Responses to second set of reviewer comments for the article "Using observations of surface fracture to address ill-posed ice softness estimation over Pine Island Glacier"

We would like to thank the reviewers and editor once again for their very fair and thoughtful assessments of the article, and for their patience in waiting for us to address their comments. The reviews comments are reproduced here and my responses are coloured in teal.

Having previously made the case that it might not be hugely valuable to include any synthetic experiments in the manuscript, we have now decided to include a section that uses a synthetic ice stream to justify the basic premise of the method (in the snapshot case) and set up some expectations for what the method might do (Section 3: A synthetic example). This sits between the methods and main results sections. This should provide some additional insight to the reader about the theoretical efficacy of the method, but stops short of making claims we would struggle to fairly translate to the real-world setting. We think that this addresses the main remaining concerns of the reviewers (and corresponds to the first option for doing so suggested by the editor in their latest report).

Responses to comments from Reviewer #1

Reviewer 1: Thanks to the authors for the effort taken to thoroughly and thoughtfully address the comments made by myself and the other reviewer. The scientific content is as good as before, and the manuscript now appears better organised and easier to follow. I think it is very interesting work and entirely appropriate to The Cryosphere. I am satisfied with the changes and happy to recommend publication.

As picked up on by the other reviewer and the editor, the results in this paper are more qualitative than quantitative, but in this case I don't think that's a weakness. I am of the opinion that outputs from inversion processes are often better judged by looking at the distribution of output fields (such as those shown in Fig. 5) than by quantitative measures (as long as the misfit is reasonable, which it seems to be here). The authors are careful to point out the limitations of the methodology and do not overstate their findings, giving a realistic view of when the method would be most usefully applied.

The addition of the 'Next Steps' section in the discussion to explicitly explain the authors' reasoning should aid readers in understanding decisions made in devising this work, and opens the way for other interested modellers to explore the idea further with synthetic experiments. However, it does also emphasise a remaining small complaint I have with the manuscript. The authors refer a few times to what they would 'expect' to see (e.g. lines 118, 352, 385) but without explicitly stating why they would expect that. The manuscript is fine as it is, but these points would be stronger if the reasoning behind these expectations were made clear, and I think the authors should consider this.

Thank you for the very thoughtful comment. I have added a note accompanying the use of 'expect' in line 118 that lets readers know that this is due to the coincident changes in dynamics and structural integrity on the glacier. As for lines 352 and 385 (and less explicitly elsewhere) hopefully the new section "A synthetic example" goes some way to informing the reader of our expectations.

I will make one further comment, although this is not something which would necessarily need changing. It's down to personal preference and I mention it for the authors to consider at their discretion. When presenting misfit as in Fig. 2, I find it more useful to either present the misfit as a proportion of the measurements, or to see the measurements plotted alongside. In this case I know that the speed of Pine Island means that maximum misfit on the order of 200-300 m/a is not unreasonable, but others may not be so familiar with the glacier.

This is a very good point. I have added a note to the end of section 4.1 describing the speeds we see on Pine Island Glacier which should help put these misfits in context.

Below I list some minor typographical errors I noticed while reading through, which the authors may wish to correct:

- Line 42 – "constitutive"

- Line 110 – “functional”
- Line 240 – “snapshot”
- Fig 2 & Fig 3 – Here you use the American “regularization”, while you use the British ‘regularisation” in the text
- Line 287 – “from” is repeated
- Line 348 – “principal”

Thank you for your keen eye! I have corrected these.

Responses to comments from Reviewer #2

Reviewer 2: Thanks to the authors for addressing and replying to all of my earlier comments. The changes have greatly improved the clarity of the manuscript.

My only remaining comment has to do with the takeaways of the paper. I mentioned in my initial review that I found it challenging to draw any concrete conclusions about the results presented given that there is no known "right answer" to the problem, and therefore no metrics for determining whether the inclusion of fractures as prior information improved the results of the inversion.

This concern still stands to some degree. I acknowledge the point that there are many assumptions that would have to be imposed in order to construct any synthetic experiments that would quantitatively address the question (e.g. the effect of fractures on ice softness). That being said, I do think there are relatively well-known approaches to these that the authors could take. For example, the effect of fractures on ice softness is parameterized in largely the same way in every continuum damage mechanics study (see, for example, Chris Borstad's studies), and there would be a reasonably strong justification for following these approaches. The imposition of a crevasse field and choices about other processes affecting ice softness would need to be assumed as well but I do think these are justifiable choices that can also be evaluated in a systematic way (e.g. imposing many different kinds of crevasse fields, doing multiple experiments with different processes affecting ice softness). I believe such a study would be the only way of conclusively answering the question posed in the paper of "whether the introduction of genuine prior information into the inverse problem results in solutions that are more appealing than those found in other, heuristic regularization methods" (lines 92-94), similar to the study by Gudmundsson and Raymond (2008) to evaluate the use of such methods on uncovering basal topography and basal friction.

This being said, if the authors choose not to include something like this (which is ultimately up to the authors' discretion), I would recommend a reframing of the present study. As illustrated by the lines I cited above, the study is currently motivated by this question of determining whether including fractures as prior information can improve the inverse method. I'm not convinced that the methods the current manuscript uses actually answers this question (and, as a side note, I'm not sure I understand what "appealing" means in this context). I would perhaps recommend a slight reframing of the scientific question at hand to be less focused on testing the use of prior information and more focused on, as an example, evaluating different ice softness fields that can be recovered in Pine Island Glacier and what these different softness fields mean for the use of inverse methods to estimate ice softness. Of course, the exact framing I leave up to the authors, but I think it's important to make sure that the question posed is answered in the manuscript. I also think including the discussion of why the authors chose this methodology (e.g. Section 4.4: Next steps) should be included far earlier in the manuscript, if the authors choose not to include synthetic experiments, to illuminate for the reader why the methodology was chosen near where the methodology is described.

Thank you very much for this considered and well-reasoned assessment of the work. The work of Gudmundsson and Raymond (2008) provides a promising template for an alternative format for this kind of study, though it would change the article substantially to go down that route and would be, I think, unnecessary. The deficiencies pointed out in the current approach are, to some degree, addressed with that kind of experimental set up, though it replaces them with the difficulties in translating the intuition to the real world, as we have discussed before. As such, I think there is merit in the approach we take here and we are justified in leaving alternatives to future work. The new section "A synthetic example" at least shows that the idea makes sense where we have strong priors on where the ice is likely to be damaged, and also provides additional context to the approach we take throughout the rest of the article. With this, I am of the opinion that we do answer the question we pose in the article, all be it, broadly qualitatively.

As one final note, it may be worth it to contend in a bit more detail with the Gerli et al. 2024 study. I know the authors reference this in line 394, but an added sentence or two to explain why their results deviate from the results of the Gerli study would be enlightening. I leave this up to the authors' discretion, however, since this wasn't mentioned in my original review.

I have added and additional sentence to the discussion noting that the contradiction between the two studies shouldn't be too surprising given the degeneracy in solutions. I am hesitant to provide too strong a critique of that work despite thinking its conclusions are slightly unhelpful. I hope the general points about the importance of priors and the dangers of over-interpreting the solutions to ill-posed problems come across in the article.

References

Gudmundsson, G. H. and Raymond, M.: On the limit to resolution and information on basal properties obtainable from surface data on ice streams, The Cryosphere, 2, 167–178, https://doi.org/10.5194/tc-2-167-2008, 2008.