed area on Thwaites or the small section at the upstream edge of the crevasse field? It would be great to see this analysis extended to the rest of the Amundsen Sea Sector (we are told other glaciers show similar results). If the concern is that the porosity over the area has to be relatively similar to get a well-defined stress envelope, then the area could be broken up into smaller sub-regions. Given the potential here it seems a shame to limit the conclusions to a particular glacier rather than saying something more general about the failure behaviour of ice.

Specific comments

1. I would use the word “ellispe” or “elliptic” throughout the article when describing the von Mises stress envelopes, rather than “ovoid/ovaloid” unless that’s standard.
2. Figure 1. I think the colours for panels b and c might be labelled the wrong way round. If not, it would be better if they were consistent with figure 6.
3. Line 90: It is not clear to me why the use of Sentinel-1B images would result in geometric inconsistency.
4. Line 104: Is there any scaling applied when the images are converted to 8-bit integers, for example by something like requiring the mean backscatter to correspond to a value of 100. Otherwise, do the values concentrate at 0?
5. Line 127: I would write “Adam” optimisation rather than “Adams” optimisation.
6. Line 137: This part about how the images were combined - including the statement about the translational equivariance - is a little unclear to me. Is it the case that the padding is applied to an image patch prior to its processing by the neural network, and that the processed image could be in a different location?
7. Line 237: “interpretted” should be “interpreted’

8. Line 244: Figure 4b seems to be referenced both as evidence for the visibility and invisibility of crevasse features in optical imagery. Perhaps this should be 4c?

9. Line 276: Could you provide a citation for the 500 ma^{-1} increase in speed deep into the interior of the Pine Island Glacier betwen 2017 and 2022?

10. Figure 6 is very nice, but it is not really discussed in the main text. Perhaps some details specific to the figure should be discussed in section 3.2.

11. I think Figures 6-8 could be restructured to make them more compelling. For example, it would be great to see a before and after image for Pine Island, Thwaites and Pope/Smith/Kholer along the lines of figure 6 to accompany the timeseries of crevasse area change in Figure 7. It is a bit of a pain to go back to figure 1 to see the path/frame numbers when looking at figure 7b/c, so would it be possible to reproduce the acquisition footprints here as well? I think it would be better to either make 7a its own figure or combine it with figure 8 to improve the focus of this part of the article. As it stands, these sections seem to jump about a bit and lack focus to some extent.

12. Figure 7a: Could the authors comment on why the density of pixels seems to change from year to year at the same location? For example, the densities seems uniformly lower on Thwaites in 2022 than 2021. The corresponding crevasse area timeseries in 7b show that the total number of datapoints should be roughly the same so the datapoints are more widely distributed? What causes this?

13. Figure 7a: There are some faint dashed lines in the figure which, in the top row at least, indicate the coordinate axes $\sigma_{1,2}$. These seem to change position in years 2017-2020. Given the statement on line 290 I guess this is a mistake?

14. Line 292: Is this calculated as an average over the whole timeseries of crevasse maps and velocity fields or does it correspond to a specific date? If the latter, why was it chosen?

15. Line 294 and Figure 8: Can the authors elaborate on how the dashed curves were calculated as these form the basis for the observed strengths. For example, are they the closest ellipses to a contour on to the density plots (10% of the maximum density defined over some area could be an option)? There is also some inconsistency between how the bounds are reported in the article - sometimes as $75 - 210$ kPa and other times as $70 - 200$ kPa.

16. Figure 9: Could you include in the caption that the red lines are the same as those in the previous figure?

17. Section 4.4.2: This section is not quite right. The maps of fractures in the studies referenced deal in large part with the surface expressions of ice shelf rifts and basal crevasses, not with the kinds of crevasses considered in this article. It is true that the surface crevasses seen here, when advected onto the ice shelves do not change a great deal but this has little relevance to the other studies referenced here, or to the work of Gerli et al., 2023. I would recommend removing this section and adding a sentence to the end of the first paragraph at the start of section 4.4 explaining that your results show crevasses of this type have no relevance to ice shelf weakening.

18. Line 480: “surface strain” should be “surface strain rate”.

References:

Jelitto, H. and Schneider, G.A., 2018. A geometric model for the fracture toughness of porous materials. Acta materialia, 151, pp.443-453. https://doi.org/10.1016/j.actamat.2018.03.018