Responses to Reviewers' Comments for Manuscript https://doi.org/10.5194/egusphere-2023-1957

# Failure strength of glacier ice inferred from Greenland crevasses

Addressed Comments for Publication to

EGUSphere (The Cryosphere)

by

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**Response to RC1**

**General Comments.** Grinsted et al. use ice-sheet-wide geophysical datasets to assess the applicability of a previously untested failure criterion, the Schmidt-Ishlinsky (hereafter S-I) criterion, to predict ice failure across the Greenland Ice Sheet. This is done with the intention of being able to develop accurate predictors of crevasse formation and presence in large-scale ice-flow and fracture models. The paper is timely and is a clear evolution of the ongoing large-scale work being done on ice failure in recent years. It is a bit of an "open secret" that von Mises (hereafter vM) is a poor predictor of crevasse failure and it is good to see work being done to find better approaches, especially involving large-scale datasets. I have some thoughts regarding the failure criterion chosen, and in particular the lack of wider assessment given the prime opportunity provided by the datasets collated.

**Response:** Thank you for your feedback.

From our perspective we would not say that we assess the applicability of the Schmidt-Ishlinsky criterion, as it is not a criterion we propose a priori. Rather we make an empirical study of the failure criterion, and then find that it happens to be well modelled by the S-I criterion.

We hope that we address your detailed comments in our replies below.

**Comment 1**

As written, it is unclear what motivation or hypothesis led the authors to propose and test the S-I failure criterion. After the abstract, the criterion is not mentioned again until the discussion (L136), where it is defined but without any clear motivation as to why. At this point, it is argued that although both vM and S-I perform quantitatively identically (L144), if measurement noise was better S-I would likely be the better performer (L144-146). It is not possible for the reader to assess whether S-I performs better or worse than any other alternative criteria common in the literature, as this data is 'not shown' (L147-148).

**Response:** The motivation for the S-I criterion is purely empirical - It is *the only* simple failure criteria that fits the data (simple in the sense that it has no shape parameters). We realize that this may not have been clear as S-I is introduced in the methods before any results are shown.

Here's how we arrived at that conclusion. We calculate an empirical failure envelope by calculating the moving 5°median radial distance in the $\pi$-plane. This is shown as a fat solid black line in figure 4. It has a distinct hexagonal shape that we can compare that against a library of failure envelopes (see fig3.6 and fig3.7 in Kolupaev, 2018). We judged that this was so distinctly Schmidt-Ishlinsky type that we not need to quantitatively assess the misfit of different criteria. However, we have since done so, and find that the root mean square log misfits of the S-I, vM, and Tresca envelopes are 0.02, 0.04 and 0.08 respectively. Hence, we can now also conclude that the S-I criterion objectively provides the best fits to the data.

In our revisions we intend to make the following changes:

- Make it 100% clear how we arrived at the S-I criterion.
- Expand on the description of how we calculated the empirical failure envelope.
- Score the different failure criteria in terms of a RMS misfit between the proposed failure envelopes and the empirical failure envelope.
- Include the misfit score of additional (worse) failure criteria in the literature such as the Tresca/Mohr-Coulomb.

We will probably not plot additional failure criteria in figure 4, as it is already hard to distinguish the three lines.

**Comment 2**

The S-I is not a criterion I have encountered before in the glaciological literature, and appears to be pretty niche outside the discipline as well - indeed, googling the phrase 'Schmidt-Ishlinsky failure criterion' includes this preprint among the top results. As a result, I think it is important that the authors do more to contextualise and explain the criterion, and their motivation for choosing it. This is especially true as the authors disregard even comparing the criterion to other options, as previously noted (L147-148). Given the clear effort that has been put into producing and collating the various ice-sheet-wide datasets, it would be very interesting to see a comparison/EDA of all previously suggested criteria, and make it more convincing that the S-I criterion is (qualitatively) a better option.

**Response:** The motivation for the criterion is entirely empirical. It simply fits the data best (see our response to comment 1).

The difference between the vM criterion and S-I is relatively subtle considering the imperfections of real world data. So, it is not that surprising to us that the S-I like behaviour of glacier ice has not been spotted before. It may seem like an exotic criterion, but consider that nearly the entire ice flow literature uses Glen's flow law even though lab experiments indicate that the flow law for isotropic ice really should depend on the third invariant of the deviatoric stress tensor (Morland, 2007, see e.g.). Sometimes tradition and convenience is a good explanation for why a particular simple model is used.

We acknowledge that additional work is needed to understand why ice has Schmidt-Ishlinsky like behaviour. But for context note that other polycrystalline materials, such as mild steel, copper, nickel alloy, titanium, stainless steel, have been found to have Schmidt-Ishlinsky like behaviour (e.g. Kolupaev, 2018, table 2.1).

We intend to make the following changes to the manuscript to address this comment:

- We will compare to additional criteria proposed in the literature. Specifically Tresca/Mohr-Coulomb. We can only compare to pressure independent criteria, as our method only can derive the deviatoric stress tensor. We intend to do this in the text using RMS misfit scores (see our response to comment 1).
- We will list other polycrystalline materials have been found to have Schmidt-Ishlinsky behaviour for context.

**Comment 3**

In the absence of this comparison, a deeper theoretical concern I have is that the S-I criterion doesn't appear to improve upon a key weakness of the vM criterion, which is that it is relatively insensitive to the direction of stress - indeed, it is noted by the authors that the difference between compressive and tensile ice strength is an order of magnitude, and that this isn't consistent with the vM criterion (L38-40). As the authors note, the S-I criterion does not significantly deviate strongly from this assumption (L154). Although they highlight that the S-I criterion implies ice is 15% stronger in shear relative to the tensile stress (L142), as far as I can tell from Fig. 4 the S-I criterion implies that ice is *exactly as strong* in tension and compression. However, examining Fig 4 appears to show far fewer crevasses in the compressive sections of the figure - especially if the color scale is log, which these plots often are. Therefore, although I am excited by the datasets and study design presented by the authors, the paper (at least, in its current form) leaves me questioning that crevasse failure can be adequately described by any prescribed radius in the $\pi$-plane.

**Response:** Thank you for the comment.

Yes – the S-I criterion is by definition equally strong in tensile vs compressive regimes. However, this is supported by the data. We calculated the empirical failure envelope (solid black line in fig4), and we can therefore determine the failure strength of the tensile vs compressive directions directly from the data. This shows that the tensile failure strength is only 4% greater than the compressive failure strength (see figure 4). So, while this may be surprising, the data show that ice is nearly equally strong for tensile vs compressive deformation. This is part of the reason why the S-I criterion is a really good fit. We would therefore also disagree that this is a great weakness of the vM criterion.

Please note: The frequency of crevasses under different modes of deformation cannot be directly inform on the relative strengths for each mode. For example, we see most crevasses in shear zones. That is probably simply because the greatest surface stresses produced by ice flow tend to occur in shear zones. The high frequency of shear crevasses **does not** mean that ice is weaker for shear (indeed we find the opposite to be the case). Similarly, we caution against interpreting the *"far fewer crevasses in the compressive sections"* in terms of a failure strength.

Finally, we do not understand the comment *"leaves me questioning that crevasse failure can be adequately described by any prescribed radius in the $\pi$-plane."* The $\pi$-plane is just a pressure independent visualization method. **All** pressure independent failure criteria can be represented as a radius in the $\pi$-plane. The sum of the three principal deviatoric stresses is always zero, and so we effectively only have 2 degrees of freedom. The conclusions does not depend on the particular 2d projection we used. They would be the same if we had used a Vaughan (1993)-style projection.

We intend to make the following changes during revision:

- We will add a discussion of the frequency distribution for different modes.
- We will highlight the 4% empirical difference between Tensile and Compressive directions.
- We will specify in the figure caption that the color scale is linear.

**Minor comments**

**Comment 4**

L95/Fig 1 - It is not mentioned in the methods exactly how the high-elevation exclusion zone is exactly derived. Manually determined? If so, the mask could be included as supplementary data.

**Response:** It is a manually traced polygon. We initially used a mask based on whether the elevation was above a sloped plane, but deemed it was just as arbitrary and required an overly elaborate explanation.

We intend to include the mask file as a data-supplement or archive the data in data repository.

**Comment 5**

L46 - Chudley et al. (2021) also use this data to assess crevasse formation, which is probably worth including/contrasting/comparing in the discussion.

**Response:** Thank you for the comment.
Agreed.

We intend to add a comparison to Chudley et al. (2021) failure envelopes.

**Comment 6**

L80/Fig 4 - Although I understand that plotting on the $pi$-plane is a key point of this paper, I imagine most will be more familiar with plotting on a simple $\tau_1/\tau_2$ plot following Vaughan (1993). I highly suggest including this alternative visualisation in the supplementary material to aid the interested reader in comparing and contrasting, as well as in understanding how this visualisation differs from Vaughan's approach.

**Response:** Thank you for the comment.

There are many ways to visualize the failure stress state (Kolupaev, 2018, ch3). While the style used by Vaughan (1993) is indeed widely used, the $\pi$-plane visualization is common in the broader material science community. We prefer following the material science community in this regard, as the $\pi$-plane plot is independent of pressure (i.e. plots for $\sigma$ or $\tau$ gives the same result) and takes all stress components into consideration. The style used by Vaughan (1993) is a particular 2D-slice through the 3D stress failure space for $\sigma_3 = 0$ (Kolupaev, 2018, ch3.4), which makes the stress-state assumptions made not as clear as they could be. Several glaciological studies simply plot $\tau_1$ vs $\tau_2$ calculated from horizontal velocities, which we believe to be wrong or misleading; this suggests $\tau_3 = 0$, which is only true if there is zero horizontal divergence. If $\tau_3 = 0$ is not fulfilled, then we should no longer expect the points to fall on e.g. a vM ellipse, even if the points are generated by vM failure.

We will consider adding a second panel to figure 4 with $\sigma_1$ vs $\sigma_2$ given $\sigma_3 = 0$ projection. I.e. plotting $\tau_1 - \tau_3$ against $\tau_2 - \tau_3$.

**Comment 7**

Fig 3 - Some indicator of y axis scale might be nice (unless normalized?)

**Response:** Thank you for the comment.

These are basically histograms normalized to have peak height 1.

We intend to describe this in the caption.

**Comment 8**

Fig 4 - color scale needed for quantities.

**Response:** Thank you for the comment.

This is a 2D hexbin histogram. The gray color scale is linear in the counts per bin.

We intend to describe the linear color scale in the caption.

### Comment 9

L114-115 - Observational evidence of this can be found in recent papers (Harrington et al. 2017, doi:10.3189/2015AoG70A945; Hubbard et al. 2021, doi:10.1029/2020AV000291). I agree that it is likely that modelled MAT represents a lower bound of likely temperatures. Though for practical purposes, I don't have a better suggestion of how this can be approached.

**Response:** This is an important caveat as discussed in the manuscript. We also did not have a practical way of adjusting the temperature that we were happy with, and so we opted for the simplest solution where we use unadjusted CARRA along with a simple sensitivity calculation.

In our revisions we will check if we can strengthen our discussion of this caveat using the references provided by the reviewer.

### Comment 10

L123-128 - This is absolutely fascinating. How could the seasonally varying regions be better represented? Is it a case of crevasse initiation being initiated at the maximum velocity/stress? Or limited by the minimum velocity/stress?

**Response:** Our interpretation is that crevasses are initiated at the maximum velocity/stress.

It would be great if we could drop our constraint that limits our study to regions with steady ice flow. Ideally we would like high quality concurrent snapshot observations of crevassing and strain rates. Then we could directly relate the formation of new crevasses to changes in ice flow. However, such remote sensing products do not exist yet. Ice sheet wide "snapshot" velocity products, such as those provided by PROMICE, unfortunately have quite high levels of noise which in turn result in very noisy strain rates. This is why we had to use long-term average velocities for our analysis. We speculate that rather than post processing velocities, then it may be possible to make a new low-noise remotely sensed strain rate product using InSAR techniques (Andersen et al., 2020, see e.g.). Further, there is also no off-the-shelf product that reliably detects new crevasses over time for the entire ice sheet. But the rate of improvement in remote sensing products has been amazing, so we look forward to what the future will bring.

We will verify that this is discussed in the manuscript. We believe it to be, but we may elaborate on this point.

### Comment 11

L129-132 - Or limited by resolution/ability of crevasse dataset? The crevasse dataset is taken from another source and no limitations are discussed in the paper.

**Response:** We acknowledge that this might be an alternative explanation in some regions.

We intend to make the following revisions to address this comment:

- In these sentences: Add that another explanation might be limitations of crevasse data set.
- Describe the data limitations in the data section too.

**Comment 12**

L136 - does the data have a hexagonal pattern, or is the data clustered around the shear components and the hexagonal plotting style gives the impression that this is the case?

**Response:** The empirical failure envelope has a hexagonal pattern (Solid black line in fig. 4; see our response to review comment 1). This is not a feature of the plotting style.

We have outlined the changes we intend to make in order to address this point in our response to review comment 1.

## References

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