Responses to reviewer and editor 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 editor and reviewers very much for the taking the time to read the article and for providing us with valuable and insightful feedback. (And also for their considerable patience in waiting for our responses.) All reviewer comments and responses are collated in this document, with each review reproduced in full and the editor's comments included at the bottom of the document. Responses to any general comments of the reviewers are coloured in teal, while responses to specific comments are tabulated afterwards.

A central theme of both reviews is that parts of the article should be restructured to make it easier to follow. To this end, I have made various structural changes, informed by specific comments, for example, moving text between the introduction and methods sections. I hope the reviewers find that these changes have improved the flow and clarity of the article. I have also added an additional figure (Fig. 3) that expands on the distribution of misfits for the three regularisation methods for the snapshot inverse problem. Finally, I have, without prompting from reviewers, removed the appendices after considering whether they were really necessary, though I am happy to add them back in.

Responses to comments from Reviewer #1

Reviewer 1: In this manuscript, the authors investigate the effect of assimilating more prior information into inversions for ice stiffness. The data informing the priors are strain rates, and locations of fracture derived from satellite imagery. Pine Island is chosen as a study area due to the large amount of fracturing observed there. Experiments are carried out using both snapshot and time-dependent inversion processes, using different regularisation. The results show that the use of this data in priors results in stiffness fields which better visually represent observed fracture patterns, without affecting the velocity misfit. The use of methods informed by fracture data could be important for improving inversions of floating ice, but is likely not have much impact on grounded areas. It is suggested that these methods would be best suited to diagnostic modelling and attempts to evolve stiffness fields through time.

This study will be valuable to a particular niche of ice flow modellers, and is certainly within the scope of The Cryosphere. I personally found it to be interesting, although I think wider interest will be limited as the focus is only on the inversion process and, by the authors' own admission, unlikely to be of much help to long-term predictive simulations.

My main issue with this manuscript is that it can be quite difficult to read, and is unclear at times. The introduction seems a little muddled, with some parts referencing specifics of this study among a more general review of the relevant issues. I would recommend moving anything specific to this study (sliding law, value of n in flow law etc.), and the more detailed discussion of reasoning behind the methods used found in the last paragraphs, into the methods section, so that it can all easily be found and doesn't over-complicate the introduction.

I also found the methods section difficult to follow in places. Section 2.1 would in my opinion benefit from being restructured. I also think the methods section should contain a clear summary of all the experiments which were carried out, as these are not all introduced until during the results section.

The scientific content of this manuscript is good, and worthy of publication, but I think work needs to be done to improve the clarity of its presentation. For this reason, I recommend publication after revisions.

Response: We would like to thank the reviewer for their positive comments about the article and their thorough and thoughtful review. I have restructured the introduction and methods sections (and the results to some degree) to make things clearer. Many of the changes made are in response to specific comments laid out in the review below.

Responses to specific comments from Reviewer #1

Reviewer 1		
ID	Reviewer Comment	Response
1	Line 27: Is this a typo, or is the approximately equal sign there for a reason? If not a typo, please explain what is meant and be clear what value for n is being used in your work.	This is not a typo, though I have made this less ambiguous by adding the sentence: "The value of the exponent n is dependent on the particular mechanisms by which creep occurs within the ice and various properties of the crystal grains (e.g. Haefeli (1961)), and takes a value between 1 and 4 in most cases. Here, we take the common reference value of $n = 3$".
2	Line 30: It may be helpful to write Eq.1 in a form which includes $\phi(x)$ for clarity.	I have changed this sentence to be: Here, we consider the approach in which these are prescribed *a priori* and a 'stiffness' field $\phi(x)$ is defined over the domain to account for unknown deviations in the expected ice rheology, such that eq. 1 becomes $\tau_{ij} = 2\phi\eta\dot{\varepsilon}_{ij}$
3	Line 43: I think the sliding law used in this study should be stated in the methods section rather than the introduction	Thank you, I have moved this line into the methods.
4	Lines 48-51: It's a little unclear at points in this introduction whether you are talking about the specific process(es) used in your study, or more generally. As a more general point, some inversion processes use u and v velocity components as two separate observed fields, and some can also make use of thickness changes dh/dt. This doesn't mean the problem is ever not ill-posed, but there is a greater variety in approaches that just using a single u field. If this statement is referring to the specific process used in this study, please make this clear.	I hope that the changes made to the introduction and methods have addressed the issue of clarity and distinguished statements that relate to methods in general and those we use ourselves in the article. I have also added a couple of sentences that make it clear that other data can be included in the inverse problem as suggested.
5	Lines 92-5: This detail probably belongs in the methods section	This has been moved into the methods.
6	Lines 97-102: As above, better to put the detail in methods.	This has also been moved into the methods

7	Lines 113-4: Could this point about the link between dynamics and fracturing over the rest of Antarctica be expanded on in the discussion?	I appreciate the desire to expand on this, but I think it might be difficult to do so without moving into speculation. PIG has shown a strong connection between fracturing and broader dynamics over the last decade, e.g. the cited studies showing links between calving, shear margin degradation and changes in ice speed; also the very low basal stresses found far inland of the grounding line. There is a feeling that this is not replicated in other places round Antarctica, though there is actually little concrete evidence of that. For example, when carrying out inverse problems, I have not seen very low basal stresses on grounded ice in many other places, but I haven't actually done or seen proper analysis on it. I think it might be better to keep this to a short statement reflecting a generally held belief than include too much pontification. I could be persuaded otherwise though.
8	Lines 121-2: You refer to this past paper a few times without detail. As it relates to an important source of data in this study, a brief description of the method would be useful in this section, or at least mentioning that it uses a machine learning technique to identify crevasses.	I have included a short paragraph with a little more detail about this dataset.
9	Lines 128-49: In my opinion, these paragraphs would benefit from a little restructuring. I think the definition of $\xi = \min\{\xi_{\mathrm{frac}}, \xi_{\mathrm{shear}}\}$ should be introduced first, defining what the components are, before then presenting the details of how the components are calculated. This would have made it easier for me to follow, although that may be a personal preference.	Thank you for the good suggestion, I have changed the structure as suggested.

10	Lines 161-3: Could you give a reason for the choice of initial guesses? After stating that this can have a large influence on the optimisation, I feel a justification of the choice is required. Why not, for example, use a uniform guess for C or a value of 0.5 for ϕ?	This is a very good point! The optimisation problem will be more easily solved if the initial guess is close to the optimal solution. If we think Glen's flow law with a choice of $n = 3$ is correct, the ice is unbroken, and the temperature field we get from the thermomechanical spin-up described in the text is a good approximation, then taking $\phi = 1$ is the right choice. Even if those assumptions seem loose, $\phi = 1$ is still a natural choice as another value would require justifying why you think there is bias in the viscosity and how large you think that bias is. Regarding the choice for C, you are asking the quite a lot of the inverse solver if you supply a uniform initial guess as the field can vary by orders of magnitude. The assumption is made that under the shallow-stream approximation, most of the stress-balance on grounded ice is accounted for by sliding and gravity. In that case, a C field that accounts for the grounded ice speed will be close to the C field when the full inverse problem is performed for both control parameters over grounded and floating ice. I have changed the wording of this section slightly to make these points in the article.
11	Lines 194-7 (also Lines 226-30, 241-245): I think a summary of all experiments should be included at the end of the methods section, before going into the results. This will help to show readers exactly what you're doing in the context of methodology you've described. Introducing the exact cases during the results section seems a bit late.	This is a good point. I have attempted to make this clearer in the methods section by including lists of simulations for both snapshot and time-dependent problems.
12	Lines 203-4: The subpanel letters do not match the figure. These should be d,e,f not e,f,g	Thank you very much, I've fixed this now.
13	Lines 283-5: This is worded quite vaguely. If a reference to the previous paper is required (I would argue it is not here), be clear about what suggestion is being referred to.	I have changed this to read: "This suggests that observations of surface fracture on grounded ice have limited use in reducing the degeneracy associated with mixing between C and ϕ fields" and removed the reference to a previous work by the authors.
14	Lines 334-6: The chosen value should also be labelled on Fig.5. In fact, it would be good to have the values labelled for each circle on the figure.	I have added a labels to each of the circles in figure 5 as suggested.

Responses to comments from Reviewer #2

Reviewer 2: This study investigates the use of surface fracture and strain rate data in constraining inversions for ice rheology. The study considers two applications – the "snapshot" inversion infers both ice viscosity and basal friction in a single timepoint and the "time-dependent" inversion infers viscosity on an ice shelf at many points in time. The study finds that the inclusion of this additional information into regularization terms can alter the estimates found by the inverse method and possibly allows for an improved physical representation of ice viscosity in the inversion. The addition of this new data appears to be most useful on floating ice.

The application of more data, particularly that of surface fracture, to constrain glaciological inversions is a potentially very useful contribution, as inverse methods are widely used to initialize models and investigate drivers of ice sheet change. The study itself is very applicable to The Cryosphere. Below I describe some comments about the work itself and the presentation.

Response: We would like to thank the reviewer for their compliments on the content of the article and insightful review.

The study focuses on the application of these new methods to a case study of Pine Island Glacier. This makes it challenging to draw a concrete conclusion about whether this new data does improve the inversion because we don't know what the "right answer" is. Without knowing what ice softness is in Pine Island Glacier, it's hard to know how to compare these different cases the authors present (no regularization, heuristic regularization, data-informed regularization) rather than to say that they are different in certain ways. It seems to hamper the ability for the authors to suggest that one way is "better" than the other. One way of evaluating this is comparing the misfits to see if one regularization technique improves the optimization; however, in evaluations of Figures 2-4, there doesn't appear to be enough of a significant difference in the misfits to suggest that the data-informed regularization can produce more physical insight than the heuristic regularization. The authors are very careful and measured in the way they speak about these comparisons, which I think is a strength of this manuscript – they do acknowledge cases where the inclusion of this data does not appear to contribute to the inversion (e.g. on grounded ice). However, I still struggle with what the takeaways should be if there is such a difficulty in comparing between these cases. Possibly a clearer approach might be to test this technique on a synthetic case that approximates the PIG case study, in which a synthetic fracture field is imposed and a relationship between that fracture field and viscosity is assumed. This would provide a more straightforward way to compare between the cases presented in the manuscript and enhance the takeaways for the reader.

Response: This is a very good point, and I understand the desire for more concrete conclusions, however I think that attempting to do so might end up being detrimental to the study. The ill-posedness of the problem means that we are not able to draw too many conclusions about the solution from the misfit. As the reviewer points out, an alternative is to set up a problem in which the solution is essentially known a priori, e.g. synthetic examples. I did consider this when developing the work, however, I could not think of a way of doing so that would allow us to generalise to real world data given the number of assumptions we would have to make (e.g., as the reviewer suggests, a relationship between crevasse field and softness). Given that the effect of fractures on the rheology is always unknown prior to carrying out the inverse problem, we are of the opinion that it is most appropriate to carry out these experiments on real data, and make-do with more qualitative statements about the success of the approach. It is my hope that there are important and interesting conclusions that people can draw from the article anyway. For example, the benefit of including fracture data in constraining damage/softness fields on floating ice is demonstrated convincingly, and the article provides a valuable demonstration that one can make use of additional data to change the solutions of the inverse problem.

I have added a little bit to the discussion explaining our approach (section 4.4 "Next steps"). I have also added another figure showing the distribution of misfits for the snapshot inverse problem for the three cases, which could be of interest.

(See also my response to the editor's comments, at the end of this document.)

Responses to specific comments from Reviewer #2

The description of the methods I found to be often hard to understand, in terms of the organization of the methods section and the wording of the explanations:

Reviewer 2		
ID	Reviewer Comment	Response
1	A bit more explanation for how fractures are identified and how those fractures are converted into a continuous field to produce f would be helpful here, especially for those that haven't read the previous papers that describe these methods.	I have added a paragraph to the methods describing briefly how crevasse data are generated. I have also added a note that the smoothing is done by convolving the maps with a Gaussian kernel.
2	Line 44: the relationship between softness and stiffness seems to imply that stiffness is bounded between 0 and 1 – is this the case, and if so, why does this need to be the case? Stiffness appears to be simply a multiplicative factors on viscosity, in which case I don't see why viscosity can't vary by orders of magnitude	(For reference, this refers to the relation $\varphi = (1-\phi)$ where φ represents softness and ϕ represents stiffness.) It is true that stiffness is bounded at the bottom by 0 (leading to an upper bound on softness of 1) which just prevents negative viscosities. In general, the stiffness is not bounded at the top by 1, meaning one can have negative softness. In our case, we do bound our stiffness at the top by 1, though it makes little difference to the solutions. It is right to say that the effective viscosities can vary by orders of magnitude, but this is almost all accounted for by Glen's flow law already. Where it is not, and we need the multiplicative scalar, important changes in viscosity are invariably on the side of reduced viscosity. We account for this with a stiffness that can reduce all the way to zero.
3	Lines 151-153 form the key description of the "snapshot" inversion and yet I found this to be challenging to understand. What is epsilon meant to represent physically? What is γ, physically? I also found it challenging to understand ξ and its relationship with ϕ. Having a clearer description of all these parameters would be very useful.	This is a good point, I have tried to make this clearer. Earlier on in the manuscript, I have included the line: "In essence, [ξ] should reflect our confidence in our initial guess for the ice rheology." I have then changed the line in question to read: "In the case of the snapshot inverse problem, the assumption we wish to encode in our prior for ϕ is that $\phi \sim \mathcal{N}(1, \gamma^2)$ where $\xi \to 1$, and γ is a small number related to the strength of the prior." which removes one of the parameters in the original sentence (ϵ) and states that γ is related to the strength of our prior - which goes on to be related to the regularisation parameter. Hopefully this is clearer and will be readable by most - maybe with the use of Appendix A if required.

4	The L-curve section seems to be most applicable in the methods section, as I found myself wondering while reading how the regularization parameters were chosen and whether there was an L-curve-style approach to finding them. For example, lines 165-166 mention that there is an independent search for the regularization parameters but without further information it is hard to understand what this means.	The inclusion of this section is not so much to explain the method, but to make a more general point about the use of L-curve analysis when carrying out inverse problems. Rather than moving the section, I have included a sentence in the methods section explaining that L-curve analysis is used at each juncture to tune regularisation parameters. Hopefully, that means the section on L-curves in the discussion makes slightly more sense in the context of the rest of the article.
5	The term "high" shear strain rates is used often but not defined until line 145. A definition earlier (when it is first referenced) would be useful.	I tried moving the definition further up, but it seemed a little out of place. Instead, I have included a parenthetical "defined below". I can change this if the reviewer still thinks it is required.
6	Lines 128-130 imply that ξ is a mask of only 0 and 1 values, but Figure 1 makes it seem like ξ is continuous.	Good point. I have changed this to state that the field "goes to 0/1" rather than "is 0/1" in different areas.
7	Some of the equations (especially the regularization equations, such as Equations 10 and 11) could use much more explanation to describe what the terms mean and to remind the reader what the parameters are (I had trouble, for example, remembering the distinction between f and ξ).	I have rewritten this section, including reducing the number of parameters one needs to keep track of and introducing ξ earlier. I hope the various equations are now easier to follow.
8	The paragraph in lines 131-139 state that there are some things to note in the fracture data that are useful to understand the stress balance of PIG but the paragraph doesn't explain what the implications to the stress balance are.	This is an excellent point. I have added two sentences, one about the area of grounded crevasses, and one about crevassing in the shear margins: "If this is indeed an area in which membrane stresses form a significant component of the stress balance, the presence of crevasses deeper than the firn layer could have implications for the dynamics of this region by changing the horizontal transmission of stress." and: "Viscous deformation in shear margins can account for a significant portion of the stress budget of an ice shelf, so changes to the large-scale rheology in such locations will influence the distribution of stress throughout the ice shelf."
9	Line 200 – "The phi fields in each case are substantively different…" – it took me a while to understand what the different "cases" were (it is clear upon looking at the figure but it may be helpful to state this in the text as well)	Hopefully this is clearer in the modified manuscript. The 'cases' are outlined in the methods section and at the beginning of the results section.
10	Line 202 – "of even slow-flowing ice streams" – I wasn't sure what the "ice streams" were referencing here.	Thank you, that was unclear. I have changed "ice streams" to "parts of the glacier".

11	Figures 2 and 4 – it would be helpful visually to add more labels to the colorbars rather than just the top and bottom labels. It could also be a useful diagnostic to visualize the misfit as a percentage of the observed velocity, to give some context to the absolute numbers.	Thank you for the comment. I have added more labels to the colourbars for all figures throughout the manuscript. Hopefully that makes things easier to interpret generally. I have not added the relative misfit as the important thing for these figures is the difference between the cases, rather than the misfits themselves. Adding an extra row to the figures makes them look a bit cluttered while not adding much.

Responses to comments from the editor

Your manuscript has received two constructive reviews. Both reviewers have acknowledged the scientific quality of your work. Thank you for addressing their comments thoughtfully and for incorporating improvements into the revised version of your manuscript. However, I have identified several concerns that I believe need to be addressed in your next revision:

1: Consistent with the feedback from Reviewer #2, my primary concern is "whether this new data does improve the inversion". I understand the challenge to draw conclusions with synthetic data. While I leave the ultimate decision in your hands, I strongly recommend that you carefully reconsider Reviewer #2's suggestions, particularly regarding the inclusion of additional experiments. If you choose not to conduct these experiments, I advise adding a detailed discussion section that explicitly explains your rationale. This should also outline what you believe would constitute reasonable next steps for anyone aiming to build on your work.

2: Quantification of results: As a manuscript focused on numerical modeling, I find it surprising that the results section contains minimal quantitative metrics, especially when comparing with observations. Specifically, I encourage you to include quantitative measures such as the min, max, and mean misfits to assess model performance more comprehensively. Such metrics would be particularly useful for interpreting the results presented in Figures 2 (d–f) and Figures 3 (d–f). Providing these objective metrics will not only help readers better evaluate model performance but may also offer opportunities to enhance the depth of your discussion.

Therefore, I now strongly encourage you to submit a revised version that addresses these remarks. Based on this revised manuscript, I will invite both reviewers to assess whether the improvements warrant acceptance for publication in The Cryosphere.

Response: Thank you very much again for considering the revised article and for your constructive comments. I hope that you find the additions to the article and responses below satisfactory.

1: The question of "whether this new data does improve the inversion" is a bit difficult to answer exactly. Ultimately, the best approach is the one that most often gives you the right answer. However, it is difficult to judge what the right answer is without knowing it a priori. There are problems for which a good misfit indicates a good solution so, by performing different experiments and analysing the misfits, one can understand the quality of the method. However, it is fundamental to the nature of ill-conditioned problems (such as this, and others that naturally arise in viscous flow problems) that this is not true. Hence, though we could analyse the misfit much more extensively in the article, we have no guarantees that it would tell us anything reliable about the quality of the solutions. (Having said that, I have added a bit more about the misfit, described below in my response to your second suggestion.)

As you point out, an alternative approach is to use synthetic experiments in which the solution *is* known a priori. An example we could have set up is to have determined a steady-state softness field by prescribing a crevasse-depth formula and interpreting the relative depth of crevasses as 'damage' (1-softness). Then used the location of the crevasses to construct our prior, then recover the solution and show it works better than other regularisation methods (and you could even probably state by how much the new approach is better). This is appealing in a number of ways, not least because it would have been more straightforward than using real data. However, due to the large number of assumptions we can't justify and whose effect we can't quantify, we judged that experiment like that would not tell us anything useful about the real-world case. We are still of the opinion that a better approach is to consider a real-life example, and look for features in the solutions we would expect to see. In so doing, we are sacrificing the ability to make quantitative statements for the assurance that we are at least measuring something sensible.

I have added a short section to the discussion (Section 4.4 "Next Steps") explaining this rationale. I think the section is redundant, but am happy to keep it if the reviewers agree that it improves the article.

There is also the question that your conception of an improved method for the inverse problem will probably depend on what you want the solution for. We discuss this in the article. It might be that, even if the softness field you recovered were a perfectly faithful representation of the bulk material properties of the ice at that moment in time, it might be less useful to you than one that is not. (E.g. if you want a generic softness field that isn't too wrong for many timesteps of a transient simulation).

Overall, this is probably not going to be the kind of article that someone can easily skim to find a metric telling them the best approach to employ in their model. But we (and, I think, the reviewers) are of the opinion that it is still of value and interest to the community - perhaps more to those who have a particular interest in inverse problems. I think that makes it worthy of publication in The Cryosphere.

2: Thank you for this suggestion, I agree that there could be more numbers in the article. (Though, related to the first point, it would be a shame if these quantitative statements about the misfit overshadowed the much more important qualitative discussion of the solutions.) I have added a figure to the article (Fig. 3) that shows the distributions of misfits on grounded and floating ice for the snapshot inverse problem (there is already a subfigure looking at the misfit for the time-dependent problem) and various statements about the mean misfits for different cases. I think this will be interesting to the reader and hopefully satisfies the requirement for including more numbers.

References

Haefeli, R.: Contribution to the Movement and the form of Ice Sheets in the Arctic and Antarctic, Journal of Glaciology, 3, 1133–1151, https://doi.org/10.3189/S0022143000017548, 1961.