Review of "Using observations of surface fracture to address ill-posed ice softness estimation over Pine Island Glacier" by Surawy-Stepney et al.

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.

Specific Comments

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.

Line 30: It may be helpful to write Eq.1 in a form which includes $\phi(x)$ for clarity.

Line 43: I think the sliding law used in this study should be stated in the methods section rather than the introduction.

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 (d*h*/d*t*). 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.

Lines 92-5: This detail probably belongs in the methods section.

Lines 97-102: As above, better to put the detail in methods.

Lines 113-4: Could this point about the link between dynamics and fracturing over the rest of Antarctica be expanded on in the discussion?

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.

Lines 128-49: In my opinion, these paragraphs would benefit from a little restructuring. I think the definition of $\xi = \min\{\xi_{frac}, \xi_{shear}\}$ 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.

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?

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.

Lines 203-4: The subpanel letters do not match the figure. These should be d,e,f not e,f,g.

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.

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.